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The American Political Science Review

Vol. 72 June, 1978 No. 2

TABLE OF CONTENTS

The Political Implications of the Public Choice-Property Rights School 399
Norman Furniss

Institutional Solutions to the N-Prisoners' Dilemma 411
John M. Orbell and L. A. Wilson II

Mentoring in Political Life: The Case of Willy Brandt 422
Barbara Kellerman

Personality Effects on American Foreign Policy, 1898–1968:
A Test of Interpersonal Generalization Theory 434
Lloyd S. Etheredge

Military Advancement: The Visibility Theory of Promotion 452
David W. Moore and B. Thomas Trout

The Effects of Campaign Spending in Congressional Elections 469
Gary C. Jacobson

Uncertainty and the Formal Theory of Political Campaigns 492
John A. Ferejohn and Roger G. Noll

Explaining Presidential Popularity 506
Samuel Kernell

The Decline of Class Voting in Britain: Problems of Analysis and Interpretation 523
Mark N. Franklin and Anthony Mughan

Collective vs. Dyadic Representation in Congress 535
Robert Weissberg

Policy Making in Congressional Committees: The Impact of "Environmental" Factors 548
David E. Price

Committee Decisions under Majority Rule: An Experimental Study 575
Morris P. Fiorina and Charles R. Plott

The Competitive Solution for N-Person Games without Transferable Utility,
with an Application to Committee Games 599
Richard D. McKelvey, Peter C. Ordeshook, and Mark D. Winer

Reassessing the Mediating Impact of Legislative Capability 616
Lance T. LeLoup

Communications 622

Editorial Note 631

Book Reviews 632

Office of Publication: Curtis Reed Plaza, Menasha, Wisconsin.
Second class postage paid at Washington, D.C., and at additional mailing offices.
Printed in the United States of America by George Banta Company, Inc., Menasha, Wisconsin.
Composition by TypoGraphics, Columbia, Maryland.
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Book Reviews

Political Theory and Methodology 632

Frank Barnaby and Ronald Huisken, *Arms Uncontrolled.* Alan Dowty. p. 632.
R. N. Berki, *Socialism.* Mark E. Kann. p. 633.
Baruch Brody, *Abortion and the Sanctity of Human Life: A Philosophical View.* John T. Noonan, Jr. p. 634.
E. S. de Beer, ed., *The Correspondence of John Locke.* Robert H. Horwitz. p. 635.
Thomas R. Dye. *Policy Analysis: What Governments Do, Why They Do It, and What Difference It Makes.* J. Oliver Williams. p. 635.
Hans Enzensberger, *Politics and Crime.* Selected and translated in part by Michael Roloff. Dennis D. Dorin. p. 636.
Duncan Forbes, *Hume's Philosophical Politics.* J. G. A. Pocock. p. 638.
William A. Galston, *Kant and the Problem of History.* Thomas G. West. p. 639.
John H. Hallowell, ed., *Prospects for Constitutional Democracy: Essays in Honor of R. Taylor Cole.* Alfred Diamant. p. 640.
Joseph Hamburger, *Macaulay and the Whig Tradition.* Nannerl O. Keohane. p. 641.
John Hoffman, *Marxism and the Theory of Praxis: A Critique of Some New Versions of Old Fallacies.* Edward Andrew. p. 642.
Mark Hulliung, *Montesquieu and the Old Regime;* and Melvin Richter, *The Political Theory of Montesquieu.* W. B. Allen. p. 644.
Arthur Lehning, ed., *Michael Bakunin: Selected Writings;* Sam Dolgoff, ed., *Bakunin on Anarchy;* Michael Bakunin, *God and the State;* Emma Goldman, *Living My Life;* Emma Goldman, *Anarchism and Other Essays;* and Peter Kropotkin, *Fields, Factories and Workshops Tomorrow.* Edited by Colin Ward. William H. Harbold. p. 646.
Duncan MacRae, Jr., *The Social Function of Social Science.* Terence Ball. p. 649.
David Miller, *Social Justice.* Benjamin R. Barber. p. 650.
Peter H. Nidditch, ed., *An Essay Concerning Human Understanding.* By John Locke. Robert H. Horwitz. p. 651.
Michael Oakeshott, *Hobbes on Civil Association.* Thomas A. Spragens, Jr. p. 652.
Longin Pastusiak, *Komputery a polityka.* Roger E. Kanet. p. 653.
Gordon J. Schochet, *Patriarchalism in Political Thought: The Authoritarian Family and Political Speculation and Attitudes Especially in Seventeenth-Century England.* John Sanderson. p. 654.
Martin Seliger, *Ideology and Politics.* William A. Mullins. p. 655.
Hugh Stretton, *Capitalism, Socialism and the Environment.* Ronald Tiersky. p. 657.
Tracy B. Strong, *Friedrich Nietzsche and the Politics of Transfiguration.* Mark Blitz. p. 658.
Dennis F. Thompson, *John Stuart Mill and Representative Government.* George W. Carey. p. 659.
Paul Wilkinson, *Political Terrorism.* R. L. Nichols. p. 660.
Oran R. Young, *Bargaining: Formal Theories of Negotiation.* Robert Lyle Butterworth. p. 661.

American Politics 662

Henry J. Aaron, *Who Pays the Property Tax? A New View.* Clara Penniman. p. 662.
Paul R. Abramson, *Generational Change in American Politics.* Kristi Andersen. p. 663.
Philip Bean, *The Social Control of Drugs;* and Richard L. Rachin and Eugene H. Czajkoski, eds., *Drug Abuse Control.* Mark Moore. p. 664.
W. Lance Bennett, *The Political Mind and the Political Environment: An Investigation of Public Opinion and Political Consciousness.* Philip Converse. p. 666.
Monroe Lee Billington, *The Political South in the Twentieth Century;* and George Brown Tindall, *The Persistent Tradition in New South Politics.* Alexander Heard. p. 668.
Lynton K. Caldwell, Lynton R. Hayes, and Isabel M. MacWhirter, *Citizens and the Environment: Case Studies in Popular Action;* and Richard A. Liroff, *A National Policy for the Environment: NEPA and Its Aftermath.* Lettie McSpadden Wenner. p. 669.
John J. Corson and George A. Steiner, *Measuring Business's Social Performance: The Corporate Social Audit;* and James W. McKie, ed., *Predicament: Studies in the Regulation of Economic Activity.* Robert Chatov. p. 671.
Michael N. Danielson, *The Politics of Exclusion.* Oliver P. Williams. p. 672.
Thomas R. Dye, *Who's Running America? Institutional Leadership in the United States.* Alan Stone. p. 673.
Ward E. Y. Elliott, *The Rise of Guardian Democracy: The Supreme Court's Role in Voting Rights Disputes, 1845–1969.* Loren P. Beth. p. 674.
Roger Freeman, *The Growth of American Government: A Morphology of the Welfare State.* Lloyd M. Wells. p. 676.
Bernard J. Frieden and Marshall Kaplan, *The Politics of Neglect: Urban Aid From Model Cities to Revenue Sharing.* Dorothy Buckton James. p. 677.

Michael Harrington, *The Twilight of Capitalism;* Robert L. Heilbroner, *Business Civilization in Decline;* and Daniel Bell, *The Cultural Contradictions of Capitalism.* Andrew Hacker. p. 678.
James Hottois and Neal A. Milner, *The Sex Education Controversy: A Study of Politics, Education, and Morality.* Patricia Bayer Richard. p. 680.
John C. Hoy and Melvin H. Bernstein, eds., *The Effective President.* Philippa Strum. p. 681.
Herbert Kaufman, *Are Government Organizations Immortal?* Dean E. Mann. p. 682.
Lawrence J. Korb, *The Joint Chiefs of Staff: The First Twenty-five Years.* Paul R. Schratz. p. 683.
Harry Kranz, *The Participatory Bureaucracy: Women and Minorities in a More Representative Public Service.* Norton E. Long. p. 684.
Curt Lamb, *Political Power in Poor Neighborhoods.* Margaret Levi. p. 685.
Gilbert Levin, Edward B. Roberts, and Gary B. Hirsch, *The Persistent Poppy: A Computer-Aided Search for Heroin Policy.* Jack H. Nagel. p. 687.
Sar A. Levitan and Robert Taggart, *The Promise of Greatness: The Social Programs of the Last Decade and Their Major Achievements.* Dale Rogers Marshall. p. 688.
Richard Loss, ed., *Presidential Power and the Constitution: Essays by Edward S. Corwin.* Myron Q. Hale. p. 689.
Dan W. Lufkin, *Many Sovereign States: A Case for Strengthening State Government–An Insider's Account.* Ira Sharkansky. p. 690.
Thomas D. Lynch, *Policy Analysis in Public Policymaking.* John N. Collins. p. 691.
Duncan J. MacLeod, *Slavery, Race and the American Revolution.* Robert McColley. p. 692.
David R. Mayhew, *Congress: The Electoral Connection.* Lawrence C. Dodd. p. 693.
Arnold J. Meltsner, *Policy Analysts in the Bureaucracy.* Mark V. Nadel. p. 695.
Thomas R. Morris, *The Virginia Supreme Court: An Institutional and Political Analysis.* Chester W. Bain. p. 696.
Stuart S. Nagel, *Improving the Legal Process.* Larry L. Berg. p. 696.
Gary Orfield, *Congressional Power: Congress and Social Change.* Garrison Nelson. p. 698.
Diane B. Paul, *The Politics of the Property Tax.* Charles E. McLure, Jr. p. 699.
Paul E. Peterson, *School Politics Chicago Style.* Frederick M. Wirt. p. 700.
Ronald Radosh, *Prophets on the Right: Profiles of Conservative Critics of American Globalism.* Richard A. Melanson. p. 701.
Milton Rakove, *Don't Make No Waves . . . Don't Back No Losers: An Insider's Analysis of the Daley Machine.* James D. Fratto. p. 702.
Clinton Rossiter, *The Supreme Court and the Commander in Chief.* Francis D. Wormuth. p. 703.
Allan P. Sindler, *Unchosen Presidents: The Vice President and Other Frustrations of Presidential Succession.* Landis Jones. p. 704.
Helene V. Smookler, *Economic Integration in New Communities: An Evaluation of Factors Affecting Policies and Implementation.* Eric L. Stowe. p. 705.
Harold W. Stanley, *Senate vs. Governor, Alabama 1971: Referents for Opposition in a One-Party Legislature.* Coleman B. Ransone, Jr. p. 706.
Frank X. Steggert, *Community Action Groups and City Government: Perspectives from Ten American Cities.* Lynn Foster. p. 706.
Gilbert Y. Steiner with Pauline H. Milius, *The Children's Cause.* John S. Robey. p. 708.
Denis G. Sullivan, Jeffrey L. Pressman, and F. Christopher Arterton, *Explorations in Convention Decision Making.* Byron E. Shafer. p. 709.
Thomas H. Tietenberg with Pierre Toureille, *Energy Planning and Policy: The Political Economy of Project Independence.* Eugene Bardach. p. 710.
William M. Wardell and Louis Lasagna, *Regulation and Drug Development.* Charles T. Goodsell. p. 711.
Robert C. Wilson, Jerry G. Graf, Evelyn R. Dienst, Lynn Wood and James L. Barry, *College Professors and Their Impact on Students.* Sheilah Koeppen Mann. p. 712.

Comparative Politics 713

Lewis Austin, *Saints and Samurai: The Political Culture of the American and Japanese Elites.* Jim Dator. p. 713.
Adda B. Bozeman, *Conflict in Africa: Concepts and Realities.* James R. Scarritt. p. 713.
James H. Buck, ed., *The Modern Japanese Military System.* N. B. Thayer. p. 715.
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Wayne A. Cornelius, *Politics and the Migrant Poor in Mexico City.* Jorge I. Domínguez. p. 719.
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R. Hrair Dekmejian, *Patterns of Political Leadership: Egypt, Israel, Lebanon.* Leonard Binder. p. 722.
Lawrence C. Dodd, *Coalitions in Parliamentary Government.* Kenneth Janda. p. 722.
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Heinz Eulau and Moshe M. Czudnowski, eds., *Elite Recruitment in Democratice Polities: Comparative Studies Across Nations.* Samuel J. Eldersveld. p. 727.

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Leopold H. Haimson, ed., *The Mensheviks: From the Revolution of 1917 to the Second World War.* William G. Rosenberg. p. 730.

Peter Harries-Jones, *Freedom and Labour: Mobilization and Political Control on the Zambian Copperbelt.* James R. Scarritt. p. 732.

Arnold J. Heidenheimer, Hugh Heclo, and Carolyn Teich Adams, *Comparative Public Policy: The Politics of Social Choice in Europe and America.* Richard I. Hofferbert. p. 733.

Christopher C. Hood, *The Limits of Administration.* Jeffrey L. Pressman. p. 734.

Samuel P. Huntington and Joan M. Nelson, *No Easy Choice: Political Participation in Developing Countries.* G. Bingham Powell, Jr. p. 735.

Tareq Y. Ismael, *The Arab Left.* Kamal Ali Beyoghlow. p. 736.

Zev Katz, Rosemarie Rogers, and Frederic Harned, eds., *Handbook of Major Soviet Nationalities.* V. Stanley Vardys. p. 737.

Allan Kornberg and William Mishler, *Influence in Parliament: Canada.* Lawrence LeDuc. p. 738.

Stephen Koss, *Nonconformity in Modern British Politics.* W. J. M. Mackenzie. p. 740.

Craig Liske, William Loehr and John McCamant, eds., *Comparative Public Policy: Issues, Theories, and Methods.* John E. Brandl. p. 740.

Roy C. Macridis, *French Politics in Transition: The Years After De Gaulle.* John S. Ambler. p. 741.

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Louis Maisel and Paul M. Sacks, eds., *The Future of Political Parties.* Thomas H. Roback. p. 743.

Mahmood Mamdani, *Politics and Class Formation in Uganda;* and Issa G. Shivji, *Class Struggles in Tanzania.* Ali A. Mazrui. p. 745.

Joseph A. Massey, *Youth and Politics in Japan.* Akira Kubota. p. 747.

Renate Mayntz and Fritz W. Scharpf, *Policymaking in the German Federal Bureaucracy.* Arnold J. Heidenheimer. p. 748.

Ali A. Mazrui, *Soldiers and Kinsmen in Uganda: The Making of a Military Ethnocracy.* Michael F. Lofchie. p. 749.

B. O. Nwabueze, *Presidentialism in Commonwealth Africa.* René Lemarchand. p. 751.

Jeffery M. Paige, *Agrarian Revolution: Social Movements and Export Agriculture in the Underdeveloped World.* Irwin J. Schulman. p. 752.

Howard R. Penniman, ed., *France at the Polls: The Presidential Election of 1974.* Howard Rosenthal. p. 754.

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Alejandro Portes and John Walton, *Urban Latin America: The Political Condition From Above and Below.* Henry A. Dietz. p. 757.

Lucian W. Pye, *Mao Tse-Tung: The Man in the Leader.* Eugene Victor Wolfenstein. p. 758.

Rouhallah K. Ramazani, *Iran's Foreign Policy 1941–1973: A Study of Foreign Policy in Modernizing Nations.* Marvin G. Weinbaum. p. 759.

David Rock, *Politics in Argentina, 1890–1930: The Rise and Fall of Radicalism.* Richard J. Walter. p. 761.

Dov Ronen, *Dahomey: Between Tradition and Modernity.* Henry L. Bretton. p. 761.

Marc Howard Ross, *Grass Roots in an African City: Political Behavior in Nairobi.* Richard E. Stren. p. 762.

William T. Salisbury and James D. Theberge, eds., *Spain in the 1970s: Economics, Social Structure, Foreign Policy.* George W. Grayson. p. 763.

Richard Sandbrook, *Proletarians and African Capitalism: The Kenya Case, 1960–1972.* Martin Godfrey. p. 765.

Giovanni Sartori, *Parties and Party Systems: A Framework for Analysis. Volume I.* Leon D. Epstein. p. 766.

Mário Soares, *Portugal's Struggle for Liberty.* Translated by Mary Gawsworth. Ronald H. Chilcote. p. 767.

Richard H. Solomon with Talbott W. Huey, *A Revolution Is Not a Dinner Party: A Feast of Images of the Maoist Transformation of China.* Edward Friedman. p. 768.

Rodolfo Stavenhagen, *Social Classes in Agrarian Societies.* Jorge I. Domínguez. p. 769.

David R. Sturtevant, *Popular Uprisings in the Philippines, 1840–1940.* Benedict J. Kerkvliet. p. 771.

Robert H. Swansbrough, *The Embattled Colossus: Economic Nationalism and United States Investors in Latin America.* Robert A. Pastor. p. 772.

Uriel Tal, *Christians and Jews in Germany: Religion, Politics, and Ideology in the Second Reich, 1870–1914.* Translated by Noah Jonathan Jacobs. Martin Jay. p. 773.

Jean-Claude Vatin, *L'algérie politique: histoire et société;* and Jean Leca and Jean-Claude Vatin, *L'algérie politique: institutions et régime.* Clement Henry Moore. p. 774.

Gary Wasserman, *The Politics of Decolonization: Kenya Europeans and the Land Issue, 1960–1965.* John W.

Harbeson. p. 776.
William Foote Whyte and Giorgio Alberti, *Power, Politics and Progress: Social Change in Rural Peru.* Mitchell A. Seligson. p. 777.

International Politics 778

Sydney D. Bailey, *The Procedure of the UN Security Council.* A. LeRoy Bennett. p. 778.
Robert A. Bauer, ed., *The Interaction of Economics and Foreign Policy.* Fred A. Sondermann. p. 779.
Roger A. Beaumont and Martin Edmonds, eds., *War in the Next Decade.* Francis A. Beer. p. 780.
Vivian A. Bull, *The West Bank–Is It Viable?* Robert J. Pranger. p. 780.
David Carlton and Carlo Schaerf, eds., *The Dynamics of the Arms Race.* Frank Barnaby. p. 782.
Francis T. Christy, Jr., Thomas A. Clingan, Jr., John King Gamble, Jr., H. Gary Knight, and Edward Miles, eds., *The Law of the Sea: Caracas and Beyond.* Charles L. Cochran. p. 783.
Ralph N. Clough, A. Doak Barnett, Morton H. Halperin and Jerome H. Kahan, with Alton H. Quanbeck and Barry M. Blechman, *The United States, China, and Arms Control.* Frank Hawke. p. 784.
Jessica Pernitz Einhorn, *Expropriation Politics.* Robert H. Swansbrough. p. 786.
Richard A. Falk, ed., *The Vietnam War and International Law–Vol. 4–The Concluding Phase.* David Elliott. p. 787.
John King Gamble, Jr. and Dana D. Fischer, *The International Court of Justice: An Analysis of a Failure.* Thomas R. Hensley. p. 788.
Robert Gilpin, *U.S. Power and the Multinational Corporation: The Political Economy of Foreign Direct Investment.* Bernard Mennis. p. 789.
Hussein A. Hassouna, *The League of Arab States and Regional Disputes: A Study of Middle East Conflicts.* Paul C. Noble. p. 790.
Roger H. Hull, *The Irish Triangle: Conflict in Northern Ireland.* William D. Jackson. p. 792.
Robert Jervis, *Perception and Misperception in International Politics.* Dina A. Zinnes. p. 793.
Peter J. Katzenstein, *Disjoined Partners: Austria and Germany Since 1815.* John Dreijmanis. p. 794.
Geoffrey Kemp, Robert L. Pfaltzgraff, Jr. and Uri Ra'anan, *The Other Arms Race: New Technologies and Non-Nuclear Conflict.* Lawrence D. Weiler. p. 795.
William R. Kintner and Harvey Sicherman, *Technology and International Politics: The Crisis of Wishing.* Harold Sprout. p. 796.
Charles P. Korr, *Cromwell and the New Model of Foreign Policy: England's Policy Toward France, 1649–1658.* Michael F. Foley. p. 798.
✓Richard E. Lapchick, *The Politics of Race and International Sport: The Case of South Africa.* Thomas F. Pettigrew. p. 798.
Diana Lary, *Religion and Nation: The Kwangsi Clique in Chinese Politics, 1925–1937.* Donald G. Gillin. p. 799.
Richard Little, *Intervention: External Involvement in Civil Wars.* Melvin Small. p. 800.
Richard W. Mansbach, Yale H. Ferguson, and Donald E. Lampert, *The Web of World Politics.* Charles F. Hermann. p. 801.
Bernard Mennis and Karl P. Sauvant, *Emerging Forms of Transnational Community: Transnational Business Enterprises and Regional Integration.* Ralph M. Goldman. p. 802.
E. B. F. Midgley, *The Natural Law Tradition and the Theory of International Relations.* Anthony D'Amato. p. 804.
David Mitrany, *The Functional Theory of Politics.* Robert O. Keohane. p. 805.
David Mozingo, *Chinese Policy Toward Indonesia, 1949–1967.* Harold C. Hinton. p. 806.
Alec Nove, *Stalinism and After.* Thomas B. Larson. p. 807.
Patrick O'Meara, *Rhodesia: Racial Conflict or Coexistence?* A. R. Wilkinson. p. 808.
Charles Pentland, *International Theory and European Integration.* Michael Haas. p. 809.
Elmer Plischke, *United States Diplomats and Their Missions: A Profile of American Diplomatic Emissaries Since 1778.* Gerald E. Wheeler. p. 810.
Gareth Porter, *A Peace Denied: The United States, Vietnam and the Paris Agreement.* MacAlister Brown. p. 811.
Satish Raichur and Craig Liske, eds., *The Politics of Aid, Trade and Investment.* Robert W. Russell. p. 813.
P. Sreenivasa Rao, *The Public Order of Ocean Resources: A Critique of the Contemporary Law of the Sea.* Irvin L. White. p. 814.
Robert A. Scalapino, *Asia and the Road Ahead: Issues for the Major Powers.* Ikua Chou. p. 815.
James P. Sewell, *U.N.E.S.C.O. and World Politics: Engaging in International Relations.* Walter H. C. Laves. p. 816.
Sheldon W. Simon, *Asian Neutralism and U.S. Policy.* Paul M. Kattenburg. p. 817.
Howard McGaw Smyth, *Secrets of the Fascist Era: How Uncle Sam Obtained Some of the Top-Level Documents of Mussolini's Period.* F. W. Deakin. p. 818.
Richard F. Staar, ed., *1975 Yearbook on International Communist Affairs.* Andrew Gyorgy. p. 819.
Robert W. Tucker, *The Inequality of Nations.* Joan E. Spero. p. 820.
Donald P. Warwick, with Marvin Meade and Theodore Reed, *A Theory of Public Bureaucracy: Politics, Personality, and Organization in the State Department.* William P. Snyder. p. 821.

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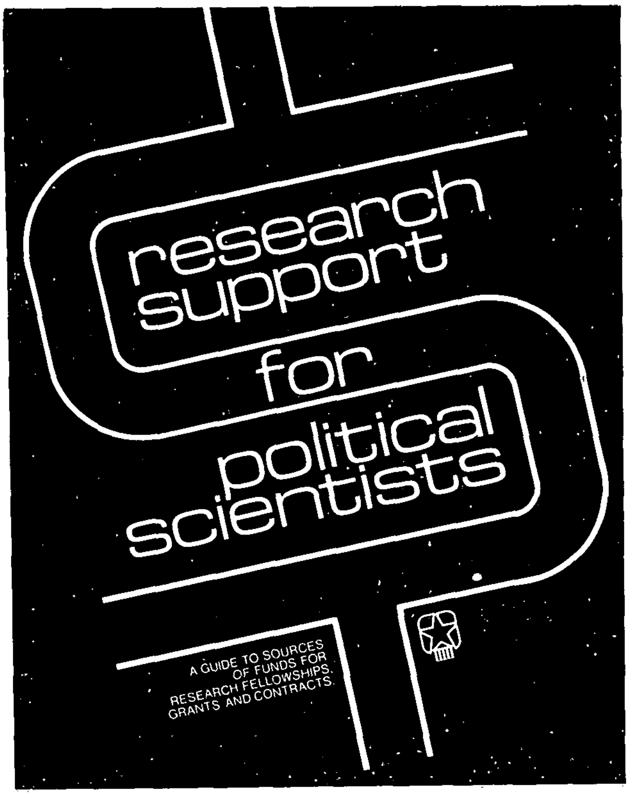
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INDEXING: Articles and notes appearing in the *Review* before the June 1953 issues were indexed in *The Reader's Guide to Periodical Literature.* Current issues are indexed by *The International Political Science Abstracts,* the *United States Political Science Documents,* and the *Social Sciences and Humanities Index.* Microfilm of the *Review,* beginning with Volume 1, may be obtained from University Microfilms, 313 North First Street, Ann Arbor, Michigan 48106. A *Cumulative Index of the Review,* Volumes 1–62; 1906–1968, may also be obtained from University Microfilms. Articles appearing in the *Review* are listed regularly in *ABC Pol Sci* and *Current Contents: Behavioral, Social & Management Sciences.* Book reviews are indexed in *Book Review Index.*

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The Political Implications of the Public Choice - Property Rights School*

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The public choice-property rights perspective is an important intellectual movement aimed at refurbishing and broadening the scope of classical microeconomic reasoning. This paper assesses the political implications of the "school"–its ability both to generate significant political insights and hypotheses and to use political arguments and findings to buttress the assumptions of microeconomic theory.

I conclude that, in the main, neither effort realizes its goal. What we do find revealed by a study of the political implications of the perspective is a well-developed, if generally unacknowledged, normative position, a position that deflects attention from the major sources of power in society and thus complicates the articulation of prescriptions or recommendation statements.

Although the emergence of a distinct "property rights-public choice school" has been fairly recent, its impact has been widespread, stimulating investigation, for example, in areas seemingly as diverse as taxation policy, pollution control, bureaucratic behavior, and theories of justice.[1] The purpose of this paper is to assess the political implications of the perspective and to detail the normative assumptions that underlie it. I conclude that the type of "economic reasoning" advanced by the perspective is not very useful in generating insights into the problems of politics; indeed, it leaves major areas of political analysis unacknowledged. The road to this conclusion is somewhat laborious. We begin by reviewing the place of the property rights perspective in economic theory. From there we study its contribution to a series of issues ranged roughly from economic to political. This investigation leads us finally to a discussion of the theory of the state.

Property Rights and Classical Economic Theory

The property rights perspective embraces three major assumptions of classical economic theory. The first is "methodological individualism"—the positing of the individual as the basic unit of analysis and her or his preferences as the paramount consideration (see Furubotn and Pejovich, 1972). This assumption is closely associated with the second: each individual acts rationally to maximize his own utility, that is, each individual knows his own volitions and pursues them coherently. This maximizing behavior is accepted as the norm. The third assumption (its logical connection with the first two shall be raised later) states that these maximizing individuals will be able to attain gains from voluntary exchange. That is, all things being equal, the free market is desirable.

Where the perspective diverges from classical economic theory is in its denial that these conditions encompass all one needs to know about a particular situation. They rest on, and are only made relevant within, an established system of property which delineates the basic division between mine and thine. Specifically, an existing system of property is a necessary condition for exchange, for "a system in which the rights of individuals are unlimited would be one in which there are no rights to acquire" (Coase, 1960, p. 44). The creation of property rights, moreover, goes far beyond separating mine from thine in a static manner; it also orients the interaction between holders of these rights. "An owner of property rights possesses the consent of fellowmen to allow him to act in particular ways.... It is clear, then, that property rights specify how persons may be benefited and harmed, and, therefore, who

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*Acknowledgments: I want to thank Alfred Diamant, Timothy Hennessey, Trudi Miller, Arthur Schweitzer, and the three referees for the *American Political Science Review* for their incisive and helpful comments. My work on property rights in particular and the institution of property in general has been advanced by a Faculty Fellowship from the National Endowment for the Humanities.

[1] The seminal work of R. H. Coase (1960) is a worthy place to mark the delineation of the perspective. The somewhat unwieldy phrase, "public choice-property rights," has been used because in the interface between politics and economics "public choice" focuses on the former and "property rights" on the latter. While this paper concentrates on the intersection between the two, as the argument proceeds there is a shift in emphasis from property rights to public choice.

must pay whom to modify the actions taken by persons" (Demsetz, 1967, p. 347). This dynamic element injects politics and law into the center of "political economy." No longer can the institutional and the legal structure be treated as a "background factor"; both must be specified precisely. This concern with institutional design and with government as enforcer of rights accounts for much of the interest in the perspective by other social science disciplines. It also forms the basis for the property rights critique of Pigovian welfare economics.

The Challenge of Pigou. In the *Economics of Welfare* Pigou drew a distinction between private and social product. He argued that in the course of legally authorized activities by an individual or a firm, there may arise incidental by-products ("externalities") gratuitously helpful or harmful to others.[2] This being the case, the output of competitive industry can be greater or less than the "proper" output—"Private enterprise left to itself, even when it operates under conditions of simple competition, often leads to a distribution of resources less favorable to the national dividend than some other possible distributions" (Pigou, 1960, p. 381). The solution to this undesirable outcome is taxation or (if output is too low) subsidies to competitive industries and a system of price-setting for monopolies. Pigou even posited the possibility that government *operation* of industry might offer significant advantages (pp. 381–408). In short, welfare economics as developed by Pigou posed a direct challenge to the third assumption of classical economic theory that *ceteris paribus* the free market is desirable. And its conclusion that self-interest needed to be organized through human institutions ascribed to government an integral role in economic organization, far more basic than that posited, for example, by Keynes.

This method of handling "externalities," the incidental by-products of private activities, runs directly counter to the aim of classical economic theory to *expand* the scope of market relations. The challenge is so basic that for Samuels the original impetus for the school was the "effort to make welfare economics safe for the market system" (1972, p. 93). Indeed much of Coase's "The Problem of Social Cost" (1960) is cast as a refutation of Pigou. Coase notes that Pigou does not propose to use the proceeds from taxation to pay those directly harmed (Coase, 1960, p. 41); it might be small comfort to the owner of firm B, forced to incur added structural expenses because of the vibrations produced by firm A (say a car shredder plant) to know that firm A had to pay extra taxes. More generally, the Pigovian solution entails a very "idealistic" view of human affairs—to bring private and social costs into alignment governmental administrators must be not only omniscient but also "economic eunuchs" (Buchanan, 1973, p. 591; in general, see Coase, 1960, p. 34 ff).

The public choice-property rights school, however, goes beyond even the broadest ad hoc criticism and challenges Pigovian welfare economics on the basic level of approach with the notion of reciprocity displacing speculation on the national dividend as the matter of central concern. "We are dealing with a problem of a reciprocal nature. To avoid the harm to B would inflict harm on A. The real question to be decided is: should A be allowed to harm B or should B be allowed to harm A? The problem is to avoid the more serious harm" (Coase, 1960, p. 2). From this position Coase derived what has become known as the "Coase Theorem," which, as formulated by Buchanan, states that "when transactions costs are absent and when income-effect feedbacks are not relevant, the allocational results of voluntarily negotiated agreements will be invariant over differing assignments of property rights among the parties to the interaction" (Buchanan, 1973, p. 580). In other words, if information is free and negotiations costless, and if different income levels do not change demand patterns, agreement does not affect the allocation of resources.

Note that we are referring to the composition of total output. The distribution of income and wealth is obviously altered in the negotiating process; indeed, it is what the negotiating process is all about. Here we see the close relationship between externalities and property rights. If we define property rights as the right to control resources, the right to enjoy the income stream from their use, and the right of transfer to someone else (see, for example, Frech, 1975), the violability of the first two in particular depends on how externalities are handled. For the public choice-property rights school, negotiations can proceed only after the establishment of a legally sanctioned structure of property rights. After this establishment there is, as was argued above, no a priori reason for government interference. It can now be claimed that the presence of an externality does not of itself prescribe corrective governmental action. More—such intervention might even "forestall or distort the negotiations between the affected parties" (Buchanan, 1973, p. 579). In short, market solutions are reimplanted at the center of welfare economics.

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[2] Most of the issues in "externalities" have, since Pigou, been well delineated. The argument concerns how broadly they can be seen as operating (is innovation an externality?) and what, if anything, should be done to resolve them. For a useful review see Mishan, 1971. For a discussion of the central role Pigovian welfare economics *could* play in a challenge of classical economic theory, see Samuels, 1971.

The Critique of the "New Welfare Economics" and the Argument of This Paper Restated

The refurbished classical economic theory outlined above has not gone uncontested within the discipline of economics. Some have disputed the effectiveness of market solutions to externality problems; others have attacked the neglect of any consideration of the structure of social power (see Mishan, 1967, and Samuels, 1972, for two incisive critiques). It is not our purpose to review this debate directly but rather to note that its presence makes the assessment of the *political implications* of the perspective more complex. We cannot merely begin with a respect for the power of "economic reasoning," unchallenged within its own field, and see how far it can valuably be extended. The division between economics and politics is far less clear-cut. The school itself becomes impelled (or sees the opportunity) to address issues that greatly expand the traditional concerns of economists to encompass the development of property rights, the role of law in the establishment and attenuation of rights, and, above all, a theory of the state. In their fine interpretative essay, Furubotn and Pejovich reach, I believe, a similar conclusion: Without a theory of the state "a theory of property rights cannot be truly complete" (1972, p. 1170). This theory is needed not only because the perspective will not otherwise have extended its purview over all of social science, but also because a theory of the state is needed to confute challenges closer to home.

The argument of this paper is that this effort, while impressively drawn, does not succeed. The public choice-property rights perspective offers in the end little that is politically insightful or relevant. Nor can its theory of law and the state be used adequately to confirm its economic prescriptions and assumptions. What we find revealed instead is a well-developed, if unacknowledged, normative position, a position that contributes to the "unworldliness" of the perspective by deflecting attention from the major sources of power in society and thus greatly complicating the articulation of prescriptions or recommendation statements.

Elements of a Critique of the Property Rights School

In this section we shall deal with three separable issues arising from a study of the school—externalities, corporate power, and the development of law. In the interface between economics and politics the discussion proceeds roughly from the latter toward the former. At the conclusion of this section I shall bring together what I have found to be the normative position of the school.

The Complexities of Externality Policy. We recall the centrality of the notion of reciprocity in the critique of Pigovian welfare economics. If the "real question that has to be decided is: should A be allowed to harm B or should B be allowed to harm A," and if the "Coase Theorem" has much applicability in the "real world," the outcome would be to underscore the utility of private negotiations as a way of resolving externality issues. Public policy would strongly favor the current distribution of property rights, as long as these rights had been acquired lawfully or legitimately. And if, for example, it was decided to curtail an existing right by government fiat, the affected party (for example, the firm that could no longer discharge untreated wastes into public water supplies) would have to be compensated for any losses incurred.

Application of this rule is, however, far from straightforward. It must be preceded by a differentiation between marketable and non-marketable ("public") goods,[3] a task which, as Vincent Ostrom (1975) describes, is not easy and which is probably developed along a continuum rather than by dichotomization.

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[3] Public goods (defense, roads, water resources, etc.) are instances of market failure arising from the absence of exclusivity of a good and the jointness of its use. The classic example in Hume's sodden meadow which it is in the interest of all to have drained but which it is in the interest of none to undertake alone. As a consequence, "exchanges in genuinely public goods will not be consumed voluntarily in the same institutional framework that facilitates exchanges in private goods" (Buchanan, 1975, p. 38). Buchanan also speaks of "public bads," the sum of individual actions creating private goods which lead not to public benefit but to public debility (pp. 120 ff). For a general discussion of public goods theory see Mishan, 1971, and Ostrom and Ostrom, 1976.

Even should this division be resolved satisfactorily, we must still consider the large numbers of individuals involved in most externality situations, a fact too often obscured by a concentration on two-party examples (e.g., see Daly, 1974). As numbers rise, so do transactions costs, information costs, the potential distortion of different income levels. It is not to be expected that under these circumstances negotiations will proceed apace; indeed it is interesting that *they rarely occur at all.* (That is, citizens rarely band together to bribe a factory to reduce its smoke, or an airport to mitigate its noise.) This is not necessarily because everyone is content with the status quo. Rather, costs and uncertainties are too high. At a minimum it would seem requisite to transfer the onus of compensation to the side with the fewest numbers. In a case studied by Thomas Crocker (1971), such a shift did lead to negotiations between processors of phosphate rock and surrounding citrus and beef farmers. Since, however, these negotiations were stimulated by Florida's attenuation of the phosphate processors' property rights, the attitude of the property rights perspective toward these negotiations would seem to be unclear.

These complexities gain immediacy when we turn our attention to the "real world." Even if we (society) structure negotiations so that legal liability is placed on the producer (which, according to a strict interpretation of the property rights perspective, we should not do since the right to create diseconomies is an intrinsic property right), we remain concerned with market externalities in a highly restricted sense. If the harm suffered cannot be translated readily into cash and cannot be laid to the specific activity of another property holder, compensation seems unlikely. Moreover, we (the individual, society) continue to be left with the impression that we can afford to let externalities occur unrestrained and *then* arrange for compensation for (some) of their harmful effects. If this passive position could ever have been tolerated, it no longer can be. New chemical compounds, artificial genetic mutations, side-effects from nuclear energy, cannot simply be let loose upon the land with compensation to be subsequently determined. Some must be regulated and others banned at the outset. Compensation through the market cannot counter all the effects of externalities; neither can property be allowed to generate externalities subject to subsequent market compensation.

The Nonperception of Corporate Power. The narrow focus of the school on reciprocally negotiated agreements, combined with the micro-economic assumptions outlined at the beginning of this paper, also obscure the dominance in society of power blocs and associated bureaucracies. State bureaucracies tend to be considered only in their interference in negotiations based on previously defined property rights, an interference said by Buchanan (1975) to be producing a "continuing and accelerating growth in our own Leviathan" (p. 14). Corporations are dealt with in terms of the rather peripheral question (not, it must be admitted, very satisfactorily resolved)[4] of whether managers act differently from owners, and if so how they might be restrained.

What is overlooked is the well-known direction of the economy by a finite number of corporations, associated trade unions, professional associations, etc., able and very willing to impose prices and market conditions. And their numbers are diminishing. The 200 largest corporations now control over 60 percent of the assets of manufacturing firms; in 1941 approximately 1000 of the largest corporations controlled a majority of assets.[5] Moreover, these corporations are linked to public bureaucracies in the United States in what can be called a "positive state model" (see Furniss and Tilton, 1977, pp. 14–21, for a detailed statement). In this relationship the services government provides are substantial—opening up foreign markets (a consistent policy from the Marshall Plan on), financing basic research, guaranteeing profits, bailing out large bankruptcies, maintaining a steady flow of arms sales abroad.

Avoiding the implication of these activities robs the property rights perspective of what one might assume would be a major critical thrust—an attack on the power of the few over the many and the overlapping threat to individual civil liberties. I return to this point later. Even more disturbing in terms of possible prescriptions, it must not be forgotten that prior to the establishment of the corporate-government collaboration, corporations and industrialists *really* ran amok. In the well-known words of Karl Polanyi (1944), "The American Constitution . . . isolating the economic sphere entirely from the jurisdiction of the Constitution, put private property thereby under the highest conceivable protection, and created the only legally grounded market society in the world. In spite of universal suffrage, American voters were powerless against owners" (pp. 225–26). This record makes one loath to recommend blithely a "return to free enterprise" as the solution to the very real dangers of the present government-business collaboration. Has this marriage brought at least a "civilizing influence" on the business partner that one ought to preserve? How is the central question of controlling corporate leaders to be furthered by a divorce? Perhaps the property rights school can resolve these questions. But to do so it must recognize and incorporate the fact of corporate power, of its past abuses, and of its present alliance with the governmental bureaucracy.

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[4] In particular, there is a difficulty in accounting for the power and profitability of noncompetitive corporations, whose managers are not guided by the same motives as the classical entrepreneur. For a review of this problem, see Furubotn and Pejovich (1971). Coase (1937) advances an early and remarkably prescient answer.

[5] Statistics from Wallace Peterson, 1974, p. 487. Whether individual property holders continue in some sense to control these corporations is an intriguing issue which is not, however, central to our present concern. For an argument that they in fact *are,* see de Vroey, 1974.

Misconception of the Development of American Law. The citation of the past legal abuses of corporate power brings us to a discussion of the relationship between property rights and law. The initial stand of the public choice-property rights school (Buchanan and Tullock, 1962, Demsetz, 1967) was that individuals with defined rights existed before any contractual or legal system was established. Law thus confirmed prior property rights. This position obviates the need for any searching inquiry into the origins of the state; its problem, as Buchanan subsequently pointed out, is that it also avoids asking how these rights came to be assigned in the first place (Buchanan, 1975, pp. 7–23).

One might expect the legal consequence of this insight to be an enlarged role for collective intervention in property rights distribution. If the collectivity is involved in the *practical* creation of property rights, it might be deemed to have something to say about its subsequent distribution. This, for example, is the conclusion Thomas Lewis (1975) derived from a provocative study of the implication of money in Locke's theory of property: "Although the right to property is an individual right, the effective exercise of that right is a consequence of civil society and therefore *actual* property is not an individual but a social creation" (Lewis, pp. 266–67). An acceptance of this argument, however, would lead from a concern with established property rights brought to market negotiations toward a focus on the *political* creation and revision of property rights. This shift need not lead to "radical" or "redistributive" conclusions. (Blackstone, after all, did not hesitate to adopt this view.) But it does not describe a process upon which classical economic theory can offer much insight. On the other hand, if this political sphere is to be shunned, how does one avoid the same problem that arose in the *Calculus of Consent*—the problem that any status quo can be justified, even one that allows little or no scope for Buchanan's (1975) well-articulated ideal of "free relations among free men."

The public choice-property rights perspective has always had trouble with this admittedly complex issue. As Buchanan (1975) acknowledges, the *Calculus of Consent* can indeed be used "to rationalize almost any conceivable decision rule. . ." (p. 7). That the problem is not completely resolved is indicated in comments throughout *The Limits of Liberty* such as "The status quo defines that which exists. Hence, regardless of its history, it must be evaluated as if it were legitimate contractually" (Buchanan, 1975, p. 85). Nonetheless, it is perceived that "contrary to orthodox economic methodology, the rights of persons to property . . . cannot be treated in isolation from those rights which are indirectly represented by membership in a collectivity that is constitutionally empowered to make decisions under predetermined conditions." This emphasis allows Buchanan in another essay to distinguish between good and bad law and between proper and improper judicial enforcement. Good law, proper judicial enforcement, upholds the sanctity of contract based on laissez-faire principles. And it becomes possible to set forth the maxim that "the only role of the judiciary should [be] one of determining whether or not the decision taken by the legislature was made constitutionally" (quoted in Goldberg, 1974, p. 575 n. 35). This maxim, however, tells us nothing about the type of law that will emerge which would (given the general language of the Constitution) vary with each interpreter and not be subject to precedent.[6]

Not only do the constitutional counsels become ambiguous, they also sharply restrict the actual focus of property rights school. Not discussed are at least the following political-legal issues that one might have thought would assume new prominence in a scheme that rejects the isolation of property from other citizen rights: First, whether property rights, originally basic to the establishment of individual autonomy, have now become a hindrance to the development of a free society, and, if so, how the prior relationship can be re-established (this problem is addressed in a different way by John Stuart Mill, 1968, esp. pp. 936–71, T. H. Green, 1941, pp. 211–29, and R. H. Tawney, 1964). Second, whether there should be more emphasis on the *utilization* as opposed to the *status* of property (see Friedmann, 1972, pp. 93–118). Third, whether the extension of actual property rights to all must of necessity greatly reduce the bundle of rights held by some (as when the Supreme Court held, in *Jones* v. *Mayer, Co.*, that the 1964 Civil Rights Act invalidated *all* racial discrimination, private and public, in the sale or rental of property).

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[6] An indication of the uncertainties that can result from this form of "strict constructionism" is indicated by its vigorous espousal by the late Justice Black, whose opinions tended to diverge markedly from

What *is* revealed by this "strict constructionism" is the strong normative bias, the infusion of value judgments, into the heart of the perspective. That the individual preferences referred to are the preferences of the author is apparent in the *Limits of Liberty;* it would take an omniscient observer with great psychological acumen to discern any "scientific" veracity in the following:

> [During the 1960s] collective decision makers *were led* to enact programs which found their origins, not in the demands of citizens, but in the brains of academicians and the slogans of politicians.... Citizen reaction *was acerbated* by an activist judiciary.... As the federal courts *were seen* to 'make' law in their own idealized image, by natural extension individuals *came to think* of their own private criteria for discriminating between 'good' and 'bad' law (Buchanan, 1975, pp. 172–73, my emphasis).

This bias is perhaps most apparent in the treatment of an "activist judiciary" bent on making "law in their idealized image." Historically, the great period of judicial activism, after *Marbury* v. *Madison*, came in the late nineteenth and early twentieth centuries with the discoveries by the Supreme Court that invisible capital held by a corporation was property, and that the corporation, being merely economic man writ large, could be accepted as a person and thus was equally entitled to all constitutional guarantees including those establishing contracts. These two findings, as Justice Holmes remarked in *Lochner* v. *New York* (1905), enabled the Court to read Spencer's *Social Statics* into the constitution and thereby strike down numerous legislative efforts to make the people slightly less "powerless against the owners." (This history is described in some detail by Miller, 1972.)

With the Depression, this opinion, never fully accepted judicially outside the Supreme Court and not consistently applied within it, underwent extensive revision. First, the elemental distinction was made between a contract for work and a contract for sale, with the former a proper concern of collective action. Next, beginning with *Nebbia* v. *New York* (1934), a stance of *judicial restraint* in the economic field was adopted, with legislative supremacy acknowledged by a unanimous Court in *Ferguson* v. *Skrupa* (1963).

This revised position, to be sure, contains many ambiguities. As has already been alluded to, actual constitutional interpretation, particularly of issues involving possible violations of the Bill of Rights, involves individual interpretation, the discovery, in Justice Douglas' words (in *Griswold* v. *Connecticut*), of "penumbras formed by emanations" that give the constitution meaning. Now, since one person's "penumbra" is another person's dangerous backsliding into activism, the potential for disagreement is endless. Moreover, since rights in practice are often hard to divide neatly between "personal" and "property," a decision whether a particular case falls into a protected category or not can be equally contentious (see Funston, 1975).

In sum, there is a real need for a broad focus on the legal implications and the potential libertarian benefit of the corporate domination of the legal process—evidenced in such concrete ways as access to legal information, access to courts, ability to win favorable judgments, and ability to enforce judicial decisions.[7] More generally, neither the public choice-property rights school nor social science generally has come to grips with what Warren Gramm has

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those articulated within the public choice-property rights perspective school. The comment of Robert Dixon (1965) is applicable to both parties: "It may come as a surprise to some to find that all Mr. Justice Black has been doing in his constitutional adjudication, at least in his own self-analysis, is to apply the 'clear meaning' of the constitutional text.... One aspect of 'clear meaning' jurisprudence is its impredictability in the guise of being best articulated" (pp. 13–15).

[7] This legal bias (which should not occasion much surprise) is spelled out by Marc Galanter (1974). Useful statistical confirmation is presented by Craig Warner (1974).

termed the "higher law" of the modern business enterprise which is based on the premise that for large corporations normal common or statutory law is outmoded. It has been replaced by a "symbiotic relationship between the business, legal, and political communities" which replaces legal rules by dialogue and compromise (Gramm, 1973, p. 579, and passim). Even in cases where disputes are aired in court, direct orders to the corporation to desist its activity are followed, not by compliance, but by appeals, delays, and even overt refusal. Needless to say, the uncovering of large-scale bribery seems to send no one to jail. It is this phenomenon, rarely reviewed[8] much less pursued in social science literature, that could help account for the widespread discontent with governmental institutions.

Personal Preferences and Empirical Investigation. The common claim of the public choice-property rights school is that personal preferences are uniformly eschewed in favor of rigorous scientific inquiry (see Buchanan, 1975, p. 15, for a typical statement). This claim, as we have seen, cannot be sustained—the more one moves from economic theory to politics, the more (as one might suppose) preferences and values intrude. In itself, this development is far from objectionable; a discussion of values, as Max Weber argued long ago, is intrinsic to any important social science investigation. The problem with the school is that denying the presence of normative concerns causes values to remain latent, and the tension among them unexplored.

To begin with, the very laudable goal of "free relations among free men" seems to consist of noninterference with individuals rationally maximizing their utility. This position seems straightforward enough, and it has a surface plausibility that generates a fairly wide appeal.[9] But when utility maximization is transferred from the context of bargaining over economic goods to the arenas of politics and moral behavior, it is soon discovered that excessive individualism is not socially cohesive, that one does not *really* want everything decided on the basis of the highest bidder, and that a lack of regard for others brings great pressure for social compulsion (that is, freedom pushed too far results in the establishment of less freedom).[10]

Something more, in other words, is needed besides utility maximization. This Buchanan (as did Herbert Spencer when faced with a similar need to restrict nongovernmentally the compass of laissez-faire) locates in the concept of "ordered anarchy." While anarchy cannot work as a universal organizing principle for social order, it does have "essential properties [which] can be observed to operate over large areas of human interaction." These properties are accepted standards of conduct that lead people not to play stereos in the middle of the night, to refrain from interrupting speakers, to have a care at what hour they run their lawnmowers. These are the courtesies, the deference to others, that make society function naturally; any reduction in their sphere should be seen "as an unmitigated bad" (Buchanan, 1975, pp. 4, 5). The ideal image evoked by Buchanan is the university of the 1950s when there existed "relatively pure standards of free expression," when students were deferential to professors, when in general, matters aligned with Buchanan's preferences.

The major difficulty with this notion is that utility maximization and ordered anarchy can be reconciled only by arbitrarily restricting their scope. In economic affairs (particularly those corresponding to free market principles) utility maximization is to be the norm. No room to reorder property rights here! But in other activities one must be tolerant, submissive, nonmaximizing. Why should others subscribe to this division? Why, to return to our example, should students accept it, particularly when, by the testimony of the school itself, universities are exemplars of not-for-profit firms in which managers (deans and professors) have "unusual scope for increasing their non-pecuniary income at the expense of the firm's customers and patrons" (Furubotn and Pejovich, 1972, p. 1154)? And, indeed, the turmoil of the 1960s did result, as one might expect, in concrete gains for students. Parietal rules collapsed, some student control over the curriculum and personnel decisions was established, grades soared. In short, students maximized their utility.

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[8] For two notable exceptions, see Davis (1969) and Lowi (1969). Davis remarks that "not only is the power of selective enforcement typically ungranted through legislative delegation, and not only is it completely unguided by statutory or other standards, but it is also unstructured and unchecked, and it includes the power to set aside legislation in whole or in part, no matter how clearly the legislative will has been expressed" (p. 218).

[9] Samuel Brittan (1973), for example, has been a consistent proponent of the argument that capitalism and diverse life styles are not only compatible but two peas in a pod—the capitalist and hippie should unite against the bureaucrat. Also, as he admits, they rarely do. Do they know something he does not?

[10] This dilemma is apparent in Buchanan (1975). He begins by sounding like Brittan: "Under regimes where individual rights to do things are well defined and recognized, the free market offers maximal scope for private, personal eccentricity, for individual freedom in its most elementary form." But then he notes two pages later that the "counterculture" with its "explicit flaunting of traditional codes of conduct . . . [placed] stresses which were evidenced by calls for 'law and order,' for formalization and enforcement of rules that were previously nonexistent" (pp. 18, 20).

Now, it is quite true that the consequences of this utility maximization have not been uniformly propitious. Some "reforms" have been unfortunate, others (grade inflation) have probably been self-defeating. This is only to say that "ordered anarchy" does have value. How unfortunate, then, that the property rights perspective clings so tenaciously to utility maximization. Would the value of "ordered anarchy" not be better advanced by an attempt to construct a perspective that would extend the benefits of mutual tolerance and sympathy to the economic sphere and restore it elsewhere where it has been debased? In other words, an adherence to the value of "ordered anarchy" might seem to mandate a *restriction*, not an expansion, of market relationships.

To summarize this section, as the perspective moves from economic toward political concerns, there arises the at least latent tendency to espouse the value of ordered liberty. This value, however, conflicts with the earlier commitment to unrestricted free enterprise and inviolable property rights. As long as one maintains, as for example does Coase (1960), that in city planning "it would be desirable to preserve . . . areas . . . for residential or professional use . . . only if the value of the additional residential facilities obtained was greater than the value . . . lost" (p. 10), there is little reason why others should not display similar attitudes in other fields.[11] For while it may have been historically accurate that social controls (the family, the school, the church) enabled the unfettered pursuit of utility in the economic sphere while enforcing an agreeable deference elsewhere, no longer do not operate as effectively.

The Theory and Role of the State

Thus far, we have indicated that the property rights perspective does not do much to fill the gaps in our understanding of the political implications of externality, the ramifications of corporate power, or the function of law in advanced industrial society. This conclusion reinforces the previously quoted judgment of Furubotyn and Pejovich that without a theory of the state "a theory of property rights cannot be truly complete." The parts do not stand well alone; perhaps, however, they coalesce better as a whole.

This theory of the state, as Furubotyn and Pejovich acknowledge, was rather slow to emerge from the property rights perspective. Some delay, no doubt, was occasioned by the nature of the task: an account of the establishment and legitimation of property rights had to be developed and integrated with the issues raised in the study of public goods. More delay may have resulted from the "end of ideology" factor—if the existing distribution of property and the institutional structure that sustained it were indeed accepted by the bulk of the population, there was less need to delve into the potentially embarrassing question of origins or, as Nozick would say, "justice in acquisition." In any event, we have witnessed recently a spate of works employing economic reasoning to arrive at or derive a theory of the state. (For a provocative review, see Gordon, 1976). Here we focus on Buchanan's work, *The Limits of Liberty* (1975); first, because Buchanan is one of the major figures associated with the perspective; second, because his book provides a clear, articulate, yet subtle, argument of the position; finally, precisely because the argument is rigorously drawn, we see starkly the tension between the blessings of ordered liberty and the operation of unrestricted markets founded on inviolable property rights.

Buchanan's theoretical description of the state, briefly, is as follows: a number of free (independent) individuals are made interdependent by the competition for scarce resources. (That is, consumption of a unit by A imposes an "external diseconomy" on B and vice versa.) In the course of this competition there occurs a "natural distribution" of the unit "secured upon investment of effort in attack and/or defense of consumption shares" (Buchanan, 1975, p. 24). Each individual, in other words, has some property, which is, moreover, "naturally" maldistributed. At this point there arises the potential for contractual agreements as both parties recognize the wasted effort in securing and defending their stocks of the unit and see that they would both be better off were the "natural distribution" to be formalized as guaranteed property rights; that A might emerge with less than B does not affect the potential gain. Property rights thus emerge secured in a first "Constitutional Contract" (analogous to Locke's "original compact").

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[11] Coase does seem to diverge from Buchanan in not allowing any room for ordered anarchy. He does not flinch, in other words, from total utility maximization everywhere: ". . . we should use the same approach for all markets when deciding public policy" (1974, p. 389).

At this stage (again in a pattern similar to Locke's) it becomes possible to form a "post-constitutional contract" to capture the realizable surplus through public goods trade. This contract creates government, the "productive state." Ideally, this state should function according to the principle of unanimity; this decision rule for reaching collective decisions would be equivalent to the two person trade in private goods. In practice, however, given the large number of participants, and the costs of information and vetoes, it becomes necessary to move away from a strict unanimity rule. All such qualifications should be made clear and accepted at the constitutional stage.

From these contracts spring political institutions with clearly defined roles. First, although (as in the "prisoners' dilemma" game) each participant in the constitutional contract can be expected to gain from its enforcement, as the number of participants rises, so does the ability, and thus the rational incentive, to default on one's obligations. I benefit if my property is respected, but I gain still more if I can, in addition, take goods belonging to others. My chances of doing so undetected go up with the number of mutually interdependent actors. Thus the need to enforce agreement by establishing concomitantly an enforcing agent (the "protective state" founded on the judiciary). Second, institutions should be created to facilitate, in the post-constitutional role, complex exchanges among individuals (collective choice through the productive state). These institutions and their functions should be clearly demarcated. The enforcing agent, the judiciary, should be "independent of the choice-making arm of the collectivity, the legislature" (Buchanan, 1975, p. 69). Likewise, it should not engage in legislation. On the other side, the legislature should not tamper with the constitutional contract and should restrict itself to mutually beneficial trades.

We can now see concretely how Buchanan would approve of a constitution as defined by *Lochner* v. *New York*. On the one hand, the judiciary refrained from social intervention; on the other hand, it sternly forbade the legislature from attenuating property rights or from regulating contracts. But, as Buchanan realizes, this era of what is termed "legal formalism" was both short-lived and unlikely to return.[12] The inability or lack of desire on the part of the "protective" state to keep either itself or the "productive" state within its prescribed sphere leads to what Buchanan in chapter 9 calls the "Threat of the Leviathan." In this chapter Buchanan poses in stark form the "First Problem of Politics . . . the antique and yet ever-recurring problem of how citizens can prevent their rulers from becoming tyrants" (Dahl and Lindblom, 1953, p. 273).

This, broadly, is the theoretical picture of the state, how it ought to work, and why it might not work properly. The difficulties inherent in this construction go, I believe, to the center of the perspective. First, we have the probable lack of coincidence between property rights as they might have existed at the moment of the original contract and as they do exist now. Buchanan can believe, in a passage previously cited, that the status quo "must be evaluated as if it were legitimate contractually," but there is no reason for others to agree. There is no reason why an individual, thinking back to an original compact, would not conclude that he would have had more, had he not for various reasons been dispossessed from his original and therefore rightful inheritance. (Remember that the school vigorously asserts its value-free nature—statements on how an individual *ought* to behave are deemed out of place.) And, indeed, most movements for basic reform in the status quo claim exactly this—they are "radical" in the sense that they look to reconstruct an order more in keeping with their notion of original precepts. Less fundamentally, it would not be surprising to see individuals and groups attempt, with varying degrees of success, to use the political institutions of the "productive state" to reorder the pattern of property relations. A prohibition against using agreed-upon decision rules for this purpose, one would anticipate, would be viewed as inherently arbitrary.

Second, the public choice-property rights perspective itself allows for the creation of less-than-unanimity rules to realize the gains from public goods. The institution of less-than-unanimity rules, however, raises the prospect that an individual might consider his utility level lowered and would be powerless to do anything about it (precisely why the decision rules are needed, to avoid constant vetoes). In this case, implementing an exclusion rule (e.g., making it so the individual would not be affected by the decision) raises difficulties with the notion of inalienable property rights. Either way, the line between what functions are and are not legitimate for the "productive state" to perform becomes blurred.

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[12] There may be less appreciation that the era of legal formalism exemplified by *Lochner* only came "into being after the transforming surge of postrevolutionary legal activity had become uncongenial to its beneficiaries" (Horwitz, 1977, p. 266).

Third, we return to the question of why property rights should be valued. The answer, implicit in the perspective, is that they engender or embody freedom by permitting, indeed mandating, independent action and by providing a bulwark against state (collective) tyranny. Let us grant this premise. Would it not follow that it should be a primary goal of public policy to further the wide dissemination of property, and if the "free market" fails to do so (and in fact results in its *concentration*), should it not be jettisoned? This was the explicit argument of social theorists as early as Green and Hobhouse.[13] It is given immediacy by the movement (particularly in Europe) toward co-determination in and co-ownership of industry. The results of capitalist production, the creation of a class with no private assets, these proponents urge, must be counterbalanced by public policy leading to a dispersion of property rights. A similar, if less comprehensive, argument is made by many "institutional economists," with writers as early as John Commons declaring there was a "property right in a job." (For a summary of this theme, see Kanel, 1974.) Such a right, if recognized and instituted, would constitute a major redistribution that could be undertaken only through the political process.

Buchanan himself, in some of the most notable passages in the *Limits of Liberty*, recognizes that the specter of democracy becoming its own Leviathan cannot be dissipated merely by delineating the "structural flaws" in the present order. For Buchanan, the problem is not just that bureaucrats, their number swollen to a critical size, are ceaselessly trying to expand their spheres, that politicians are always concerned with "doing something," and that judges, motivated by who knows what, are out of control. These individuals derive a good deal of latent energy from the mass of people who do not accept the current pattern of property claims and who see nothing amiss in getting more politically through the institutions of the productive state. Thus it is naive to propose (see, for example, Meltzer, 1976) constitutional limitations as a viable solution. A new bargain must be struck.

For Buchanan, the way out, the way to draw the claws of the bureaucrats and others who frustrate the common good, is to appeal to the self-interest of the *rich* (no other type of appeal, under the property rights perspective, is permissible). The rich are told first of all that they may legitimately expect their " 'property rights' to be respected and honored, and violations of these rights enforced, only as a component part of the more inclusive contractual arrangement which predictably requires that they pay differentially higher shares in those goods and services provided jointly for the whole community" (Buchanan, 1975, p. 73). In other words, the rich really ought to pay more taxes. But even this is probably not enough. To make sure that the political process restricts itself to supplying public goods:

> the rich man, who may sense the vulnerability of his nominal claims in the existing state of affairs . . . can potentially agree on a once-and-for-all or quasi-permanent transfer of wealth to the poor man, a transfer made in exchange for the latter's agreement to a genuinely new constitution that will overtly limit governmentally directed fiscal transfers (p. 73).

This scheme has similarities with the position of Green and Hobhouse that a stable order requires (they would also add, that social justice demands) that each individual be given a stake in society, the stake being property. If implemented, it would produce a redistribution of wealth that would make what current reform proposals there are look parsimonious indeed. But the plan hasn't a chance. It is unrealistic because it rests solely on an entreaty to the rich and rejects as illegitimate the only forum (politics) that has ever succeeded in effecting transfers.

Let us look at this plea from the point of view of the "rich man" (not a very descriptive term, but then, being founded on methodological individualism, the perspective cannot admit the existence of classes). There are three reasons to reject it, any one of which would be sufficient. First, *how* exactly will future transfers be "overtly limited"? How is the next generation to be bound? What if wealth (as is likely) again becomes maldistributed? What is to prevent the new "poor man" from threatening to install the Leviathan unless there is another distribution of wealth? With these uncertainties, the rich man would be foolish to transfer a good deal of actual wealth in return for a paper promise (remember what happened to *Lochner*) unless he were under extreme compulsion, which, second, he is not. The best available evidence points to a remarkable continuity in the (mal)distribution of assets in the United States since World War II with the top 5 percent holding around 25 percent of the wealth, in terms of money income (*after* taxes) the top two deciles garnered 43 percent, the bottom two 4.5 percent (see the excellent comparative study by Sawyer, 1976). The "rich man" in the United States has even succeeded in staving off welfare reforms considered basic to collective decency in most other advanced industrial countries—full employment guarantees, health insurance, family allowances. In these conditions, why concede anything?

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[13] T. H. Green (1941) stated that the rationale of property "is that everyone should be secured by society in the power of getting and keeping a will" (p. 220). Hobhouse (1915) maintained that property is "an integral element in an ordered life of purposeful activity," but that industrialization has "virtually abolished property *for use*." Its reinstitution requires state intervention (pp. 9, 21).

Finally, the operation of political institutions in the United States has not only not threatened to produce a major redistribution of wealth and power (to be sure, the distribution would be somewhat more skewed without "governmentally directed fiscal transfers"); as we have previously described, government assistance is essential for corporate activity. For Buchanan, government may be becoming a Leviathan; for the rich man its dictates can be circumvented with relative ease. To summarize, if Buchanan believes a transfer of wealth is necessary, he would do better to address his appeal to the "poor man." But this he cannot do since he must deny that any transfer is "deserved" (it is expedient) and since he rejects the use of political institutions to effect it.

Conclusion

These difficulties in constructing a viable theory of the state help confirm our finding of the previous section that the political implications of public choice-property rights school are cloudy at best. Despite ad hoc empirical insights, it has not been demonstrated that "market logic" can be applied fruitfully to the broad range of political problems beyond the ken of traditional microeconomic theory. Rules for behavior based only on individual preferences have not been developed; the perspective is infused with values. But again, despite our discovery that many of the values are latently attractive, the perspective does not function well as an exponent of a normative position because the very presence of values is denied. Nor is the perspective a good source of "recommendation statements" because we start not with empirical description but with a construct that screens out important areas of social reality. Finally, the effort at diagnosis is not persuasive. No one can question the plethora of "public bads." Whether their cause lies in the failure to observe unrestrained utility maximization in the economic field and orderly anarchy in all others is doubtful. It seems more likely that the effective actualization of many of the values inherent in the perspective—the focus on the individual as the essential concern; the importance of property in structuring relationships and in defending against dangerous augmentations of collective power—has been thwarted by the concern to promote the sanctity of the market and the inviolability of private property, which themselves cannot be totally reconciled. The rigorous presentation of the perspective is evident, and the scarcity of alternative perspectives and analytical tools that help order and give insight into contemporary social life make its attraction understandable. But, I conclude, claims of major political significance cannot, in its present development, be sustained.

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Institutional Solutions to the N-Prisoners' Dilemma*

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The existence of N-prisoners' dilemmas (conflict between individual and collective rationality) is a standard justification for collectivizing decision making through the state, but there is little theory about how different institutions operate to "solve" such dilemmas. The efficiency of majoritarian democracy, "selfish dictatorship" and uncoordinated individualism is explored using a simple nine-person game. Majoritarian democracy is found to produce equilibrium outcomes at the maximum social product available when the opportunity costs of "cooperating" are less than half the magnitude of the externalities, but to produce no such equilibrium when the opportunity costs are greater than that. Our research draws implications for the role of information in constitutional design, and of altruism.

The logic of the prisoners' dilemma[1] provides what is probably the most widely accepted, and certainly the most coherent, justifying theory of the state available today. When individual behavior involves external diseconomies (pollution, for example) or external economies (as in the case of collective goods), markets can be expected to produce suboptimal outcomes. Individuals will not voluntarily restrict their own polluting behavior if it costs them substantially to do so and neither will they contribute toward costly collective goods when others can be expected to provide them. Taking such "collectively regarding" actions means that they will run the risk of being a "sucker" who is left carrying all the costs—and that they will pass up the benefits that come from being a "free rider" on the collectively regarding actions of others. *When the externalities are of a sufficient magnitude,* selfish-maximizing individuals will behave so as to produce outcomes that nobody wants: in the pure case collective goods will not be produced at all and negative externalities will eliminate all the gains individuals make from their own

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*Dallas Hardison and Anne Schneider participated in the original sessions out of which this paper was developed and have been constant sources of help since then. Brian Barry, Robert Campbell, Robyn Dawes, Geoff Fougere, R. Kenneth Godwin, David Messick, William Mitchell, Howard Rosenthal, Barry Siegel and Jeff Smith have all given valuable advice and encouragement. Versions of the paper have been delivered to the University of Oregon Psychology Department's seminar on collective dilemmas, the Political Science and Sociology Departments of the University of Canterbury, New Zealand, and to political science classes at the University of Oregon; thanks are due to the participants in these meetings who invariably offered much-needed and constructive criticism.

[1] The prisoners' dilemma refers to the parable of two prisoners who have been captured and charged with the same crime. They are locked in separate cells unable to communicate with each other. Each is told that his punishment will depend on his decision to confess or deny the joint execution of the crime, but that the severity of the punishment depends on the simultaneous decision of the other prisoner. Punishments are structured so that: (a) there is a dominant strategy to confess for each prisoner, but (b) both confessing leads to each receiving a punishment that is worse than had both chosen the dominated strategy of denial. The following payoffs meet the definition of a prisoners' dilemma:

		Prisoner A	
		confess	deny
Prisoner B	confess	5 5	8 0
	deny	0 8	1 1

For each prisoner, the dilemma lies in the conflict between what is rational action for oneself as an individual and what is rational action for oneself as a member of a two-person collective. By attending to their individual payoffs, prisoners will be led to an outcome that is at best suboptimal for themselves and at worst a "collective disaster." For the classic statement, see R. Duncan Luce and Howard Raiffa (1957).

selfish actions.[2] Just as some means of coordinating their behavior can get the two prisoners the lowest total years behind bars, so—it is argued—can the state coordinate individual behavior to produce an "optimal" level of negative and positive externalities.[3]

But saying that coordination by the state is necessary if a society is to reach an optimal outcome in prisoners' dilemma situations begs the crucial *constitutional* question: What *form* ought the state's institutions take in order to insure such an outcome? More generally: How does variation in the institutional context of situations characterized by external effects alter the chances of an optimal outcome? The essential nature of the prisoners' dilemma does not lie in the assumption that decisions are made in the absence of coordination. It lies, rather, in a particular configuration of payoffs facing individual decision makers—where a "cooperative" solution exists for which the aggregate payoff is greatest but where there is a dominant strategy for individuals to "defect" from this cooperative solution. Where such a configuration exists (or where there is such a "state of nature," if we are to use a somewhat older terminology) it is clear that uncoordinated decision making by selfish maximizers will produce a suboptimal outcome. But the mechanism is not the same thing as the problem; the decision rules are a variable that can be manipulated as much as any other.

If psychology can tell us about the individual characteristics that influence "cooperation" and "defection" in prisoners' dilemma games (and, in social psychology, about the interpersonal patterns that explain this choice behavior), and if economics continues to report on the circumstances surrounding market success and failure, it would seem an appropriate disciplinary division of labor for political science to claim this constitutional question as its own. That is what we will address here.

Background and the Model

Although the first steps toward formulating the institutional question, and toward providing some answers, were taken more than 50 years ago by economists Knut Wicksell (1958) and Erik Lindahl (1958), there has been a long period in which this question has been neglected. Concern with the environmental crisis has, however, led to some constitutional inferences from what is essentially prisoners' dilemma logic. Although his argument is not particularly formal and only parallels prisoners' dilemma logic in some places, Robert L. Heilbroner (1974) has concluded that "iron governments," most likely military governments, are inevitable if certain ecologically necessary policies are ever to be implemented. Garrett Hardin's well-known analysis (1968) of the "Tragedy of the Commons" is often thought to lead in the same direction. Hardin is concerned with common property resources available for exploitation by profit-maximizing individuals. His paradigm case concerns common grazing lands onto which farmers are free to add cattle for their private benefit. Prisoners' dilemma logic dictates that the commons will eventually be destroyed through overgrazing; farmers recognize that unilateral restraint on the part of each will involve costs that are in no way matched by private benefits and they continue to add cattle—despite recognition that such behavior contributes to the loss of the common resource.[4] Hardin's conclusion, consistent with other analyses of external effects, is that coordination is necessary if the commons is to be saved. He wants "mutual coercion mutually agreed upon."

Impressed by the similarity between Hardin's logic and the essentially prisoners' dilemma logic by which Thomas Hobbes justifies his Leviathan,[5] William Ophuls (1973) has argued that the ecological crisis presents mankind with a choice between "Leviathan and Oblivion." Although at some points he seems to imply that such a Leviathan must be authoritarian or at least antidemocratic, Ophuls really goes no further than to make the unremarkable point that the need for coordination implies the need for more *government*. Like the biologist Hardin, he leaves unanswered the question as to the *form* that government must take. Although neither Garrett Hardin nor Thomas Hobbes can be charged with confusing the two, there is a difference between increasing the scope of government and making governmental decisions in a more authoritarian manner.

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[2] Much social and political conflict can thus be traced to differences among a population about what constitutes positive and negative externalities—or collective goods and collective bads. As Mancur Olson suggests, a society is more likely to cohere "if people are socialized to have diverse wants with respect to private goods and similar wants with respect to collective goods." Diversity of tastes about private goods is necessary to ensure a willingness to engage in market behavior, but consensus about desirable collective goods is necessary if conflict is to be avoided. See Mancur Olson (1969, p. 151).

[3] The grandfather of this line of thought is, of course, Thomas Hobbes, and most contemporary writers acknowledge this fact. For arguments about the appropriate limits of the state see, most importantly, Buchanan (1974), Tullock (1970 and 1974), and Baumol (1965). The justifying theory of the state in these terms has come under severe attack recently in the critical work of Michael Taylor (1976).

[4] Robyn Dawes, J. Delay and W. Chaplin (1974) have shown that the number of farmers influences the certainty of this outcome. With only few farmers, the individual can benefit directly from his own restraint, thereby providing an incentive toward restraint for all. And, of course, when the resource is owned by a single individual, all the costs and benefits of that individual's action are internalized—providing a very sure incentive for restraint. On the latter point, see the important property rights literature, particularly the collection by Furubotn and Pejovich (1974).

Russell Hardin (1971) has pointed out that the usual analyses of prisoners' dilemma focus on the strategy of players making their individual decision whether to defect or cooperate and has proposed an alternative analysis in which the players make a collective choice among the various possible *outcomes*. He concludes that the "all cooperate" outcome is the dominant one in competition with all possible others and that, therefore, "the cooperative outcome in such a game would prevail in election against all other outcomes." His analysis certainly does not support the pessimistic view of the future of democracy that appears implicit in the works of Heilbroner, Hardin and Ophuls. We propose to develop this line of thought further.

Suppose a game consisting of nine players. Each has the choice between defecting and cooperating and, as required by the prisoners' dilemma, the reward for defecting is greater than the reward for cooperating. Defecting, however, involves a "social fine" that must be paid in equal shares by all players. (A more complex—and more realistic—version of this game would have the fine shared unequally by the players. Such complexities would be easy to add to our model, but we will not develop them here.) The existence of this fine means that individual payoffs for both cooperating and defecting will decline as the number of defecting individuals increases. If the fine is great enough to mean that individual payoffs at "all defect" are less than individual payoffs at "all cooperate," the game has the essential characteristics of a prisoners' dilemma; if individual payoffs at "all defect" are more than at "all cooperate," the game does not so qualify. We will develop three versions of this game, two that are prisoners' dilemma and one that is not.

1. Modest Opportunity Costs for Cooperation. In Figure 1 we adopt the graphic conventions developed by Thomas Schelling (1973) and others[6] to portray n-person prisoners' dilemmas. The number of individuals cooperating is measured on the horizontal axis with "all cooperate" at the extreme left of the axis. Payoffs for individual action, measured in positive and negative dollars, are on the vertical axis.[7] In our first game we will set the payoff for cooperation at \$3.00, the payoff for defection at \$5.00, and the social fine—to be paid in equal shares by every player each time one player defects—at \$5.00. We can now specify curves showing the payoffs for defection and cooperation as a function of the number of players cooperating; for this first game they are labeled D and C_1 respectively.[8] Since the right extremity of C_1 is above the left extremity of D, the game meets the definition of a prisoners' dilemma. The constant opportunity cost of \$2.00 (the sum foregone by a cooperator who might have chosen to defect) we will arbitrarily define as "modest."[9]

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[5] This similarity has been noticed by other authors, including Michael Taylor (1976), Orbell and Rutherford (1974), and David Gauthier (1969).

[6] Particularly H. Hamburger (1973).

[7] The dollar numerarie is for convenience only—we might equally deal in anything to which a value can be attached.

[8] We have borrowed the broad outlines of this game from work conducted at the University of Oregon and the Oregon Research Institute by Robyn M. Dawes. See, in particular, Dawes (1975). Notice that setting the magnitude of the social fine equal to the payoff for defection effectively brings the value of "all defect" to zero. This is for arithmetic convenience only. If the social fine were less than the payoff for defection, the payoff for "all defect" would be some positive value, and if it were more, the value would be negative.

[9] Because of the currency of pollution problems, we will develop our models in terms of external diseconomies, but the outcomes would be no different if we developed them in terms of external economies and collective goods. A collective goods version of the game just developed, then, would involve the same payoffs for "cooperating" and "defecting" (\$3.00 and \$5.00, respectively) but instead of the \$5.00 social fine to be shared by the collectivity, there would be a \$5.00 *benefit* to the collectivity every time an individual *cooperated*. It can readily be seen that, although this changes the intercepts, these payoffs produce the same general configuration as the one just described.

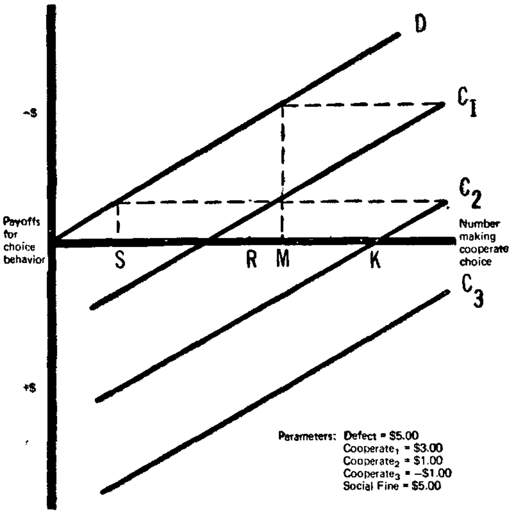

Figure 1. Three Configurations with Externalities

2. Substantial Opportunity Costs for Cooperation. We can increase the opportunity costs for cooperation to $4.00 (which we will call "substantial") by keeping the payoff for defection and the social fine both constant at $5.00 but dropping the payoff for cooperation to $1.00. This new cooperate curve we have labeled C_2 in Figure 1; because its right extremity is also above the left extremity of the defect payoff curve, this game is also a prisoners' dilemma. The most obvious difference is that cooperation is now relatively expensive and therefore, we can predict, relatively less likely; if altruists might have been willing to carry the $2.00 cost of community-regarding action in the first case, they might think longer before taking the equivalent action here. Initially, at least, it would seem that ecological crises such as these are more intractable—and more requiring coordination by government.

3. A Non-Prisoners' Dilemma With Externalities Present. If we once again keep the same $5.00 payoff for defection and the same $5.00 social fine, we can drop the cooperate payoff lower to –$1.00. Since the right extremity of the cooperate curve (which we will label C_3 in Figure 1) is now *lower* than the left extremity of the defect curve, the game no longer meets the definition of a prisoners' dilemma. It is important to notice that externalities are still present, but that they are now no longer sufficient to mean that the optimal social payoff is reached when all cooperate. Instead, if the society is to do as well as it might, it is best for all its members to *defect.* To anticipate somewhat, we would not expect this to be a particular problem for uncoordinated individuals since the incentive in that direction is now substantial. But it remains to see what outcome would be produced for such a game by various mechanisms of collective decision making.[10]

In Table 1 we have recorded the individual payoffs that result for the three configurations just discussed. The numbers (of our nine-person

Table 1.

		Number Making Each Choice									
	D:	**9**	**8**	**7**	**6**	**5**	**4**	**3**	**2**	**1**	**0**
	C:	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
Opportunity Costs Low	*D:*	.00	.56	1.11	1.67	2.22	2.78	3.32*	3.89*	4.44*	
Prisoners' Dilemma	*C:*		–1.44	–.88	–.33	.22	.77	1.33	1.88	2.44	3.00
Social Aggregate		.00	3.04	6.01	9.03	11.98	14.97	17.94	20.94	23.96	27.00
Opportunity Costs High	*D:*	.00	.56	1.11*	1.67*	2.22*	2.78*	3.32*	3.89*	4.44*	
Prisoners' Dilemma	*C:*		–3.44	–2.89	–2.33	–1.78	–1.22	–.67	–.11	.44	1.00
Social Aggregate		.00	1.04	1.99	3.03	3.98	5.02	5.97	7.01	7.96	9.00
Opportunity Costs High	*D:*	.00*	.56*	1.11*	1.67*	2.22*	2.78*	3.32*	3.89*	4.44*	
Non-Prisoners' Dilemma	*C:*		–5.44	–4.89	–4.33	–3.77	–3.22	–2.66	–2.11	–1.56	–1.00
Social Aggregate		.00	–.96	–2.01	–2.97	–3.98	–4.98	–5.85	–6.99	–8.04	–9.00

*Denotes payoffs for defecting choice which will be preferred to payoff for all cooperate.

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[10] Since "all cooperate" now means that the society makes a markedly suboptimal payoff, perhaps the prisoners' dilemma terminology is less appropriate; "defection" with all the negative connotations attached to it is now socially desirable. Schelling's more general formulation (1973) of "binary choices with

society) cooperating and defecting are specified across the top of the table and the payoff going to each individual in each category is indicated below. Notice that, for each of the three cases, we have recorded the "aggregate social payoff" returned to the society when different proportions cooperate. This can be understood as the amount of wealth in the society as a whole as a consequence of particular behavioral outcomes; the figures are reached by multiplying each payoff by the number of cases involved and summing the results.

Institutional Outcomes

We will explore the social consequences of making decisions by three mechanisms: uncoordinated individualism, majoritarian democracy, and what we will call "selfish dictatorship." In so doing we will make the assumption that our nine players are motivated solely to maximize their individual payoffs, not the social aggregate. In fact, if players were motivated in the latter way there would be harmony between individual and collective rationality—and no dilemma to explore. The assumed selfishness might be pessimistic, but working from this position means that, should we identify some limits to the capacity of institutions to provide "solutions" to such dilemmas, we will know the extent of the social need for altruism.

1. Uncoordinated Individualism and Selfish Dictatorship. The outcomes produced by uncoordinated individualism in these three cases are well recognized. When there is no means of coordinating their behavior, selfish maximizers will defect in all three cases—for the two prisoners' dilemmas producing a "social disaster" and for the non-prisoners' dilemma producing the best outcome available to the society. It is, of course, quite probable that their recognition of what is likely to happen in the first two cases will lead them, as rational individuals, to consider constitutional revision in order to avoid this generally unwanted state of affairs; Hobbes built the necessity of Leviathan and the moral justification for it on this basis. But the outcome *should* they make their decision in this way is quite inevitable.

An analogue to selfish dictatorship can be provided by letting one of the nine players determine, unaided, what behavioral pattern the society will follow. [A *benevolent* despot, of course, will simply discover the pattern that produces the best possible outcome for the society and implement it. But philosophers since Plato have failed to come up with a foolproof way of ensuring that despots are, in fact, benevolent, and we need not concern ourselves with this case.[11]] A selfish individual able to determine the behavioral outcome by his or her own "vote" would do best as the lone defector with all others cooperating, meaning that the defector would decree the outcome immediately short of the right extremity where the largest social product lies. In the first two configurations, when a prisoners' dilemma is present, the society would have to accept substantial inequality and a suboptimal product, but things could be a lot worse.

Things *would* be a lot worse if the selfish dictator were making a similar decision in the context of the non-prisoners' dilemma. From Table 1 we can see that the outcome would still be selected that made the dictator the lone defector, but—because the social maximum is now at the extreme left of the matrix at "all defect"—this would leave the society almost (but not quite) with the worst possible outcome, which is now located at the extreme right. Resorting to selfish dictatorship in the context of configurations that involve externalities but are not prisoners' dilemma seems a prescription for social disaster.

2. Majoritarian Democracy. Let us now assume that our nine players arrive at their collective decision through majority vote. As before, they are still concerned with their individual payoff, but now they must maximize it by voting for the collective outcome that offers them the most as individuals. To point up the real-world analogue, we will imagine the existence of political entrepreneurs, not players in the game itself, whose sole concern is to win office and who must do that by gaining more votes that

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externalities" avoids the problem by labeling the choices simply "left" and "right," but for simplicity we will retain the prisoners' dilemma terminology here.

[11] A more complex model than the one offered here might be concerned with the costs of imposing collective decisions that governments must carry. Implementation of a policy in our model means specifying the individuals who will cooperate and who will defect and structuring things so that it happens that way. This obviously involves the wide range of sanctions available to governments, and imposing such sanctions involves costs. Such a more complex model might also deal with the possibility of side-payments for selfish dictators (or majority) who could legislate the collective maximum and then proceed to redistribute the product entirely to themselves.

their competition.[12] In our analogue, therefore, the outcomes offered by these competing entrepreneurs can be understood as political platforms; a platform consists of a statement about the number of individuals who will be obliged to cooperate and the number who will be permitted to defect, and who those individuals will be. We will make the simple—and no doubt unrealistic—assumption that the voters understand perfectly the contents of the various platforms and the platforms' benefits for themselves *and* for the society.[13]

This democracy, it is important to remember, is not a "free" society in the sense that all are free from constraint; its members are obliged to abide by the majority's collective decision. If voters remained free of the obligations of a "democratic" decision that offered them less as individuals than an alternative, it is clear that they would move as individuals to the alternative—and that the outcomes to be expected from "democracy" would be indistinguishable from those under uncoordinated individualism. Indeed, the only difference between the two mechanisms would be that one began with a meaningless democratic charade. In a recent critique of American political institutions, L. J. Sharpe (1973, p. 133) says:

> How we are to control our leader is assuredly a central problem in a democracy; but, if it is elevated to become the only problem, it is difficult to escape the conclusion that the incapacity of government is a positive good. Yet . . . democracy *above all other systems* has to be capable—has to be functionally effective—because it alone claims to offer the people a choice. To ensure that the choices offered are real ones . . . government at the outset must be strong.

More explicitly in the context of "commons problems," Beryl Crowe (1969) has doubted that governments can make commons-saving decisions stick. Whatever the strength or weaknesses of real-world democratic governments, the majoritarian democracy in our model is, in Sharpe's term, "functionally effective."

Where the opportunity costs for cooperation are modest, our majoritarian model predicts not only the maximum social product as the outcome, but also an egalitarian distribution of that product. To see why this is so, imagine a political entrepreneur who offers the electorate "all defect"—the outcome produced in the absence of government by uncoordinated individualism. His action provides an opportunity for a competitor to offer "eight defect; one cooperate" and to win thereby; while one voter (the individual who would make −$1.44 from that outcome compared to $0.0 on the first offer) would not support this alternative, all the others would support it since it offers them $0.56 compared to $0.0. Thus, although one individual would be obliged to suffer from the defecting action of the other eight, the second platform would dominate the first.

But the same logic can be extended to show that all platforms are dominated by the "all cooperate" platform at the extreme right—where the collective maximum is located. When every platform is challenged by one to its right, there is one voter who stands to lose if the challenger wins, but in every case the challenger is preferred by all others. The defect payoffs noted by an asterisk in Table 1 will be preferred by some players to the payoff they are offered along with everyone else for "all cooperate," but in no case under majority rule will there be sufficient such players for the platform involved to win. Graphically, we can reach the same result by noting that the positive slope of the two payoff functions assures both cooperators and defectors more in platforms to the right of the horizontal than in platforms to the left. Because the number of cooperators increases to the right, however, each successive platform to the right will oblige a single individual to cooperate who was previously permitted to defect. This person will be the lone voter to oppose the rightward move.

It is clear, then, that competing political entrepreneurs operating under majoritarian rules will all be led to offer the electorate the same platform, and that this platform, when implemented, will produce the maximum available benefits to the society. It is not clear just which of the various entrepreneurs will end up being responsible for government—that decision will have to be made by rules beyond those contained in the model—but neither does it matter in terms of the social outcome.

Where the opportunity costs for cooperation are substantial, the outcome from democratic institutions is not nearly so happy. Using the same logic, we can show that every platform is dominated by the platform immediately to its right, but this time the "all cooperate" platform is not an equilibrium point; that platform can be beaten by "two cooperate; seven defect" well to the left of the matrix. In short, for this configuration there is *no* equilibrium solution that can be predicted under majoritarian rules, and a winner would have to be decided by additional rules–saying, for example, who could make a final bid for voter support. In the absence of such supplementary rules, all we can say is that there is no equilibrium solution and, as a result, there is no way to predict what the outcome will be.

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[12] We can assume with Anthony Downs (1957, p. 28) that these politicians "formulate policies in order to win elections, rather than win elections in order to formulate policies."

[13] Anthony Downs (1957) provides an important analysis of how uncertainty affects the strategic thinking of voters and politicians. In our model, no doubt, politicians might find reason to obfuscate exactly what they are offering, and voters might find reason to make long-standing decisions in favor of one or the other politician because of the costs involved in gathering more perfect information. We can remember, though, that competition between the politicians can help keep those same politicians honest as they give information.

This can be readily demonstrated graphically. In Figure 1 we have identified as R the platform at which 50 percent of the players are offered D and 50 percent are offered C. Platforms to the right of this offer a majority C, and platforms to the left of this offer a majority D. If we project a horizontal line from the right extremity of the C_1 function to intersect the D function, we have identified a set of D payoffs that are superior to the C payoff offered everyone at "all cooperate"; these are the payoffs offered to D in the platforms to the right of M (and identified by an asterisk in Table 1). But, because M is to the right of R, there are insufficient players offered these D payoffs ever to comprise a majority against the "all cooperate" platform which is therefore an equilibrium solution. The players offered D in platforms to the right of M will always oppose that outcome, but they will always be outvoted.

Projecting a similar horizontal from the right extremity of C_2 identifies a further set of platforms in which the D payoff is superior to the payoff offered everyone at "all cooperate"–those to the right of S on the horizontal axis. The important difference is that S is to the *left* of R, meaning that there exists a set of platforms (SR) that offer a *majority* the option of D behavior that pays more than the payoff under "all cooperate." Thus, any of these platforms, if offered in competition with "all cooperate," will win. But none of them will be an equilibrium solution since they can all be defeated by platforms to their immediate right. Any platform to the right of S might win–depending on which political entrepreneur makes the final "bid" for voter support.

While only a subset of platforms (SR) can defeat "all cooperate" in head-to-head competition, notice that *any* platform is possible under majority rule for this configuration. We have identified as K the point at which the C_2 function intersects the horizontal. Since platforms RK offer a majority the cooperative alternative, and since for those platforms the cooperative payoff is less than the "all defect" payoff, "all defect" will win in competition with any of them. Thus, there is no reason to exclude platforms between "all defect" and S from the set of possible outcomes; under majority rule, anything is possible.[14]

When the right extremity of the C function is located below the left extremity of the D function, as in the C_3 case, the "all defect" payoff is more attractive than the "all cooperate" payoff and, once again, anything is possible. When configurations are *not* prisoners' dilemma, just as in the previous case, majority rule is capable of producing *any* outcome.

In Figure 2 we have located the C function so that the horizontal from its right extremity intersects the D function at a point P vertical to R. We can see that no platform in such a game can offer a *majority* a D payoff that is superior to the C payoff at "all cooperate"; platform R comes closest, but under it players offered D will be indifferent between R and "all cooperate." Thus, this game is a threshold case: if the C function is any higher, no platform can dominate "all cooperate" under majority rule, but if it is any lower, some *can*—and there will be no equilibrium solution.

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[14] With the exception of the "all cooperate" outcome and the "all defect" outcome, all the other possible outcomes involve some degree of inequality and it is clear that, in the real world, major fights would be fought about who would be the permitted defectors and who would be the constrained cooperators. Strictly speaking, the "platforms" in our tables identify sets of payoffs that can be distributed among any of the nine players in our game; the nine "voters" we identify could be offered either of the payoffs available from a particular platform–with the single constraint that there is a limited number of each to go around. This raises the possibility of an equilibrium platform for this configuration as well; since five votes constitute a majority, it seems possible (for example) that some other version of the "five defect; four cooperate" platform might dominate competing platforms to the right. This is not so, however. While it is true that a different version of "five defect; four cooperate" can defeat the version of "four defect; five cooperate" given first, a different version of "four defect; five cooperate" can, in turn, defeat *that*. Majoritarian democracy continues to be unable to produce an equilibrium outcome in such configurations as these, despite the ability of entrepreneurs to shift around the available behavioral options among the nine voters.

Since *DN* is the magnitude of the *D* payoff in the absence of any externality, and since *CN* is the magnitude of the *C* payoff under the same condition, and since $CN = PR = DC$, we can define this threshold case as *where the opportunity costs of C (DC) are exactly equal to the payoff from C in the absence of any externality*—or alternatively, as *where the payoff from C is exactly equal to one half the payoff from D in the absence of any externality.* Since *DN* is also the magnitude of the externality paid as a consequence of every act of *D* behavior, we can define the same threshold in terms of the externality as *where the opportunity costs of C are exactly one half of the externality associated with D.* In general, we can conclude that *when the opportunity costs of C are more than half the externality, majority rule will produce no equilibrium solution to the n-prisoners' dilemma and any outcome is possible.*

Summary

Figure 3 shows the relationship between the opportunity costs of cooperative action (measured on the horizontal axis) and the total social product (measured on the vertical axis) for a society of nine individuals and a social fine for defecting action of $5.00—as in our three cases. The heavy broken line shows the maximum social product available to this society, given different opportunity costs, and the light broken line indicates the minimum social product available. We can define the following parameters:[15]

1. In the limiting case where the opportunity costs of cooperation are equal to zero (in terms of Figure 1, when the cooperate and defect curves are identical; in terms of Figure 3, at the left extreme of the horizontal) the total social product to be derived from "all cooperate" is equal to the number of individuals cooperating times the payoff for cooperating.
2. The "all cooperate" payoff curve intersects the horizontal axis at a point equal to the magnitude of the social fine.

This intersection of "all cooperate" and the horizontal axis in Figure 3 is equivalent, in terms of Figure 1, to the configuration in which the left extremity of the defect curve is equal to the right extremity of the cooperate curve; the social fine is large enough to equal and

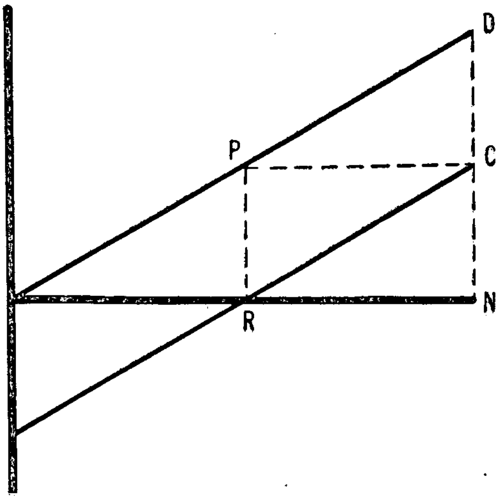

Figure 2. Threshold of Equilibrium Outcomes for Majority Rule

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[15] These parameters apply in all cases in which the social fine is equal to the magnitude of the payoff for defection, when there is equality in the distribution of payoffs for cooperation and defection, and when there is equality in the distribution of the external effects. Equality in the distribution of payoffs and of the external effects is a standard simplifying assumption of prisoners' dilemma models, but it would obviously require modification if the model were to fit a great variety of real-world situations. One of the best-known games of politics is to reap the benefits of defection while passing the costs on to someone else. In the extreme case, the costs can be borne entirely by the cooperators, meaning that, as there are fewer and fewer cooperators, the costs they must bear grow exponentially. The distribution of political resources in the population will have something to do with the probability of this happening, and it is worth remembering that our simple model assumes that such resources (in our game only the vote) are distributed equally among our nine players. Perhaps the most intractable prisoners' dilemmas are those in which it is possible to locate the costs of defection in the future and to enjoy the benefits of it now; people living in the future have no political resources with which to influence current decisions. From our model, this will most probably influence the *constitutional* decisions by those living in the present. If they are able to avoid all but a small proportion of the external effects (or, in the extreme case, all of them) they will very likely find themselves facing a non-prisoners' dilemma configuration and will opt, being selfish maximizers, to make their decisions through individualistic mechanisms. This will produce the best possible outcomes from *their* point of view—but the worst possible outcomes from the point of view of people living in the future.

negate the gains that can be made from defection. Thus, we can say further:

3. All points on the horizontal axis to the left of the intersection of that axis and the "all cooperate" payoff curve constitute prisoners' dilemmas; all points to the right of that do not.

We can now locate the payoff curves indicating the total social product when varying proportions of the population cooperate. As will be seen from Figure 3, these all intersect the horizontal axis at the same point as the "all cooperate" payoff curve (where the social fine is equal to the opportunity costs of cooperation). At that point the proportion cooperating and defecting makes no difference to the total social product.

When the opportunity costs of cooperation are less than half the magnitude of the social fine, majoritarian democracy is clearly the most effective mechanism for ensuring that a society achieves the largest possible social product. For those cases selfish dictatorship can be expected to produce a marginally suboptimal product and uncoordinated individualism can be expected to produce the worst possible product. Granted that the majority has coercive capacity sufficient to make its decisions stick, there appears to be no case here for abandoning social decision making to dictatorship; the problem is more to ensure that majority rule is substituted for uncoordinated individualism.

When the opportunity costs of cooperation are more than half the magnitude of the social fine but less than the total magnitude of that fine, majoritarian democracy has no equilibrium solution—and any outcome is possible. In this zone dictatorship continues to produce marginally suboptimal solutions and uncoordinated individualism continues to produce the worst possible product. However, it is here that arguments for dictatorship (such as the one discussed at the beginning) have some plausibility, since majority rule is relatively less attractive. But we should remember that even here the latter is at least *capable* of producing the optimal social product while dictatorship is not.

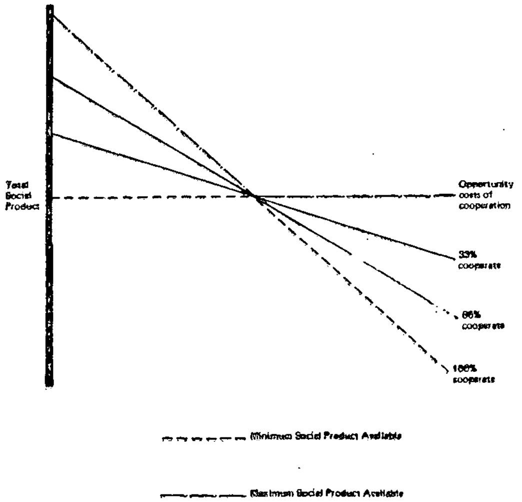

Figure 3. Generalized N-Prisoners' Dilemma

When the opportunity costs of cooperation are greater than the magnitude of the social fine, the outcome is the same under majority rule: anything is possible. Dictatorship once again produces an equilibrium outcome, but this time the "one defect" solution is just short of the worst possible for the society. For these configurations it is clear that the mechanism so highly regarded by Adam Smith, uncoordinated individualism, is superior. Democracy is *capable* of doing as well, but the equilibrium solution provided by individualism (coupled with the undoubted costs of democratic decision making) makes the choice an obvious one.

Information about the nature of the configuration facing a society is obviously critical to the constitutional problem. Without solid knowledge about the empirical character of such configurations, advocacy of particular constitutional mechanisms is likely to be based upon simple ideological assertion at best, and to be cynically self-serving at worst. But, as Mancur Olson (1975) has pointed out in the context of environmental decision making, mistakes are easy to make.[16] From Figure 3 what can we say about the probability of mistakes and their likely consequences, should we relax our assumption of perfect information?

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[16] The distinguishing characteristic of much environmental cost-benefit analysis (says Olson) is not the shortcomings of that method but the extremely large uncertainty terms that must be incorporated into it. The reason for such uncertainty terms, he says, is the great complexity of environmental systems that makes it extremely difficult to identify all the far-reaching consequences of individual or collective action. For a somewhat different argument that nevertheless makes a similar point about the need for information and the problem of getting it, see George J. Stigler (1975). The importance of information, notice, means that an opportunity exists for individuals who have control over the means of collecting it (or perhaps the means of making it generally known) to distort their findings to their own benefit. As we have seen, in each matrix there are situations where individuals stand to make substantial gains (both in absolute and relative terms) and we can expect them to exert whatever efforts of which they are capable to end up occupying preferred

Constitution builders need only worry about some mistakes—those that will lead them to design institutions which are not appropriate to the real nature of their social and physical environment. Such mistakes would be possible around the "threshold of indeterminacy" for majority rule that we have identified, leading constitution builders to advocate dictatorship when they should advocate majority rule, or the reverse. But, as we have shown, choice between these two institutions is never unambiguous in terms of the social product.

More serious mistakes would appear possible around the point at which the various social payoff functions converge in Figure 3. To the immediate left of that point, configurations are prisoners' dilemma requiring "all cooperate" and to the immediate right they are non-prisoners' dilemma requiring "all defect." Wrong information about the state of nature in this zone could lead to institutions designed to produce exactly the wrong kind of collective behavior.

But, as we have also shown, the social products produced by our alternative institutions converge at this point so that, in its immediate vicinity, there is not much to choose among them. Accordingly, the consequences of such an error in constitution building would be, at worst, relatively slight—they would be "critical—but not serious." In fact, *at* this point the society would be quite indifferent between uncoordinated individualism, selfish dictatorship and majority rule since the three mechanisms would produce exactly the same social products. In general, we can say that *the probability of error in constitution building is negatively correlated with the likely damage that can be done by such an error.* In this day of dire environmental predictions, it is, perhaps, comforting to know that we have at least some small measure of leeway for mistakes in the way we organize our lives.

In this article we have looked for solutions operating from the standard assumption of choice models that individuals are motivated by self-interest. It is the conflict between individual self-interest and the collective interest that is the essence of the prisoners' dilemma, but if there are circumstances under which altruism (motivation to maximize the collective good rather than the individual's own good) can be relied on—if we can relax our present assumption about motivation—the arguments for collective organization must be, to some extent, qualified. What can be said from our model about this?

We have defined the horizontal axis in Figure 3 as the opportunity costs of cooperating action, but it could equally be understood as measuring the "price of altruism." If a configuration is prisoners' dilemma, action in the best interests of the community involves the likelihood of loss either from being a "sucker" or from passing up the chance at a "free ride." But those losses are least toward the left of the horizontal axis and increase toward the right—meaning, in standard economic terms, that the incidence of altruism is likely to be less toward the right.[17] Consistent with the observations just made, we can notice that the social benefits available from altruism in a prisoners' dilemma configuration decline as the costs of altruism increase—up to the point at which configurations cease being prisoners' dilemma and altruism becomes a socially useless virtue.

But this decline in the probable incidence of altruism parallels the decline (that we identified earlier) in the social efficiency of majority rule. In many parts of the world, perhaps most, majoritarian democracy and altruistic behavior are both regarded as positive values, but we are left with the final conclusion that where majority rule is an effective mechanism for the satisfactory solution of prisoners' dilemmas, altruism is also relatively available; where majority rule breaks down, altruism is less available as an alternative.

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positions. The control over science and the media are obviously important resources in this respect.

[17] Richard Titmuss (1970) has developed a powerful argument that blood can be provided for a society more efficiently (less expensively and with greater certainty of quality) by relying on such altruism, and from this case he suggests that altruism is available as an organizing principle at least more than is often believed. It is well to remember, however, that the cost of altruism in blood is relatively slight to the donor—who has an infinitely renewable supply. See also Frohlich (1974), Boulding (1973), and the generally excellent papers contained in Phelps (1975).

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Mentoring in Political Life: The Case of Willy Brandt *

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Mentoring relationships have a qualitative impact on the transition from early to middle adulthood, as well as on adult life itself. This paper proposes that the exploration of the role of mentors in the lives of political leaders will produce suggestive results. The study is a case history which examines the mentor in the early and later life of the adult male, here Willy Brandt. Brandt had two mentors: Julius Leber and Ernst Reuter. Reuter is briefly discussed but we focus here on the first of these, Leber, a prominent Social Democrat during the Weimar era and a legendary member of the German resistance. The essay describes the origins, nature and termination of the Brandt-Leber relationship. The influence of the mentor on the mentee transcended the period of their actual interaction and, as the paper procedes to demonstrate, the intensity of the relationship ensured that Brandt's self-image as Leber's heir continued to affect both professional and ideological choices.

Academics continue to explore the processes of and influences on childhood and adolescence.[1] That granted, two reminders are in order: first, the boundaries between childhood and adolescence and between adolescence and early adulthood remain ill-defined. Second, it is now generally acknowledged that socialization continues to effect attitudinal and behavioral changes throughout the entire life span.[2] Indeed, it is often only by examining personality attributes as they evolve during adulthood, that we can accurately gauge what was central during the years which came before.

Until recently there has been no conception of adult development which permitted us to view the cumulative effects of passing through multiple adult eras. Developmental psychologists have stressed the preadult years. Even Erik Erikson, who has constructed a view of the life cycle and its component ego stages from birth to old age, has placed his greatest emphasis on the stages before early adulthood.[3] This lacuna in adult developmental psychology is just now being attended to.[4]

Daniel J. Levinson has headed a long-term project which used interview material from a sample of 40 males to define the psychosocial development of men.[5] His attempt was to "formulate a social-psychological conception of the life course and the various developmental periods, tasks, structures, and processes within it" and, finally, to hypothesize on "relatively universal, genotypic, age-linked, adult developmental periods within which variations occur."[6] In short, Levinson sought to connect childhood and adolescence with (male) adulthood by seeing adulthood as a definable and integral part of the whole—a unit as specific and integrated as its two earlier counterparts.

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*My thanks are to Daniel Levinson.

[1] Lloyd de Mause has emerged as a new catalyst in this area. See his edited work, *The History of Childhood* (New York: Basic Books, 1974). He also serves as editor of *History of Childhood Quarterly*. (Vol. 1, No. 1 appeared in Summer, 1973).

[2] See for example, Orville Brim, Jr., and Stanton Wheeler, *Socialization After Childhood: Two Essays* (New York: Wiley, 1966).

[3] Erik Erikson, *Childhood and Society*, 2nd ed., rev. and enl. (New York: Norton, 1963), pp. 247–74.

[4] Two major researchers in this area are psychologist Daniel J. Levinson at Yale and psychiatrist Roger Gould of U.C.L.A. Gail Sheehy's enormous popular success with *Passages* confirms that adult development is a subject whose time has come.

[5] Daniel J. Levinson, Charlotte M. Darrow, Edward B. Klein, Maria H. Levinson, and Braxton McKee, *Toward a Conception of Adult Development*. My page numbers refer to an unpublished manuscript which Professor Levinson was kind enough to provide. Despite multiple authorship, this work will be referred to in the text as "Levinson." A revised version of the manuscript with Levinson as primary author will be published by Alfred Knopf in 1978 under the title, *The Seasons of a Man's Life*. Early in his research, Levinson felt certain that the psychosocial development of men differed from that of women, and that each sex would require a separate study. He very specifically, therefore, confined his own investigation to men. (The original impetus was his own experience.)

[6] Daniel J. Levinson, Charlotte M. Darrow, Edward B. Klein, Maria H. Levinson, and Braxton McKee, "The Psychosocial Development of Men in Early Adulthood and the Mid-Life Transition," in D. F. Ricks, A. Thomas and M. Roff, eds., *Life History Research in Psychopathology* (Minneapolis: University of Minnesota Press, 1974), pp. 3, 4 of manuscript. Despite multiple authorship, this work will be referred to in the text as "Levinson."

Mentors have, of course, always been points of reference. We have long had concepts such as father figure, role model, sponsor, etc. Now, however, because of the aforementioned study, the mentor is attracting fresh interest. It appears to be a quite common element in the life history of the normal male, usually found in late adolescence and early adulthood. Yet the mentor's impact persists long after the actual mentoring has ended. His role is, in short, a perfect example of an early aspect which has a shifting, but durable import. (I remind the reader that we are talking here—as Levinson did—of male development only. Other as yet inconclusive research indicates that females have had trouble attracting mentors. We might also note that, for many obvious reasons, very few males have had female mentors.)

To explicate the concept, I will quote directly from Levinson:

> The mentor is ordinarily 8–15 years older than the mentee. He is enough older to represent greater wisdom, authority and paternal qualities, but near enough in age and attitudes to be in some respects a peer or older brother, rather than in the image of the wise old man of distant father.... He takes the younger man under his wing and invites him into a new occupational world, or an occupation-relevant world. The mentor shows him around, imparts his wisdom, cares, sponsors, criticizes and bestows his blessing.... The younger man, in turn, feels appreciation, admiration, respect, gratitude, love and identification.[7]

Mentor relationships differ in character. The extent to which they influence subsequent choices varies according to the predominant mode of relating, the evolution of the relationship, and the nature of the termination process. I intend to demonstrate that the effects of the mentor on the mentee can be profound, and life-long. To explore the genesis of one such relationship along with the effects, let us examine the following case in point.

Willy Brandt, former Chancellor of West Germany, winner of the Nobel Peace Prize, and now still an active European statesman and Chairman of the Social Democratic Party of Germany is, by any measure, a political leader. We need not debate the impact of the actions of any single political leader. But whether or not one subscribes to the "hero in history" explication of political events, there can be no doubt that the private motives reflected in the public behavior of persons exercising political power are of greater consequence, and interest, than the private motives of ordinary citizens.[8]

A curiosity about the private Brandt neatly converges with the model of the mentor—for Brandt had two: Julius Leber and Ernst Reuter. I will concentrate on Brandt's relationship to Leber and its persistent effects, describing the origins, nature and termination of their interaction. Using this account as base, I will demonstrate that the impact of the mentor transcended the actual relationship and endured to affect Brandt's later professional choices, ideological positions, leadership style, and moral code. In short, Leber remained in Brandt's public life after they had separated, and even after Leber's death.

A caveat: this paper is but one case based on the model of the mentor. It is not intended to represent a typical case. Whether, indeed, there is such a thing as a "typical" mentor relationship is, of course, an open question—one which should be the subject of further empirical study. Here, however, we are not concerned with the representativeness of this case, but rather with illustrating that Brandt and his mentors represent an illuminating variant of the mentor relationship. At the very least, Levinson's model stimulates. The reader is thus encouraged to hypothesize about different leaders developing in different settings and, thereby, about the implications of mentors for political life in a broader sense.

The First Mentor

Although Brandt has been the subject of four recent biographies, it can be argued that he has been his own most revealing chronicler.[9]

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[7] Levinson et al., "Psychosocial Development," pp. 14, 15 of manuscript.

[8] The private motives to public behavior theme is derived from Harold Lasswell, *Psychopathology and Politics,* 2nd ed. (New York: Viking Press, 1960; originally published 1930).

[9] Willy Brandt, *My Road to Berlin* (New York: Doubleday, 1960). The four biographies are: David Binder, *The Other German: Willy Brandt's Life and Times* (Washington: New Republic, 1975); Viola Herms Drath, *Willy Brandt: Prisoner of his Past* (Radnor, Pa.: Chilton, 1975); Terence Prittie, *Willy Brandt: Portrait of a Statesman* (New York: Schocken, 1975); Carola Stern, *Willy Brandt* (Reinbeck: Rowohlt Taschenbuch Verlag, 1975). Note: reference to page numbers in this essay are from Brandt, *My Road* (London: Peter Davis, 1960) and

Thus, in piecing together his past, the analyst must make use of materials fashioned by Brandt himself. We have been warned not to rely uncritically on autobiographical statements.[10] Accordingly, when considering Brandt's autobiography, *My Road to Berlin,* the reader should bear in mind that it was published at a time when Brandt was politically ambitious and, as a result, professionally vulnerable. The book is, therefore, politically as well as personally motivated. Still, these materials best capture some of the mood and detail of Brandt's early life. And their authenticity is confirmed by the more objective recorders.

Brandt was born Herbert Ernst Karl Frahm in Lübeck in 1913. (His name was changed for political reasons circa 1933.) Recalling the earliest years, Brandt engages in the unusual stylistic device of separating the identity of the child, Herbert Frahm, from that of the man, Willy Brandt. Using the third person, Brandt writes:

> Of the boy Herbert . . . I have only a vague recollection. . . . The mother was very young. . . . He never knew his father . . . and he did not want to know. . . . He carried the name of the mother; the father was never mentioned at home.[11]

The brief section which sketches the first 14 years of Brandt's life continues in this mode. It is no accident: by creating a deliberate space between his adult self and the boy from the past, Brandt appears to be disinclined to contend with the deeper implications of his early history, or even to have that history subjected to close investigation by others.

Brandt's very early childhood was less than idyllic. His mother was forced to work and generally saw him only part of Saturday and Sunday. But in 1918, the pattern of his young life was dramatically altered; his grandfather returned home from the war. Ludwig Frahm quickly became the dominant adult in Brandt's life, and continued to fill that role until he was joined, about ten years later, by Brandt's first mentor.

Brandt has always spoken of his grandfather with admiration and warmth. In his description of their first meeting, Brandt included sight, smell, and touch to convey the pleasure of the union:

> When Herbert was four or five years old . . . the war appeared in the shape of a man in a muddy soldier's coat. The man smelled of sweat, wet leather, powder and oil. The odor fascinated Herbert, as did the rough cloth of the heavy coat, the leather belt, the long bayonet. He climbed quickly on the lap of the man whom he called "Papa." He let himself be kissed. . . . He liked the soldier from the beginning. From then on the three lived together and the little apartment became a home.[12]

Thereafter, home was where grandfather was. When he remarried, he took the boy with him—despite the less than loving relationship between his new wife and his grandson.[13]

It would be difficult to exaggerate the influence of Brandt's grandfather. No father could have had more. Along with the more amorphous moral pressures which encouraged Brandt in his political activism, there was Ludwig Frahm's own complete dedication to the principles and goals of the Social Democratic Party of Germany (SPD). Brandt recalls that "to grandfather socialism was more than a political program, it was rather a kind of religion."[14] But Brandt uses the concept of religion in the liberal sense, that is, not as a cause one follows blindly, but as system of values, theory and action which one finds, after diligent scrutiny, to be the best available—and which one then attempts to propagate.

Ludwig Frahm exerted more concrete influences as well. With the support of Brandt's mother who was also active in the SPD and in local trade unions, Brandt was constantly involved with a variety of SPD youth groups. He was also encouraged to seek a higher education, and at the age of 14 was admitted to the Johanneum, an elite high school of outstanding reputation.[15] It was during the Johanneum

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Prittie, *Willy Brandt* (Frankfurt: Goverts Krüger Stahlberg, 1973).

[10] See Erik Erikson, "On the Nature of Psycho-Historical Evidence: In Search of Gandhi" in *A Source Book for the Study of Personality and Politics,* ed. Fred Greenstein and Michael Lerner (Chicago: Markham, 1971), p. 123.

[11] Brandt, *My Road,* p. 19.

[12] Brandt, *My Road,* pp. 19–20.

[13] Brandt's real grandmother had paid no attention to him. Brandt's grandfather remarried not long after her death but his new wife, whom Brandt called "aunt," was not destined to become one of the boy's favorites.

[14] Brandt, *My Road,* p. 22.

[15] The credit for this decision is given to Brandt's grandfather by the neighbor who took care of Brandt during his early childhood years. (See Prittie, p. 25.) Nowhere does Brandt himself mention that his grand-

years that Brandt first came into contact with Julius Leber, contemporary socialist of considerable prestige, and future martyr of widespread reknown.

Leber was one of the very few Germans who stayed in Germany during the entire Nazi era, and maintained his integrity wholly and publicly intact. He was executed in early 1945 for his role in the July 1944 plot to assassinate Hitler. His place in German history is secure, as is his place in Brandt's personal history—although the precise nature of that second positioning has been a matter of some debate. Brandt himself wrote many years later: "I have grown up without a father; there was an emptiness in my life—Leber filled it."[16] The two had first met when Brandt was around fourteen, through his short pieces for the local Social Democratic newspaper, the *Volksboten,* of which Julius Leber was the editor. He was also a member of parliament and a highly respected figure in the Social Democratic Party. Brandt won a prize for his literary efforts but, far more importantly, he and his articles attracted Leber's attention.

Leber was born in 1891 in Alsace, then part of Germany. He grew up in modest circumstances but, like Brandt, his intellectual superiority enabled him to attend senior high school. He became a member of the SPD in 1913; volunteered for military service in 1914 where he became a commissioned officer and won a citation for bravery. After the war, he resigned his commission to earn a doctoral degree in political science, and at the age of 30 moved to Lübeck to take over as editor of the *Volksboten.*

Leber's presence in Lübeck was promptly and durably felt. "Within a few months Leber was more than a chief editor. He quickly became ... the unrivaled leader of the Social Democrats in Lübeck. [And] the Social Democrats had, in Lübeck, their strongest and most dependable organization in all of Germany."[17] Workers came to regard Leber "as one of their own." He was a progressive thinker, concerned with both political and moral issues, and singularly attuned to the younger generation. This concern must have made him particularly receptive to an adolescent such as Brandt who was full of promise, and whose history bore some obvious similarities to his own.

Brandt's response to Leber was characteristic of the fortuitous mesh. No doubt he perceived in Leber an eminence to be cultivated and even used, but clearly he was also vulnerable on an emotional level to Leber's sincere overtures. Significantly, Brandt's description of Leber is in a uniquely elaborate literary style: "He looked like a Latin with his sharply cut features, a high forehead, burning eyes, strong jaw and a soft sensual mouth.... [He] made one think of a Roman tribune."[18]

Between about 1928 and 1931 (Brandt's age 14–17), the relationship between Brandt and Leber was warm and constant. Leber helped Brandt in countless ways. He took him under his wing professionally, lending the weight of his prestige to the multiplicity of Brandt's journalistic and party activities.[19] In addition, he supplied a source of affective support during Brandt's Johanneum years—years during which Brandt was busy and successful, but still the outsider with regard to his upper-class peers.

Most observers have regarded the Brandt-Leber relationship as like that of father and son, no doubt taking their cue from Brandt himself. Brandt perceives Leber as having stepped into the void felt by the fatherless child—even though when he met Leber he was scarcely a child. He noted that Leber made him a "militant democrat and a liberal socialist," that is, he gave him his *Weltanschauung.*

There are, however, strong indications that the Leber-Brandt interaction is not most precisely characterized by the father-son analogy. As Brandt commented in 1960:

> Leber was to me more than a teacher or older friend. I would never have admitted that I lacked self-confidence, but secretly, I had my doubts. Leber dispelled them by giving me recognition and encouragements. His occasional praise meant all the more to me because he also never hesitated to criticize my youthful impetuousity. He did it with a slight amiable irony. He treated me as his equal.[20]

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father rendered specific help with regard to his schooling. He notes instead, that the scholarship he received was "in recognition of good marks" (Brandt, *My Road,* p. 24).

[16] Brandt, *My Road,* p. 31.

[17] Anonymous description from a book compiling some writings, speeches, and letters of Julius Leber: *Ein Mann geht seinen Weg: Schriften, Reden, und Briefe von Julius Leber* (Berlin: Mosaik Verlag, 1952), p. 271.

[18] Brandt, *My Road,* p. 29.

[19] For example, through the intervention of Julius Leber, who had also named Brandt a "contributing editor" to the *Volksboten,* Brandt was admitted to the Social Democratic Party at 16. More ordinary persons had to be at least 18 to gain regular party status.

[20] Brandt, *My Road,* p. 32.

The image is correct: to Brandt, Leber was both senior and peer. As senior he carped but also bestowed the all-important blessing. As peer he accompanied Brandt through the early professional maze without condescension. It was an extraordinarily important relationship at a time in Brandt's life when he needed confirmation from a like-minded superior, and friendship from a like-minded peer.

This balance Leber offered, the balance between senior *and* peer, is not characteristic of the father-son model. Also Brandt's grandfather (father) continued to be in Brandt's life until his death in 1935; concomitantly, there was an extreme paucity of contact between Leber and Brandt after 1931. On this last point, we should also note that although opportunities for communication between the fugitives Leber and Brandt were severely limited during the Nazi era, the fact that there was none confirms the absence of father/son feelings on both sides.

We return then, to the model of the mentor. Levinson indicates that the mentor may serve a variety of functions (e.g., teacher, guide, sponsor, etc.). There are also less concrete supports. The mentor may "foster the young man's development by believing in him ... by helping to create a space within which a newly emerging self can take shape and be given a place in the world. . . ." These words, coupled with the specifically developmental emphasis of the following quote, testify to the utility of the mentor model as applied to the Leber-Brandt interaction:

> The mentor-mentee relationship, at its best, serves a developmental function: it facilitates the young man's transition from boyhood to adulthood, and by its nature it contains elements of both. . . . The mentor is usually symbolized by the mentee as something between a father and age peer. The relationship initially contains a good deal of deference and admiration on the part of the younger man but it tends over time to become more equal and mutual. The mentee initially experiences himself as the novice or apprentice, as the boyish beginner in relation to the authoritative adult, but he gradually gains a fuller sense of his own authority and of his capability for autonomous action.[21]

A final point: Leber was Brandt's senior by 22 years. Although this age difference is atypically large for a mentoring relationship, it does not preclude it. Indeed, Levinson alludes to just this possibility when he discusses the not infrequent fit between "especially youthful mentors and mature mentees." Again we envision Leber and Brandt; in sum, the mentor model fits.

The Termination

Along with many others, Brandt regards 1930 as a turning point: the Social Democrats left the Federal Government, and the Nazis became the second strongest party. The political climate of the years just before Hitler became Chancellor was taut with the expectation of change—and an extreme range of opinion on how that change should and would be manifested. Brandt, of course, responded to these events out of his own experience. Both Leber and Brandt were equally fervently anti-Nazi. But while Leber's reaction was to call for an ardent recommitment to Social Democracy, Brandt was beginning to manifest a restless impatience with the impotence of the SPD in face of the Nazi threat.[22]

During this uncertain period, a small group on the left wing of the SPD began to agitate seriously for the new party. Dissatisfied with the SPD stance vis-à-vis the Nazis and with the party's moderate economic program, the rebels formed a new leftist party in October, 1931. They were led by two SPD dissidents: Max Seydewitz and Kurt Rosenfeld. The new Socialist Workers Party (SAP),[23] although founded on some dubious assumptions, initially attracted considerable numbers from the SPD. It had a particular appeal for the SPD youth, which had launched a vociferous attack on the lassitude of party elders. Early hopes for the SPD were high. Rosenfeld articulated the optimism shared by the founding members: "Our heads are rolling. But they are not rolling in the sand. They continue to think and work: for the creation of a revolutionary class party which will build the foundation for the unity of the working class."[24]

Just as Leber was stepping up both his party and editorial activity on behalf of the SPD,[25] Brandt, age 17, quit the party to join the new SAP. Brandt's decision was not unusual, but precisely because his experience had been so conducive to a precocious formulation of a political ideology—Social Democracy—his decision to take up with the new party must be seen as something more than "the thing to do." Brandt's decision to leave the SPD and to break with his grandfather and Leber must be viewed as a major choice not only professionally and ideologically, but also personally.

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[21] This quote and the shorter ones incorporated in this paragraph are from Levinson et al., *Toward a Conception,* pp. 14, 15 of Ch. 10 of unpublished manuscript.

[22] Brandt recalls: "We fought them [Hitler youth] with words and fists" (*My Road,* p. 32).

[23] For a thorough investigation of the SAP's *legal* history see Hanno Drechsler, *Die Sozialistische Arbeiterpartei Deutschlands (SAPD): Ein Beitrag zur Geschichte der deutschen Arbeiterbewegung am Ende der Weimarer Republik* (Meisenheim am Glan: Verlag Anton Hein, 1965).

[24] Drechsler, p. 99.

As Brandt freely relates, his defection to the SAP caused both his grandfather and Leber genuine distress.[26] But it is Leber's response to which Brandt devotes the most space in his autobiography. It was not, of course, an easy break. The ties between them had been too close, the shared commitment too great. Brandt had alluded to the freedom Leber always gave him, leeway in which he might develop his own views.[27] But when he declared his intention to leave the SPD for the upstart SAP, the mentor was angry and disappointed:

> In Lübeck only a small number of older Social Democrats but many young comrades, joined the new party. Leber was trying to prevent me from following them. Was I completely mad, he asked. This time he lost his superior calm.... The new party was an association of cripples he asserted. "They wanted to be revolutionary? They were impotents who, conscious of their physical and intellectual incapacities, escaped into radicalism. In spite of your youth, you can appreciate a good book, a good drink, the favors of a beautiful girl. You are quite normal, you don't belong with that band of sectarians." I thought Leber unjust and protested more violently than I had ever dared before. We separated in bitterness.[28]

One of the other editors of the *Volksboten* remembered how amazed everyone was at the "courage" Brandt demonstrated in daring to criticize Leber.[29] The reaction was, perhaps, most comprehensible in terms of money and career: Brandt could no longer work on the *Volksboten,* and Leber's promise to help finance his university studies now had to be dismissed. The rupture with Leber was only one of the prices Brandt paid for his political choice. Indeed, the total cost of Brandt's new membership in the SAP was so high, that he must have anticipated a considerable compensation. (I might note tangentially, that it was realized.)

The Severing episode in July, 1932, made it clear that Brandt had little need to pursue an open victory over his grandfather, Leber, or the SPD. It was then that Brandt began to overtly distinguish person from party, dogma from cause. The Chancellor, Franz von Papen, had openly violated the constitution by the device of broad emergency decrees and his dismissal of the Prussian government. Carl Severing, Minister of the Interior, was the last hope of the anti-Nazi forces. They anticipated that he would summon the Prussian police to take a bold initiative against the Nazis and, together with Social Democrats (and probably the Communists and the trade unions), establish a belated bulwark against Hitler. With Severing's refusal to fight, Brandt writes, "the backbone of the opposition to Hitler was broken." On July 20, 1932, when Severing's capitulation became clear, Brandt addressed a public meeting called by the socialist workers in Lübeck. He shared the platform with Julius Leber—with whom he had broken so traumatically less than one year earlier.

Later, Brandt describes the night:

> And then we of the Socialist Workers Party saw that ... we could count on Leber as on no one else.... We arranged a mass meeting at which Julius Leber proclaimed the order of the hour: "Freedom will be with you as long as you fight for it.... We shall fight to the end...." Suddenly everyone was on his feet. Men and women, their clenched fists raised, shouted themselves hoarse. My heart stopped and then began to beat madly. I felt it in my throat. I wanted to jump on the platform, to squeeze Leber's hand. I had found him again. I had never lost him. At this moment the Nazi SA stormed into the hall.... It was Leber whom they wanted. Suddenly he stood alone and unprotected.... Then he grabbed a chair, smashed it to pieces, and using one of its legs as a weapon, he fought himself free.[30]

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[25] Editorial evidence available in *Volksboten* of July 19, 1930; October 20, 1930; June 24, 1931; and October 16, 1931. Reprinted in Leber.

[26] When Brandt participated in a 1931 incident in which protesters carried a banner proclaiming, "The Republic isn't much—Socialism is the goal," his grandfather responded in dismay, "How could all of you be so ungrateful?" Brandt told this anecdote during an interview with Günther Gaus shown on German Channel 2 on September 25, 1964, and transcribed in *Tatsachen-Argumente* (SPD Publication), 104 Bonn, October, 1964, p. 4.

[27] See, for example, Brandt, *My Road,* p. 35.

[28] Brandt, *My Road,* p. 39.

[29] Prittie, p. 29.

[30] Brandt, *My Road,* pp. 43, 44.

On that July night Brandt related, he "found" Leber again. The episode put a brake on his flight from everything connected to the SPD, and particularly on his need to triumph over symbols of earlier allegiances. Still, there is no evidence of a wholehearted reconciliation during the last months of 1932. Brandt continued to put his energies into the SAP, while on October 22, Leber wrote, "Hitler today is actually only the big fashion of yesterday. . . . Today Hitler is no longer acceptable, he is no longer elegant, he went overnight–out of fashion."[31] Only after the fact, on January 30, 1933, did Leber completely understand. As he himself then acknowledged: "Now everything is clear: Hitler Chancellor–Papen Vice-Chancellor. . . . Monstrous are the dangers. . . . But we are decided to enter the battle."[32]

Despite the continuing gap between them, when Brandt recalled the historic hours of January 30, 1933, he again did so primarily in terms of Leber's personal experience:

> That night of the 30th of January, a few hours after the radio had announced Hitler's appointment as Chancellor, Julius Leber was arrested. On his way home . . . he was held up by a band of Storm Troopers. . . . Leber was severely injured . . . [and] was imprisoned. . . . In the next two weeks I hardly went to bed. . . . A one-hour protest strike for Leber's release was carried out with impressive discipline. . . . At the intervention of his wife, Leber was allowed bail. . . . He was allowed to attend the meeting under the condition that he would not make a speech. When he appeared on the platform [on February 19th] with a bandaged head, unbroken, unbent, he shouted only a single word: "Freedom!" The people took this shout for more than a demand–it was a vow. . . . This was not the last of our illusions. Actually, this was the last free demonstration in Lübeck. It was also the last time I saw Leber.[33]

In his autobiography, Brandt uses Leber as the major focal point for his account of the catastrophic events between July, 1932 and February, 1933. This suggests that Leber continued to play an important psychological role in Brandt's emotional life, despite their split. Objective events often left their main imprint on Brandt via the effect they had on Leber.

To better understand this lingering impact, we need to consider the way mentoring relationships end. Levinson suggests that the mentee often comes to regard the person who was formerly so "loved and admired, and who was seen as giving so much," as "hypocritical, oppressively controlling, seeking to make one over in his own image rather than fostering one's independence and individuality. . . ."[34] Clearly, Brandt felt Leber to be guilty of some of these suppressive traits and, in addition, events did not prove Leber to be particularly prescient.

There is another, less conscious process at work. The break with the mentor also reflects the usually latent hostility to the "senior," the father. (This may have been complicated and/or exaggerated in Brandt's case where the real father was unknown.) The split with the senior, therefore, entails guilt–which is compounded by the interaction as friends and colleagues.

But these negative legacies, resentment and guilt, have of course countervailing positive ones which finally induce the internalization of many of the (good) mentor's most admired qualities. The benefits accrued from the nurturing win out; in this case, despite the period of political and personal antagonisms, Brandt discerned Leber's immense personal courage and moral rectitude–even as an 18-year-old, self-styled radical trying, developmentally, to leave the "family." And Brandt's guilt was eventually transformed into a sense of mission by Leber's grim fate under the Nazis, most dramatically his execution when Brandt was 31. The nature and degree of the mentor's lingering impact is determined by both the quality of the original interaction and by the way in which it was terminated. Here, the impact of these is sealed by the extraordinarily tragic circumstances of Leber's death. Small wonder that this mentor left a permanent mark which is confirmed by Brandt's words and work to the present day.

Assuming the Legacy

Brandt went into exile in Norway in 1933 at age 19; he fled from Norway to Sweden in 1940. At the end of the war, he was faced with the very real dilemma of choosing between a secure and highly creditable career in Norway, or a more uncertain if potentially more promising future in postwar Germany. The period of decision and transition–from Scandinavia back to Germany–took two and one-half years (1945–48).

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[31] Appeared in *Volksboten*, October 22, 1932, and reprinted in Leber.

[32] *Volksboten*, January 30, 1933, and reprinted in Leber.

[33] Brandt, *My Road*, pp. 46, 47.

[34] Levinson et al., "Psychosocial Development," p. 16 of manuscript. The ambivalence manifested at the time of termination never dissipates completely. Recently, Brandt professed to be "bored" talking about Leber and Reuter. (Drath, p. 350.)

Brandt's decision to return to his homeland was not part of a general pattern. On the contrary: it is estimated that of those Germans who emigrated to Sweden, only about 30 percent returned.[35] Why then did he decide to go home again? Brandt has analyzed this choice himself, and, despite his occational avowals that the question of returning to his homeland was an almost agonizing dilemma, when he wrote about it in his autobiography, he stated that there was "an event" which "clinched matters." To Brandt, the July 20th (1944) attempt on Hitler's life, in which Leber played a major part, was not just a political event, it was a personal trauma.

Brandt has claimed that his real contact "with outstanding people of the inner German resistance" came through Theodor Steltzer, who was attached to the German General's staff in Oslo. He belonged to the "Kreisauer Kreis," a resistance circle comprised of members of the German elite. Brandt writes that Steltzer "gave me insight into the thoughts and plans of the leaders of the opposition. . . . This was the first time that I heard again about Julius Leber and the important position he held among the conspirators in Berlin. I was very thankful to Steltzer for his information and asked him to give Leber my regards and to tell him about my work."[36]

These last words have a deceptively casual ring. On matters relating to the July 20th plot, Brandt was, in fact, one of the three best-informed persons among the German emigre colony in Sweden.[37] And through the connection to Leber, Brandt became more than just a politically interested observer. In June, 1944, Brandt was visited by an intermediary who, Brandt wrote, brought "greetings as well as an urgent request from my former mentor."[38] Brandt's use of the term "mentor" here is cogent and prepares the reader of his chapter–as does the title, "Leber's Last Message"–for the forthcoming emphasis, once again, on Leber. Here, as with the events relating to Hitler's takeover, the episode is told in terms of the man. Although Brandt had not seen Leber for ten years, and their contact during this period was minimal, Leber maintained a strong emotional hold over Brandt. The fact that the July plot occurred when Brandt was more alone than at most other periods during this ten-year span, also contributed to his sensitive response to Leber's reentry into his day-to-day consciousness.

Brandt recalls that in the beginning he was skeptical about the assassination plot. But he became gradually convinced of the merit of the attempt, even if it failed. Leber's experience dictates the narrative thread:

> Leber had spent four years in different prisons and concentration camps when he was finally released in 1937. He was still closely watched . . . but this did not prevent him from assuming a leading part in the resistance movement. . . . The Gestapo had not been able to break him. . . . On the 4th of July, Leber was arrested! . . . The tortures Leber and his friends had to endure were horrible, the martyrdom of their execution was the more tragic as it was utterly senseless. . . . The example of his deed and of his end spoke to me in the course of the next months with ever growing clarity and urgency. This message of the dead man, more than anything else, prompted me to dedicate my life to the reconstruction of a new Germany. . . .[39]

For Brandt, the failure of the July 20 revolt was not the end of the affair. What may well have been a lingering need to rework the episode in his own mind and/or an impulse to partake in it, to not desert Leber a second time, manifested itself in intellectual enterprise. In 1944, together with Stefan Szende, Brandt (anonymously) published a book about the episode, *Misslyckad revolt* (Failed Revolt).[40]

The precise frequency and nature of the contacts between Leber and Brandt after 1933 remain unclear. In the case of *Misslyckad revolt,* which Szende actually wrote while Brandt provided the materials, the question of sources became a subject of heated and even legal debate. Brandt listed Leber as a separate source, although as he reports in another place, he had contact with Leber only through intermediaries. It is not vital to this enterprise to list the exact times and modes of contact between the two men between 1933 and 1945—if any. It *is* important to reiterate that what happened to Leber—especially his activities during 1944 and the price paid—"clinched" major choices.

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[35] Helmut Müssener, *Exil in Schweden: Politische und Kulturelle Emigration nach 1933* (München: Carl Hanser Verlag, 1974), p. 99.

[36] Müssener, p. 128. Further comments on this subject by Brandt may be found in Willy Brandt, *In Exile: Essays, Reflections and Letters 1933–47* (Philadelphia: University of Pennsylvania Press, 1971), p. 62.

[37] The other two were Fritz Tarnow, a Social Democrat and close working colleague of Brandt's and Hans Schäffer, a former Staff Secretary in the Ministry of Finance.

[38] The request concerned Brandt's willingness to serve in a new (post-Hitler) German government.

[39] Brandt, *My Road,* pp. 129–36.

[40] The publication of *Misslyckad revolt* caused a small scandal and legal difficulties about sources. See Müssner, pp. 252–53.

Partly out of opportunism (Leber is an esteemed martyr), partly out of personal impulse, Brandt has continued to eulogize Leber in countless speeches and writings. Note the following samples, each from a different decade.

In 1952 Brandt wrote:

> Julius Leber knew how to convince.... One always saw Leber at the head.... Leber never demanded of his colleagues that which he wasn't prepared to risk himself.... Some of us youths then went into opposition against Leber.... Our admiration for Leber's person did not suffer thereby....[41]

And in 1960:

> Leber had the confidence [of the workers] for they felt rightly: here was a man who knew what he wanted and wanted it passionately; here was a man who never compromised, not a fanatic, but a real fighter.... He reproached the leaders with stifling the creative energies of the youth.... With equal determination he fought against the radicals who in pursuing their socialistic goals declared democracy ... not worth defending....[42]

And in 1970:

> He was a wonderful man.... He was a modern man.... He would belong to what one would call the right wing of his party but he was a right winger of a very activist mind; people liked him, they even loved him, and I was very much impressed by his activism.[43]

In retrospect, it seems clear that Brandt's reentry into the SPD from the SAP in the autumn of 1944 was actually a harbinger of his decision to return to Germany. There were multiple reasons for Brandt's reacquired link to the Social Democrats; his family history, of course, ranks high among them. But at *that moment in time* it was the mentor who was the catalyst. Leber had remained among the most esteemed Social Democrats. Even near the end, Leber's primary concern was the future of the party. The durability and intensity of his commitment to the SPD—as career and idea—was an aspect of his person which Brandt gradually but completely internalized.[44] Brandt's reentry into the SPD, his initial return to Germany, and his drift to politics as profession give witness that Leber's influence persisted, and even grew.

Ernst Reuter

Although this article is focused on Leber, a brief reference to Brandt's second major mentor, Ernst Reuter, acquired some two years after Leber's death, is in order. Indeed, Brandt himself comments on the connection between the two mentors and the roles they played in his life.

If Brandt did not exactly require a mentor during his first years back in Germany, there can be little doubt that he craved one. During 1947, Brandt was stationed in Berlin with the rank of Norwegian major, ostensibly to report to the Norwegian government on German affairs. On a personal level, he was clearly exploring the possibility of again living and working as a German. When Reuter entered his life, mutual admiration and affection were immediate. Reuter's own example gave prompt evidence that honorable men were returning to cast their lot with Germany (he spent the war in exile in Turkey). The ease with which Brandt was now able to justify, to himself, a reentry into the German political mainstream, was that much greater.

Prophetically, Brandt first met Reuter at the home of Annedore Leber (Julius' widow), "an oasis of kindness and warmheartedness ... amidst an icy desert of destruction."[45] As was the case with Leber, the two men quickly came to have an important relationship, one officially defined by professional contact, but transcending work into personal friendship. To Reuter, Brandt would become not only a practical heir apparent, but also the chief propagator of his political thought. For Brandt, Reuter was that almost indispensible commodity: a good (and esteemed) man who believed in him.

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[41] In Leber, p. 272.

[42] Brandt, *My Road,* p. 30.

[43] Willy Brandt, from a conversation with Lord Chalfont for the BBC television documentary, "A Man Called Willy Brandt." The show was transmitted on BBC-1, and was filmed in Bonn on September 23–24, 1970; p. 17 of transcript.

[44] Brandt's dedication to Social Democracy has not flagged. For example, no prominent socialist has played a greater role than he in rendering party (SPD) support and financial aid to the Portuguese Socialists during their recent crises. See, *New York Times,* August 29, 1975 and September 6, 1975.

[45] Brandt, *My Road,* p. 163.

As he had with Leber, Brandt spent his eloquence on the image of the man:

> It was not by chance that Reuter was the center of our circle. The heavy man with his big skull gave one the impression of an old tree which had braved many storms, and under which one would like to seek protection, and rest. He looked tired, his hair was thin, his face lined with deep wrinkles, but his eyes were young, and a half-wise, half-roguish smile was constantly dancing in them.[46]

Even as early as 1947 Brandt may have sensed the role similarity (for him) between Leber and Reuter. Certainly, he felt a sincere admiration and deep affection for both. The autobiographer establishes the congruence:

> Undoubtedly, I did not always share Reuter's views of Germany and the political tasks of the day. But from the first moment I felt that Reuter and I understood each other very well. When I was a young man I never felt that the difference of twenty years in our age alienated me from Julius Leber; that difference did not matter in my relationship with Ernst Reuter either.[47]

In 1948 Reuter became Governing Mayor of Berlin. Between 1947 and 1953, when he died, he became a true mentor to Brandt. As with Leber, the difference in age between mentor and mentee was greater than usual. But Brandt specifically stated that this age difference was unimportant. Like Leber, Reuter fits Levinson's definition of the mentor as being "near enough in age or attitudes to be in some respects a peer or older brother rather than in the image of the wise old man or distant father."[48] Also like Leber, Reuter "took the younger man under his wing, invited him into a new occupational world, showed him around, imparted his wisdom, cared, sponsored, criticized and bestowed his blessing."[49]

As early as 1947 Brandt's interaction with Reuter proved decisive. For the rootless young man it supplied necessary political, moral and affective links to Berlin, to the postwar SPD, to Germany. If Reuter cast his lot there, Brandt could, quite comfortably, do the same. (Brandt was, we might remind ourselves, still only 33 years old.) By the end of 1947, although the relationship was still young, it can fairly be said that Reuter played a substantial role in inducing Brandt to plant both feet squarely in Germany—as a politician. Reuter finished what Leber had begun.

This time the mentor relationship was ended by death. If, as Levinson claims, "the final giving up of all mentors . . . tends to occur in the middle or late thirties," Reuter's death merely terminated by natural means an exchange which had already served its primary function. Indeed, the impact of this relationship was almost certainly enhanced by Reuter's sudden and premature death which prohibited a deterioration in the interaction, or even a gradual devaluation. Reuter's role in Brandt's life was considerable. It has been noted that in mentor relationships, the blessing is the crucial element. Here, that blessing, from someone Brandt learned to love and respect, and with whom he identified, played an essential role in enabling him to withstand the more searing aspects of his early political life in Berlin. And, because in this case the separation was caused by death, the subsequent process of internalization was again exaggerated.[50] It is noteworthy that in 1956, Brandt gained access to Reuter's private papers and, together with Richard Löwenthal, wrote his mentor's life story in a 758-page biography.[51]

When mentees finally reject the intimacy of the mentor relationship, it is a signal that they are ready to become fully adult, ready to "move towards assuming . . . the functions of mentor, father and peer in relation to other adults."[52] This tends to take place at around 40. The year of Reuter's death (1953), was also the year Brandt turned 40.

Mentoring in Political Life

In Brandt's case, the interactions and aftereffects of mentor relationships were particularly intense. It is impossible to avoid the conjecture that although these relationships were not of a father-son nature, their frequency and importance resulted in part from the absence of a father. Indeed, Brandt had still other older men in his life who played mentoring roles of lesser significance.[53] (Mentoring varies qualitatively as does, for example, parenting.) There is no doubt that he actively sought out older men who were in a position to offer emotional sustenance *along with* professional help. What is quite remarkable is that in each new situation he was able to find just such a person, who then freely and willingly fostered his personal and professional growth.

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[46] Brandt, *My Road*, p. 163.

[47] Brandt, *My Road*, p. 167.

[48] Levinson et al., "Psychosocial Development," p. 14 of manuscript.

[49] Levinson et al., "Psychosocial Development," p. 15.

[50] Levinson et al., *Toward a Conception*, Ch. 12, p. 14 of manuscript.

[51] Willy Brandt and Richard Löwenthal, *Ernst Reuter: Ein Leben für die Freiheit: Eine Politische Biographie* (München: Kindler, 1957). Perhaps it should also be noted here that Brandt did some literary collaborating with Annedore Leber.

[52] Levinson et al., "Psychosocial Development," p. 17 of manuscript.

Still, the quantity and quality of mentors in Brnadt's life were determined only fractionally by the missing male parent. For while lacking a real father, Brandt had an extraordinarily devoted substitute in his grandfather. From a very early age Brandt lived in the same house with his grandfather and it was he, not Brandt's mother, who assumed the major responsibility for rearing him. (Brandt's mother married in 1927, setting up a new household in which young Herbert was not included.) Brandt did not lack a male model. Indeed, Leber, the first mentor, supplemented Brandt's grandfather; he did not replace him.

What Brandt apparently craved was support of a different kind. Mentors are, by definition, from outside the family. The lack of familial ties connotes that the mentor's interest in the mentee derives from qualities inherent in the mentee's own person. The mentor, in short, confirms the mentee in a way that a father—with his vested interest—cannot. In Brandt's case, there was an additional impetus: he had always been an outsider of sorts (illegitimate child, working-class student in an elite school, member of avant-garde splinter party, German in Scandinavian exile, etc.), a marginal figure. Mentoring provided an interaction which allowed Brandt to become half of a central relationship, an insider in a small but important world. And the element of the blessing, bestowed as it was by an eminence of sorts, facilitated entry into the larger world. Mentors enabled Brandt to be "in"—*in* a relationship and *in* the establishment. Leber gave him both himself and a hearty acceptance into the SPD; with Reuter, Brandt had Reuter as friend, and as guide to smooth the readmission into German political life.

Lest we lose sight of person in this brief emphasis on process, perhaps we should explicitly state the already obvious: Brandt's mentors were not ordinary men. Both Leber and Reuter continue to symbolize morality in political leadership in twentieth-century Germany. Anyone who would claim, even tacitly, to be their heir, would have to acknowledge this legacy and try to assume it as their own. And there can be no doubt that Brandt regards himself as their inheritor, an image which has affected his professional choices, ideological stance, leadership style, and moral code.

How did the mentoring in Brandt's history enter into his political life? To answer that question we need only look at the readily available data. The way in which a life is lived can be viewed as the result of a series of choices, not necessarily consciously made. Here we are especially concerned with professional and ideological choices. To a striking degree, the major decisions Brandt made as a young adult (and middle adult) follow the examples of Leber and Reuter.

Brandt's important professional choices may be listed as follows: (1) Although Brandt shocked Leber by joining the SAP, he did not, conspicuously, rebel totally by joining the much more despised KP (Communist Party). (2) Even after joining the SAP, Brandt made numerous efforts alongside Leber to declare a solidarity with the more general anti-Nazi goals of the SPD. (3) The years of his Norwegian exile (1933–40) were marked by Brandt's drift within the SAP to a more centrist (rightist) party stance. Almost without exception, his Norwegian friends were active in the moderate wing of socialist politics. (4) During his time in Sweden (1940–45) Brandt was increasingly attentive to the fate of postwar Germany.[54] He reentered the SPD in spite of a less than effusive welcome.[55] (5) He decided to return to Germany—a choice made the harder for his wealth of professional opportunities in Norway. (6) Brandt selected politics as his vocation, a decision dramatized by his first job in Germany as SPD functionary. (7) Despite the more quiet allure of other opportunities, he decided to enter electoral politics in the harsh political atmosphere of Berlin. (8) Persistence became

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[53] Jacob Walcher and Martin Tranmael are two notable examples. Walcher, a prominent SAP leader, took Brandt under his wing and played a substantial role in promoting his party career (1933–39). Tranmael, "grand old man of Norwegian Socialism," also took a special liking to Brandt. This connection scarcely hurt Brandt during his Scandinavian exile.

[54] For example, together with Stefan Szende, August and Irmgard Enderle and Ernst Behm, he published the thoughtful *Zur Nachkriegspolitik der deutschen Sozialisten* (Stockholm: AB Arbetarnes Tryckeri, 1944).

[55] On this see Barbara L. Kellerman, "Willy Brandt: Portrait of the Leader as Young Politician" (Ph.D. dissertation, Yale University, 1975), pp. 281–95.

Brandt's hallmark, enough persistence to go to the top as the epitome of modern Social Democratic leadership. (9) Brandt resigned almost precipitously as chancellor in the light of political failure and moral aspersions. (10) His political life continues, as evidenced by his retention of the chairmanship of the SPD, and his energetic input into the international socialist movement. The dedication to socialism persists.

Brandt's ideological choices are, atypically, more numerous and harder to identify. That is, during Brandt's adult life virtually not a week has been free of speeches, writing, and behavior requiring that he take an ideological position. Between 1931 and the present, these positions varied considerably. Still, the drift is to Leber and Reuter. We may discern a few main trends: (1) a move from left to center which was solidified between early and middle adulthood; (2) a vision of politics as the art of compromise, as illustrated by his unusual tolerance of the radical youth wing (Jusos) of the SPD while he was chancellor and SPD chairman; (3) a belief in the youth as torchbearers of a new and brighter day; (4) a conviction that politics must concern itself with ethics (and a concomitant revulsion against fighting from weak *moral* grounds); and (5) a growing belief in the "good Germany" with himself as its living symbol (as Leber and Reuter had been), as agent of national catharsis, as the collectively-chosen redeemer who could dare to sink to his knees before a Warsaw monument honoring Jewish war dead. (This role too persists. As Germany veers to the right amidst terrorist attacks [1977], Brandt warns against a backlash of intellectual intolerance.)

This single case study bears out Levinson's conclusion that men who have had mentors are in some way better off than those who have not.[56] The findings suggest four reasons. First, there is the mentor's supportive role; he bestows the all-important blessing and facilitates the younger man's entry into the professional world. Second, the mentor is a crucial agent of transition. By allowing the mentee to transform himself from an apprentice to an equal, the mentor eases the developmental transition from early to middle adulthood. Third, the mentor offers a model which, through the process of internalization, allows the mentee to draw on a major resource for growth in adulthood. Finally, having a mentor facilitates generative ability, the ability to become a mentor oneself. This last point can scarcely be overestimated. Like Erikson before him,[57] Levinson has alluded to generativity as an essential component of a realized adulthood.

Mentoring, as a potentially critical factor in adult (male) development has, with the advent of Levinson's work, received a fresh attention.[58] Like any human interaction, it derives its importance from gross similarities (all mentoring has some fundamentals in common), and its textural richness from minute differences (how precisely did this particular mentor relate to this particular mentee in this particular situation). On the question of the role of the mentor in theory of political leadership, this study suggests a first hypothesis: the extent to which a leader has been the beneficiary of *good* mentoring will help determine the extent to which his leadership style is less preoccupied with power relationships than with interactions which are more permissive and democratic. To those who would seek to confirm this proposition, Brandt's tenure as German chancellor (1969–74) might serve as a first case in point.[59]

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[56] We might hypothesize about the differences among men–here political leaders–who grew into adulthood with the benefit of mentors, those who lacked mentors, and even those whose mentors explicitly denied them the "blessing." In this third category, the famous example of Lenin's last testament warning against Stalin springs to mind.

[57] Erikson, *Childhood and Society*, pp. 266–68.

[58] The popular press has joined the ranks of the investigators. See Gail Sheehy's cover story in *New York* 5, April, 1976.

[59] Consider also the most striking example of political mentoring in our current public life: the relationship between Hubert Humphrey and "Fritz" Mondale.

Personality Effects on American Foreign Policy, 1898-1968: A Test of Interpersonal Generalization Theory *

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Whether personality characteristics of American leaders crucially determine major American foreign policy decisions has been a matter of considerable disagreement. A test of two hypotheses drawn from interpersonal generalization theory shows such influences have probably been crucial in a number of cases in American foreign policy between 1898 and 1968. In 49 cases of intraelite disagreement on force-related issues and 13 cases of intraelite disagreement on inclusionary issues the direction of disagreement could be predicted in over 75 percent of the cases by knowledge of individual differences in interpersonal relations. A four-fold speculative typology suggests fundamental personality-based differences in orientation towards America's preferred operating style and role in the international system (e.g., introverts are drawn toward impersonal principles and mechanisms like balance of power—or in an earlier period to international law).

The evidence implies that one source of war and hard-line foreign policy is the structure of self-selection and recruitment to high office in the American political system. As well, the systematic tendency to self-expressive personalization in major foreign policy decisions probably increases the rate of error of American elites.

Have personality characteristics of American leaders been decisive in major foreign policy decisions during this century? Traditionally many scholars have maintained that the answer, in general, is "no."[1] However, this thesis has never been subjected to careful test, and there appear to be reasons to believe such a test might be worthwhile: several hundred studies have shown significant correlations of personality traits with foreign policy preferences within mass publics,[2] several elite case studies and exploratory comparative analyses have made plausible arguments for the presence of significant personality influence on policy decisions in a variety of important areas,[3] and recent studies of American diplomats report signifi-

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*I would like to express my appreciation to Chris Achen, Hayward Alker, Bob Axelrod, Paul Berman, Alexander L. George, Norman Graebner, Charles Heck, Ole Holsti, Arnold Kanter, Robert E. Lane, John McConahay, Ken McVicar, the late Conrad Morrow, David Rothberg, Doug Sprague, and H. Bradford Westerfield for comments, support, and other assistance. I am grateful for the early encouragement of Harold D. Lasswell and J. D. Barber. Gail Lopata, Lisa Gregorie and Linda Woolford typed the manuscript.

[1] The traditional argument against exploring "non-rational" influence is Sidney Verba, "Assumptions of Rationality and Non-Rationality in Models of the International System" in *The International System: Theoretical Essays*, ed. Klaus Knorr and Sidney Verba (Princeton: Princeton University Press, 1961), pp. 93–117. On the traditional preponderance of the Rational Actor model see, for example, Graham T. Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (Boston: Little, Brown, 1971), p. 10. Kreuger remarks on the historiography of American foreign policy that "most American historians view diplomacy as the outcome of decisions made by rational men in pursuit of the national interest" (p. 93). Thomas A. Kreuger, "The Social Origins of Recent American Foreign Policy," *Journal of Social History*, 7 (Fall 1973), 93–101. The emerging historiographic challenge to this paradigm is discussed in Robert M. Crunden, "Freud, Erikson, and the Historian: A Bibliographic Survey," *Canadian Review of American Studies*, 4 (Spring 1973), 48–64.

[2] Useful reviews are W. Eckhardt and T. Lentz, "Factors of War/Peace Attitudes," *Peace Research Reviews*, 1 (October 1967), entire; W. Eckhardt, "Ideology and Personality in Social Attitudes," *Peace Research Reviews*, 3 (April 1969), entire; Herbert McClosky provides a discussion of both elite and mass data in his "Personality and Attitude Correlates of Foreign Policy Orientation" in *Domestic Sources of Foreign Policy*, ed. James N. Rosenau (New York: Free Press, 1967), pp. 51–109; Bjorn Christiansen, *Attitudes Towards Foreign Affairs as a Function of Personality* (Oslo: Oslo University Press, 1959); Paul Sniderman and Jack Citrin, "Psychological Sources of Political Belief: Self-Esteem and Isolationist Attitudes," *American Political Science Review*, 65 (June 1971), 401–17.

[3] See for example Alexander L. and Juliette George, *Woodrow Wilson and Colonel House: A Personality Study* (New York: Dover, 1964); James D. Barber, *The Presidential Character* (Englewood Cliffs, N.J.: Prentice-Hall, 1972); Joseph De Rivera, *The Psychological Dimension of Foreign Policy* (Columbus, Ohio: Merrill, 1968); Arnold Rogow, *James Forrestal: A Study of Personality, Politics, and Policy* (New York:

cant personality-policy preference correlations among career professionals.[4]

This study was designed to test two hypotheses of personality influence on American foreign policy between 1898 and 1968. These two hypotheses were derived from interpersonal generalization theory, a theory exemplified by Bjorn Christiansen in his classic study and review of early literature, *Attitudes Toward Foreign Affairs as a Function of Personality* (1959).[5] Christiansen's theory was that behavioral differences in interpersonal situations produced similar behavioral differences in international situations. Using a paper and pencil instrument with 40 everyday scenarios and 40 international scenarios and 167 cadets and recruits at the Military and Naval academies in Oslo, Christiansen found a moderate and statistically significant tendency for six alternative responses in interpersonal situations (e.g., asserting oneself to change the other party's behavior) to generalize in a straightforward fashion to preferred responses by Norway (e.g., that Norway should assert itself and demand that the other party change its behavior).

Christiansen's study was not definitive for understanding government behavior in international relations. It was exclusively a paper and pencil self-report measure. The completion of 80 scenarios with the same response format may have increased correlations by inducing repetitious answers. Moreover, the subjects were not national leaders with expertise, extensive knowledge, responsibility, and the necessity of accounting for their views. Unlike what happens in the real world, Christiansen's subjects did not engage in discussion or hear opposing views on issues they were asked about before selecting their responses. Most importantly, Christiansen recognized that a range of "viable" options is established by the context of the domestic and international situation. So, in no scenario were individuals asked if they wanted Norway to go to war. Thus the study did not establish that self-assertive tendencies in everyday behavior actually carry someone beyond the threshold of normal diplomacy to war.

Since Christiansen's study, several hundred other studies have tended to support his theory in mass public samples. In the terms of Eckhardt's (1967, 1969) review and factor analysis, the tendency to favor willful "compulsion" of others seems to generalize directly from interpersonal to international situations.[6] But a number of these studies also have expanded Christiansen's theory and show that the individual's tendency is to generalize acts by psychological analogy; it intersects available capabilities and, at least among Americans, this produces *magnification* of interpersonal tendencies. That is, an American who believes children should be subjected to strict discipline might be more likely to favor use of nuclear weapons internationally. In the same vein, a recent study of a random sample of 126 American diplomats found that diplomats who like to compete (behavior which might be demonstrated in daily

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Macmillan, 1963); Bruce Mazlish, *In Search of Nixon* (New York: Basic Books, 1972); Betty Glad, *Charles Evans Hughes and the Illusions of Innocence* (Urbana: University of Illinois Press, 1966); Doris Kearns, *Lyndon Johnson and the American Dream* (New York: Harper and Row, 1976). Somewhat overdrawn is Nancy Clinch, *The Kennedy Neurosis* (New York: Grossett, 1973). See also the studies based on Alexander George's "operational code" approach: Ole Holsti, "Cognitive Dynamics and the Image of the Enemy: Dulles and Russia" in *Enemies in Politics,* ed. David Finley, Ole R. Holsti and Richard R. Fagen (Chicago: Rand McNally, 1967), pp. 25–96; Ole Holsti, "The Operational Code Approach to the Study of Political Leaders: John Foster Dulles' Philosophical and Instrumental Beliefs," *Canadian Journal of Political Science,* 3 (March 1970), 123–57; Alexander L. George and Ole R. Holsti, "Operational Code Belief Systems and Foreign Policy Decision-Making" (unpublished); see also the series of papers prepared for the annual meeting of the 1973 American Political Science Association: G. G. Gutierrez, "Dean Rusk and Southeast Asia: An Operational Code Analysis"; Kurt Tweraser, "Senator Fulbright's Operational Code as Warrant for His Foreign Policy Advocacy, 1943–1967: Towards Increasing the Explanatory Power of Decisional Premises"; Loch Johnson, "Operational Codes and the Prediction of Leadership Behavior: Senator Church at Mid-Career." See also Margaret G. Hermann, "How Leaders Process Information and the Effect on Foreign Policy: An Exploratory Study," in *Comparing Foreign Policies: Theories Findings and Methods,* ed. James N. Rosenau (Beverly Hills, Calif.: Sage, 1974).

[4] Major results are reported in Lloyd S. Etheredge, *A World of Men: The Private Sources of American Foreign Policy* (Cambridge: M.I.T. Press, in press); see also Bernard Mennis, *American Foreign Policy Officials: Who They Are and What They Believe Regarding International Politics* (Columbus: Ohio State University Press, 1971). David C. Garnham, *Attitude and Personality Patterns of American Foreign Affairs* (Ph.D. dissertation, University of Minnesota, 1971).

[5] Christiansen, *Attitudes Towards Foreign Affairs as a Function of Personality* (Oslo: Oslo University Press, 1959).

[6] Eckhardt, "Ideology and Personality"; Eckhardt and Lentz, "Factors of War/Peace Attitudes."

life by liking competitive sports or vying for promotion) tended to favor the use of American military force in four scenarios involving underdeveloped countries.[7] Generalization, involving magnification of self-assertive tendencies, now does appear implicated in war.

However, with the exception of the diplomat study mentioned above, most of the studies inspired by Christiansen's work present an additional methodological difficulty if one wishes to extrapolate to the behavior of national leaders. Probably to prevent a large number of "don't know" responses, most mass public studies phrase simplistic alternatives–e.g., "a country faced with a bunch of really dangerous enemies would be better advised to shoot first and ask questions later." Such questions may tend to elicit more emotionally expressive responses than would actual complex policy questions and, if so, the results obtained would be biased in favor of substantiating the hypothesis that elite foreign policy decisions have an emotional base.

Interpersonal generalization theory seems potentially relevant but it needs to be tested directly before we may call it useful in identifying one of the factors contributing to the policy choices of elites. Interpersonal generalization theory has, of course, been used by some historians. A typical impressionistic method is to use an adjective that plausibly describes both the individual and his policies. Thus Theodore Roosevelt was "rambunctious," Wilson an "idealist," Kennedy an "activist." While such implicit use of generalization theory may be correct, the underlying method is open to criticism on three grounds. First, there is no independent assessment of the personality and policy choice variables nor are there explicit decision rules about what evidence counts for or against the interpretations. Second, it is *ad hoc*–usually only one individual is involved and the metaphor settled upon is not designed to test a larger comparative taxonomy that would aid the understanding of other cases. Third, and most seriously, the method is *inconclusive.* By failing to control carefully for and to rule out major alternative explanations (e.g., costs and benefits of the domestic and international political situations) the investigator often needs to rely on literary skill to persuade the reader that personality (rather than some other combination of factors) was a crucial variable.

I

It should be clear that the question posed for test is not whether "personality" influences policy. Broadly construed, "personality" refers to many characteristics of decision makers (e.g., at least normal intelligence, cultural values, identification with the welfare of the nation) that are probably always relevant. But such constant, *shared* characteristics can either be assumed or subsumed under higher levels of analysis. The theoretical question addressed here is whether intraelite *variations* in personality characteristics produce *variations* in policy preference so that some personality characteristics unique to a key decision maker need to be measured and taken into account to explain why some policies are adopted rather than others.

It should also be clear that what is being tested is not a general theory of international behavior–there is no claim that "personality traits produce wars" alone. Rather the question is whether certain personality traits, in certain situations (the nature of which are left unspecified here) actually have tipped the balance for or against a policy choice like war and are thus a crucial ingredient in producing the policy choices in such situations.

The research method I employed in this study was to work backward, to select cases of American intraelite policy disagreements and then to test the proposition that the direction of such disagreements could be consistently predicted from independently derived knowledge of personality differences between the participants.[8] If personality differences show only a random correlation with the direction of policy disagreement, then it cannot be said that such personality differences produced policy differences. But if the imaginary random control group of the null hypothesis is rejected, then the conclusion would be that, in the confirming cases, the observed personality difference was probably a crucial contributor to the actual policy outcome.

It should be noted that interpersonal generalization theory would also predict that policy *agreement* should tend to flow from *similar* personality traits. This is a second test which could be conducted. I did not, however, believe it would be as straightforward and direct a test. A president tends to favor as secretary of state and key advisors individuals who share the president's views and thus there is implicit pressure for them to agree to retain influence. I assumed that such situational pressures and other strains toward "groupthink" often confound or override personality similarity as a factor in producing agreement.[9] Agreement tends to be broken, I assumed, only by strongly felt (hence probably personality-based) differences. In other words, I expected less confounding of other factors in producing disagreement, and I selected the present design as a better (albeit more limited) way of giving personality factors a direct chance to register their presence.

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[7] Etheredge, *A World of Men: The Private Sources of American Foreign Policy.*

[8] I am here following Greenstein's methodological lead. However, I have altered his concept of "actor dispensability" to the slightly more constrained concept of "elite actor interchangeability," a modification which seems more useful for focusing upon different levels of analysis. See Fred I. Greenstein, *Personality and Politics: Problems of Evidence, Inference, and Conceptualization* (Chicago: Markham, 1969), Ch. 2.

Focusing on intraelite disagreements at the same time allowed me to hold constant major alternative hypotheses. The domestic and international situations are the same for all participants. (The major competing hypothesis not ruled out explicitly by this design is the possibility that intraelite role is a significant influence–i.e., that the secretary of state tends to oppose military options by virtue of the office. If personality differences and policy differences correlate with office, then a correlation between the first two may not be a causal relationship. I return to this problem later.)

Thirty-six men were selected for study–American presidents, secretaries of state, and selected advisors who served between 1898 and 1968. I assessed personality traits by searching scholarly works, insiders' accounts, biographies, and autobiographies for the men under consideration. I excerpted passages relevant to two personality dimensions–general *dominance over subordinates* and *extroversion*, omitting explicit clues that would identify the individual involved or the scholarly source, and included these passages in dossiers. These dossiers were coded in random order using the rating criteria reported in the notes of Tables 2 and 3 by two independent judges unaware of the identities of the men they rated. I, who knew the identities and the scholarly sources, also rated the men. Among the three judges correlations ranged, for dominance, from .83 to .91 ($p<.001$) and between .84 and .93 for extroversion ($p<.001$). There were no systematic differences among us, and I used the unweighted mean rating of the three judges as a subject's rating on that personality dimension except in the case of William Jennings Bryan. Knowing the original sources of the ratings for Bryan, I felt that it was the less scholarly and more idealized accounts that gave him a high-dominance rating and I lowered his dominance score since I felt the lower score would be more accurate.

The task of second-hand personality assessment of historical figures presents troublesome methodological issues which should be examined. In the first instance, one's observed behavior can differ substantially depending upon one's role and one's relation with those with whom one interacts. A man like Secretary Dulles could be dominant over his subordinates yet deferential to a superior. This social context must be standardized explicitly. I chose to assess dominance by assessing dominance over nominal subordinates on the assumption that a person's inner desire to dominate would be less inhibited and show itself more clearly in this sector of life. In addition, since America's use of force has often been directed against smaller countries, I felt this was the most relevant tendency of interpersonal behavior that would generalize.[10] Special weight in the coding was given to the scope and level of activity through which these men sought to initiate, monitor, and give personal direction to the activities of their subordinates. Thus an individual was described as very high on dominance if he was "recorded as regularly intervening at lower levels *or* ignoring subordinates completely *while setting policy himself.* May berate subordinates, seeks to impose will forcefully. He runs the show. Complex variegated information-acquisition system." A very low-dominance individual was one who "seldom interferes, defers to others, almost welcomes initiatives of others as a relief. Doesn't as much share power as abdicate it."

Criteria to assess extroversion were specifically constructed to take into account situational differences between men in these offices and a representative sample of the population. Most of these men, by virtue of their public offices, probably spent more time with more people than the average American. They probably had more friends and acquaintances. Accordingly, to differentiate among them, coding criteria gave special weight to how a man spent what little leisure time he had: did he spend it with large numbers of other people (as was true of President Lyndon Johnson) or did he prefer relative isolation (Secretary Dulles, for example, would go with his wife to an isolated island away from telephones and other people)? A very extroverted individual was one who was "emotionally outgoing, loves crowds. Enjoys contacts with many kinds of people. Leisure time spent with people." A very introverted subject was one described as "cold, icy or aloof or acutely shy. Few close friends. Leisure time spent away from people."

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[9] Irving L. Janis, *Victims of Groupthink* (Boston: Houghton Mifflin, 1972).

[10] It is conceivable that, with more cases, a less simple and more differentiated approach would be useful. Thus the present personality dimension may predict best to use of force against smaller countries but relations with autonomously powerful opponents in domestic politics might predict better to relations with the Soviet Union since World War II. For all his bullying tendencies toward subordinates Lyndon Johnson was more restrained and empathetic in dealing with the Soviet Union.

An ideal personality assessment would be made by trained observers using explicit criteria, working independently of one another, and without prior knowledge of policy attitudes. This ideal was only partially met in the available source material–in particular most writers had a prior knowledge of policy decisions, there was a tendency toward rating interdependence with later scholars partially relying on the work of earlier writers, and there was a tendency toward impressionism, a lack of rigorous detailing of specific instances which led to a generalization. Also, the high degree of consensus in this study might have resulted from bias in my own excerpting. For all of these reasons it is important to have an independent check.

Fortunately, a partial check is possible. Donley and Winter (1970) scored inaugural addresses (which were delivered, of course, prior to later policy decisions) of 11 presidents for achievement and power motivation.[11] On this subsample correlations with the present dominance ratings were .54 ($p < .01$ for N-Power and .77 ($p < .01$) for N-Achievement. Such significant correlations are supportive evidence that present ratings are reasonably valid. The high degree of consensus among coders and across related methods occurs, I think, because these were all public figures much talked about and observed (hence a consensus about them in the present ratings) and because the differences among them are quite marked.

I do not want to leave the impression, however, that the rating task was entirely straightforward. While generally successful, the effort to disguise identities was not wholly effective since both raters were political scientists (although not foreign policy experts) and had some knowledge of the people in question: Franklin Roosevelt and Theodore Roosevelt were each recognized by one rater.[12] In addition there was considerable difficulty in rating Harry Truman since he was generally egalitarian but fired General Douglas MacArthur for asserting too much independence. My own feeling was that this reflected a habitual low dominance on Truman's part, a tendency which produced such later problems that he was forced to assert himself. The judges seemed to feel the same way, although with some misgivings–and it is appropriate to note that Donley and Winter do give Truman a high relative score for power motivation. Another complex case was Eisenhower, who asserted himself strongly in establishing procedures under which his key subordinates made major decisions. He is scored low here because the rating scales assessed assertion over policy *content,* although clearly there is another, *procedural,* sense in which Eisenhower was very much in charge. Herbert Hoover also was complex: he tended to be very dominant over policy initiatives intellectually, although he was personally a tolerant and egalitarian man who kept in office a secretary of state (Stimson) with whom he often disagreed fundamentally and strongly. Hoover is scored moderately high on dominance here although it should be noted that a single rating scale collapses this distinction between intellectual and interpersonal dominance.

There is a final and important methodological point. When I began this study I was not totally ignorant of American foreign policy, and I knew a substantial amount about personalities and policies since World War II. It was this knowledge which helped convince me that interpersonal generalization theory was a good candidate for testing, and I used this knowledge, as well as previous studies, to specify the hypotheses and devise the rating criteria to capture what I thought would be the operative personality traits. I did not know in detail what all the evidence would show (especially prior to World War II) but I have accordingly been cautious to avoid counting too many recent cases on related issues (e.g., of Dulles-Kennan or Dulles-Stevenson disagreements) which I knew in advance would support the hypotheses. But the very knowledge that led me to spend time on the study and which helped to design it also tends to undermine the characterization of the study as completely a scientific test. In truth, the study was designed with some prior knowledge of the data. This problem, of course, is not unique to this study but pervades studies of well-known events. Any retrospective study of events of which the researcher has prior knowledge is tainted; indeed the more one is an expert in his field the less scientifically definitive any explanatory study will be, and the more any ultimate theoretical verification must depend upon replication by another researcher using a different sample. The present study is no exception to this caveat.

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[11] R. E. Donley and D. G. Winter, "Measuring the Motives of Public Officials at a Distance: An Exploratory Study of American Presidents," *Behavioral Science,* 15 (1970), 227–36. See also David G. Winter, *The Power Motive* (New York: Free Press, 1973), pp. 212–18. Methodological issues of attributing variations in such scores primarily to personality variations are, of course, present and are discussed in Donley and Winter. For evidence that changes in national mood may produce different American leaders, see David McClelland, *Power: The Inner Experience* (New York: Irvington, 1975).

[12] The use of trained political scientists can be challenged on the grounds they may bring bias to such tasks. My own feeling is that they bring a useful sensitivity to power. For example Franklin Roosevelt's chaotic administrative style might be interpreted as reflecting low dominance. The judges, however, saw this as a style consciously designed to heighten presidential dominance. I think they were correct in this view, but it is true that other judges could have different assessments. The same comment applies to the Truman coding problem discussed next in the text.

Actual personality ratings employed 10-point scales, and hypotheses were tested using numerical scores. These are reported in the notes to Table 2. However, for ease of visual presentation the 36 subjects are placed in one of 16 cells in Table 1.

I derived two hypotheses from interpersonal generalization theory to test for a systematic link between personality differences and policy differences. The first hypothesis was readily suggested by previous research in the Christiansen tradition, the second was my own related guess of a possible interpersonal-international linkage:

1. In cases of disagreement on the following aspects of the use of force those scoring *higher* on dominance will be *advocating* the threat or use of military force, military intervention, ultimata, and the military occupation of other countries and *opposing* moves toward disarmament and arbitration agreements when compared with those scoring lower on dominance.
2. In cases involving disagreement about policy toward the Soviet Union or the Soviet Bloc, those scored as *more extroverted* will advocate cooperative, inclusive policies—recognition, more trade, summit conferences, negotiations to resolve differences—when compared with those scored as more introverted.

Table 1. Comparative Personality Assessments: Presidents, Secretaries of State, Selected Advisors, 1898–1968

	Introvert			Extrovert
High Dominance	Dulles Wilson			Johnson Roosevelt, F. Roosevelt, T.
	Hoover Hughes Stimson	Acheson House Hull Root	Byrnes Hopkins Kennedy	
	Herter Kennan Marshall	Colby Knox Lansing	Bacon McKinley Stettinius Stevenson Truman	Bryan Taft
Low Dominance	Coolidge Day Sherman	Kellogg Rusk	Eisenhower Hay	Harding

Note: Categories are for visual display only; boxes are not equal intervals. Hypotheses were tested with numerical scores.

II

I scored policy disagreements from the same sources I consulted to obtain material for personality ratings. To eliminate some of the possible alternative explanations, I scored intraelite personality disagreements usually only when the individuals involved actually held office in the administration in power. However, to give additional coverage I also included a small number of individuals who were not in office at the time but who appeared to express themselves consistently and sincerely rather than merely and solely as a tactic to gain advantage in domestic political contests. The main consequence of this extended coding was to lengthen the coverage for President Theodore Roosevelt, Secretaries Root and Acheson, and advisors Kennan and Stevenson. Such additional cases did not bias the results of the tests (see Table 2, n. 8, and Table 3, n. 4).

As an additional cautionary restriction in coding I limited cases to those in which it seemed clear that the individual involved had access to the relevant information. The chief exclusion was comparisons between Franklin Roosevelt and Secretary Hull (whom FDR had appointed for domestic political reasons and did not usually keep fully informed).

I chose to be conservative, as I noted earlier, in coding a series of disagreements on related issues rather than to risk reporting a test more heavily weighted by a few individuals of whom I had detailed prior knowledge. In all of these series of cases (with the possible exception of Secretary Stimson's disagreements with the slightly more dominant and pacific President Hoover) this coding decision worked to lower the number of cases consistent with the first hypothesis.

The recorded disagreements are presented in Tables 2 and 3. In all, there were 49 disagreements scored on issues related to force, 13 disagreements scored on issues involving initiatives to "build bridges" with the Soviet Union.

There are, of course, methodological issues underlying this listing of cases: first, comprehensiveness; second, potential bias. Is this a definitive listing of all disagreements among these participants between 1898 and 1968? If not, are there grounds for maintaining that the listing is unbiased, especially considering my prior knowledge of the hypotheses under investigation?

No one can maintain that this is a comprehensive listing. It is doubtful that all disagreements were recorded by participants or that they all found their way from private papers to published accounts. As well, the historical literature is somewhat uneven and one has the suspicion that some administrations are more exhaustively reported than others. Nevertheless, intra-administration disagreement has been a prominent topic in the literature, and I would be surprised if marked disagreements at least on major uses of force are unrecorded.

Granted that the listing probably is not comprehensive, could it be *biased* unconsciously in favor of the hypotheses? Yes, it could. I sought to deal with this possibility in two ways: first, by circulating an earlier version of this manuscript to six political scientists and diplomatic historians, second, by using an independent coder ignorant of the hypotheses to recode the single most comprehensive source of personal views, Norman Graebner's edited collection, *An Uncertain Tradition: American Secretaries of State in the Twentieth Century*.[13]

None of the professionals felt able to certify the comprehensiveness of the listing. But their comments and the independent coding of the Graebner volume were useful and did identify three cases, totally 5 disagreements, to add to the original 44 coded for hypothesis one.

One cannot claim that these procedures are exhaustive, only that they are reasonably thorough, given available resources. The ideal alternative would be to enlist a team of diplomatic historians for several years of work in archives and private papers without telling them the hypotheses under investigation. But given these checks to eliminate coding bias unwittingly favorable to the hypotheses, and given the high percentages of cases supporting the hypotheses, it seems unlikely that massive bias in favor of the theory is undetected on such a scale as would alter the conclusions reported here, at least in the case of the large *N* for the first hypothesis.

However the professional reviewers, especially the diplomatic historians, did raise queries that deserve discussion. One diplomatic historian felt the entire theory was "obvious" and he was bemused by social scientists "hacking their way through open doors." Of more practical relevance, another objected to including President McKinley's initial opposition to the Spanish-American War, on the grounds that McKinley eventually acceded to it. (I believe McKinley's early opposition was a reasonable

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[13] Norman A. Graebner, *An Uncertain Tradition: American Secretaries of State in the Twentieth Century* (New York: McGraw Hill, 1961).

Table 2. An Inventory of Policy Disagreements: Coercion and Threat (Hypothesis One)

Subjects	Description	References	No. Consistent	No. Not Consistent
1. T. Roosevelt vs. McKinley	Roosevelt was an earlier advocate of the Spanish-American War.[1]	Howard K. Beale, *Theodore Roosevelt and the Rise of America to World Power* (Baltimore: The Johns Hopkins University Press, 1956), pp. 61–63 et passim.	1	0
2. T. Roosevelt vs. Hay	Roosevelt objected to the first *Hay-Pauncefote* Treaty because he wanted the Isthmian canal fortified.	William Thayer, *The Life and Letters of John Hay*, Vol. 2 (Boston: Houghton, 1915), pp. 339–41.	1	0
3. T. Roosevelt vs. Hay	Against Hay's objection Roosevelt used veiled threats in the Alaskan boundary arbitration with England.	Foster Rhea Dulles, "John Hay," in *An Uncertain Tradition: American Secretaries of State in the Twentieth Century*, ed. Norman A. Graebner (New York: McGraw Hill, 1961), p. 36.	1	0
4. T. Roosevelt vs. Root, Bacon	Root and Bacon opposed sending American troops to Cuba in 1906.	Philip C. Jessup, *Elihu Root*, Vol. 2 (New York: Dodd, 1938), p. 156; James B. Scott, "Robert Bacon," in *The American Secretaries of State and Their Diplomacy*, Vol. 9, ed. S. F. Bermis (New York: Cooper Square, 1963), p. 290.	2	0
5. T. Roosevelt vs. Root	Roosevelt was very reluctant to agree to 24 bilateral arbitration treaties negotiated by Root.	Charles Toth, "Elihu Root," in Graebner, p. 56.	1	0
6. T. Roosevelt vs. Root	Root opposed Roosevelt's desire to intervene militarily in Venezuela.	Toth, p. 48.	1	0
7. Knox vs. Root	While in private life Root dissented from a Knox ultimatum to Chile.	Jessup, p. 250.	0	1
8. Wilson vs. Root, Taft, Bryan, Bacon	Root, Taft, Bryan, and Bacon opposed Wilson's military intervention in Mexico. (Bryan acquiesced until the Vera Cruz landing.)	Jessup, pp. 259–61, 256; Richard Challener, "William Jennings Bryan" in Graebner, p. 92; Merle Curti, "Bryan and World Peace," *Smith College Studies in History*, 16 (April-July 1931), 180–81; James B. Scott, *Robert Bacon: Life and Letters* (New York: Doubleday, Page, 1923), p. 265.	4	0
9. T. Roosevelt vs. Wilson, Root, Taft, Bryan, Bacon	Roosevelt wanted Wilson's Mexican policy to be more forceful.	W. H. Harbough, *The Life and Times of Theodore Roosevelt*, rev. ed. (New York: Collier, 1963), p. 450. See references in 8 above.	4	1 (Roosevelt-Wilson, no difference)
10. T. Roosevelt, Lansing vs. Wilson	Roosevelt and Lansing wanted earlier American entry into World War I.	Jessup, pp. 321–23, et passim.	0	2

Table 2. (Continued)

Subjects	Description	References	No. Consistent	No. Not Consistent
11. T. Roosevelt, Wilson, Lansing vs. Root	Root opposed Wilson's "path to war" policies prior to the sinking of the Lusitania.	Jessup, pp. 321–33.	2	1
12. T. Roosevelt, Wilson, House, Root, Lansing vs. Bryan	Bryan resigned as Secretary of State in opposition to Wilson's movements toward entering World War I.	Challener, p. 98.	5	0
13. Hughes vs. Harding	Harding dissented from a stiff stand against the Obregon government of Mexico urged by Hughes in a dispute over American property claims.	Robert K. Murray, *The Harding Era* (Minneapolis: University of Minnesota Press, 1969), pp. 329–31.	1	0
14. Hughes vs. Harding	Hughes wanted to keep U.S. forces in Haiti in 1923; Harding wanted to withdraw them.	Murray, p. 334.	1	0
15. Hoover vs. Stimson	Hoover opposed Stimson's desire to "put teeth" into the Kellogg-Briand pact by invoking economic sanctions against Japanese aggression in Manchuria.	Elting Morison, *Turmoil and Tradition: A Study of the Life and Times of Henry L. Stimson* (Boston: Houghton Mifflin, 1960), pp. 445, 382–83, 403–05.	0	1
16. Hoover vs. Stimson	Hoover was a stronger advocate of disarmament than Stimson.	Morison, p. 410.	0	1
17. Acheson, Dulles vs. Kennan	Kennan advocated halting the northern advance of Allied forces in Korea at the 38th parallel.	George F. Kennan, *Memoirs, 1925–1950* (New York: Bantam, 1969), p. 523.	2	0
18. Acheson, Dulles vs. Kennan	Kennan dissented strongly from the militarization of containment urged by both men.[2]	Kennan, pp. 497–528 et passim; *Memoirs, 1950–1963* (Boston: Little Brown, 1972), passim.	2	0
19. Dulles vs. Eisenhower	Eisenhower opposed the Dulles plan for the U.S. to intervene in Indochina to rescue and supplant the French.[3]	Hans J. Morgenthau, "John Foster Dulles" in Graebner, p. 296.	1	0
20. Dulles vs. Acheson	Acheson opposed the Dulles policy of a strong military stand to retain Quemoy and Matsu.	*New York Times*, Sept. 7, 1958, p. 1.	1	0
21. Dulles vs. Acheson	In the 1958–59 Berlin deadline crisis Acheson felt the Dulles policy was too conciliatory.	*New York Times*, Nov. 19, 1959, pp. 1, 22.	0	1
22. Kennedy vs. Rusk, Stevenson	Rusk had misgivings about, and Stevenson wholly disapproved of, the Bay of Pigs invasion.	Edward Weintal and Charles Bartlett, *Facing the Brink* (New York: Scribner, 1967), p. 149; Arthur M. Schlesinger, Jr., *A Thousand Days* (New York: Houghton, 1965), pp. 259, 271.	2	0

Table 2. (Continued)

Subjects	Description	References	No. Consistent	No. Not Consistent
23. Acheson vs. Kennedy, Stevenson	Acheson favored a more hard-line stance than Kennedy or Stevenson in the 1961 Berlin crisis.	Schlesinger, pp. 380–83.	1	1
24. Kennedy vs. Stevenson	During the same crisis Stevenson urged a more conciliatory stand than Kennedy selected.	Schlesinger, p. 346.	1	0
25. Acheson vs. Kennedy, Stevenson	Acheson consistently favored the air strike option during the Cuban missile crisis.	Graham T. Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (Boston: Little Brown, 1971), p. 208.	1	1
26. Kennedy vs. Stevenson	Stevenson favored negotiations during the missile crisis; Kennedy adopted a quarantine.	Schlesinger, pp. 807–08.	1	0
27. Kennedy, Johnson vs. Kennan	Kennan dissented from Kennedy and Johnson policies in Vietnam.	J. William Fulbright, *The Vietnam Hearings* (New York: Random House, 1966), pp. 107–66.	2	0
28. Johnson vs. Kennan	During the Soviet invasion of Czechoslovakia Kennan advocated sending 100,000 American troops to Europe; Johnson did not agree.	*New York Times*, September 22, 1968, p. 3.	0	1
		Totals	38	11
		Percentage	77.5%	22.5%
		Probability[4]	$P < .0001$	

Notes

[1] While I have followed the weight of scholarly opinion in scoring McKinley as opposed to the Spanish-American War and being pressured into it, this view is not universal. See, for example, Walter LaFeber, *The New Empire: An Interpretation of American Expansion 1860–1898* (Ithaca: Cornell University Press, 1963).

[2] This case could include Kennan's dissent from the Truman Doctrine in the early Cold War period (he felt it too sweeping). See George F. Kennan, "American Involvement" in *The Viet-Nam Reader,* rev., ed. Marcus G. Raskin and Bernard B. Fall (New York: Vintage, 1967), pp. 17–18. The origin of containment is not explained, on the American side, by personality traits revealed by the personality differences model. But the sweeping implementation, military emphasis, and structuring of the Cold War do seem to be shaped by personality traits.

[3] I have followed the weight of scholarly opinion concerning Dulles' thinking. For a different view consult Robert Randle, *Geneva, 1954* (Princeton: Princeton University Press, 1969), pp. 65, 97.

[4] Since some men appear more than once, and since their views at different times are probably not related randomly, a null hypothesis imaginary base rate of random distribution (50/50) is probably slightly too liberal, although by an unknown amount.

[5] I chose to consider disagreements arising between men who *advocated* different positions. Men who tended to "go along" with the policy initiatives of others were usually excluded. For example in item 2, I counted only the Roosevelt-Hay disagreement (rather than including the Roosevelt-McKinley comparison which would also have been supportive of interpersonal generalization theory) because McKinley tended to take a back seat in foreign policy formation. The Roosevelt-McKinley comparison is included, however, on the issues surrounding the Spanish-American War because McKinley did become actively involved at that time. The reader should be cautioned, however, that this early decision was a matter of some consequence since the men who tend to "go along" turn out to be lower on interpersonal dominance. In other words, the high percentages reported favorably for interpersonal generalization theory are somewhat sensitive to this decision.

[6] I have, after considerable uncertainty, excluded comparisons between Colonel House and Woodrow Wilson on American entry into World War I and between Harry Hopkins and Franklin Roosevelt on American entry into World War II. Both advisors appeared, at times, to be more inclined to advocate military involvement at an earlier point than the president. However, both Wilson and Roosevelt were sensitive to public opinion, and it is a reasonable interpretation of their behavior that they did not move until after a buildup of public opinion; thus, I am not sure that their "true" opinions (the attitudes they would urge on a president if they were his adviser)

case to include.) Pervasive and sometimes strong objections were both the direct assertion that factors other than personality differences (especially differences in cognitive beliefs) explained the observed disagreement and the "anomaly" that high-dominance individuals did not consistently favor force in all cases and those of low dominance sometimes joined those of high dominance in advocating force in some cases. These objections usefully clarify what is tested here. First, there is no claim that different cognitive beliefs are uncorrelated with different policies. It is obvious from the words of the actors themselves that different choices *are* justified in a rationalist way. The relevant question investigated in this study is only whether the different cognitive justifications that are typically engaged and reported by participants and diplomatic historians might also be considered manifestations of deeper and more pervasive differences in motivational psychodynamics.

Another way to put the question is to ask whether one needs first to study each individual case and rule out all plausible alternative explanations before including that case and concluding that a novel general theory built on such cases is valid? I think not. Such ruling out of alternative explanations in the specific case is probably impossible since, as the existent historiography demonstrates, plausible cognitive alternatives (for example) are already possible. What the present large *N* study of a single theory does is to increase the confidence with which one can go back to these individual historical cases and make the interpretation of such cases less ambiguous by suggesting more weight be given to relevant psychodynamic considerations. But, of course, the fact that any one single case is *consistent* with the present theory does not *prove* that the case is truly *explained* by the theory, it only creates a presumption in favor of including an explanation at the personal psychodynamic level of analysis.

My own working image of policy decisions is a Lewinian field theory approach: behavior is multiply determined, and a variety of forces

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Notes (Continued)

actually differed from the views which House or Hopkins may have been freer to express. The matter need not be resolved here; but it should be noted that including these two cases would slightly increase the cases inconsistent with interpersonal generalization theory. It should also be noted that the cases listed here predominantly involve American use of force in small country arenas. Direct great power wars may involve such weighty and costly decisions as to overwhelm the personal motive differences discussed here for those who feel under attack.

[7] Were Bryan's dominance score the original 5.67 instead of the adjusted 3.67 there would be a change of 2 cases (the Root (5.33) and Lansing (4.00) comparisons in item 12) in the inconsistent column leaving 36 supportive cases and 13 opposed (73.4 percent supportive).

[8] The question has also arisen whether including men out of office biases these scores. Looking at the subset of only those holding formal offices drops 25 comparisons of which 17 are supportive (1 each for items 2, 17, 20, 23, 25; 2 from cases 8, 11, 12, and 27; 4 from case 9) and 8 inconsistent (1 each from items 7, 9, 10, 11, 21, 23, 25, 28). This leaves 24 cases of which 21 support, and 3 do not support, hypothesis one—an increase of confirmation, 87.5 percent of the 24 cases ($P < .0001$).

[9] Mean dominance scores (extroversion scores in parentheses) used to test hypotheses were Johnson, 9.67 (9.00); Dulles, 9.33 (1.33); Franklin Roosevelt, 9.33 (9.67); Wilson, 8.67 (2.33); Roosevelt, 8.67 (8.33); Hoover, 8.33 (1.33); House, 8.33 (4.33); Hughes, 8.00 (2.00); Stimson, 7.67 (2.33); Kennedy, 6.67 (6.33); Hopkins, 6.00 (6.67); Byrnes, 6.00, (6.33); Hull, 5.33 (4.87); Acheson, 5.33 (4.00); Root, 5.33 (4.33); Lansing, 4.00 (4.13); Bryan, 3.67 (adjusted from 5.67) (7.67); Stevenson, 3.67 (6.33); Stettinius, 3.67 (6.33); Knox, 3.67 (4.00); Bacon, 3.67 (6.33); Truman, 3.67 (7.33); Colby, 3.67 (4.00); Marshall, 3.33 (2.33); Herter, 3.33 (2.67); Taft, 3.33 (7.67); Kennan, 3.00 (2.00); McKinley, 3.00 (2.00); Kellogg, 2.67 (3.00); Eisenhower, 2.33 (5.00); Rusk, 2.33 (3.00); Hay, 1.67 (6.00); Sherman, 1.67 (2.00); Day, 1.67 (2.00); Harding, 1.67 (8.67); Coolidge, 1.33 (1.67).

[10] Scoring criteria for dominance were (1 to 10): *Highest scoring:* Recorded as regularly intervening at lower levels *or* ignoring subordinates completely *while setting policy himself.* May berate subordinates, seeks to impose will forcefully. He runs the show. Complex, variegated information-acquisition system. *Mod High:* In command of situation, may by-pass channels. Some power sharing and flexibility; *Mod:* Sets guidelines and makes final decisions himself. Fair amount of autonomy to subordinates. Tends to work through channels; *Mod Low:* Often takes a stand, primarily to check excesses. Generally grants large amount of autonomy to subordinates. Appoints people and tends to stay out of their way; *Low:* Seldom interferes, defers to others, almost welcomes initiatives of others as a relief. Doesn't as much share power as abdicates it; *Introversion-Extroversion* (1–10); *High Ext.:* Emotionally outgoing, loves crowds. Enjoys contacts with many kinds of people. Leisure time spent with people; *Mod. High:* Warm, outgoing, affable, possibly not drawn to crowds. Likes to socialize in his leisure time; *Mod:* Mixed—usually warm and considerate, especially with a moderate number of friends, is sometimes seen as a bit withdrawn; *Mod Low:* Reticent, shy, self-controlled, unemotional, spends leisure time in relative isolation; *Low:* Cold, icy, or aloof, or actually shy. Few or no close friends, leisure time spent away from people.

Table 3. An Inventory of Policy Disagreements:
Inclusionary Policies Toward Soviet Union and Soviet Bloc (Hypothesis Two)

Subjects	Description	References	No. Consistent	No. Not Consistent
1. Harding vs. Hughes, Hoover	Hughes and Hoover opposed Harding's desire to recognize and increase trade with the Soviet Union.	Robert K. Murray, *The Harding Era* (Minneapolis: University of Minnesota Press, 1969), pp. 348–54.	2	0
2. Eisenhower vs. Dulles	Dulles was cool toward Eisenhower's Open Skies inspection plan.	Sherman Adams, *First-Hand Report* (New York: Harper, 1961), p. 87.	1	0
3. Eisenhower vs. Dulles	Dulles was reluctant about Eisenhower's Atoms for Peace initiative.	Adams, p. 112.	1	0
4. Eisenhower vs. Dulles	Eisenhower was more drawn toward summit conference with Soviet leaders than was Dulles.	Adams, p. 88; E. J. Hughes, *The Ordeal of Power* (New York: Atheneum, 1963), p. 207.	1	0
5. Dulles vs. Stevenson	Stevenson was consistently more willing than Dulles to negotiate differences with the Soviet Union.	H. Muller, *Adlai Stevenson: A Study of Values* (New York: Harper, 1967), pp. 154–55.	1	0
6. Eisenhower vs. Herter	Herter had grave doubts about Eisenhower's desire for a September, 1959 summit meeting with Khrushchev.[1]	G. Bernard Noble, *Christian Herter* (New York: Cooper Square, 1970), p. 78.	1	0
7. Kennedy, Acheson vs. Stevenson	Stevenson favored early negotiating rather than confrontation in the 1961 Berlin crisis.	See references 23 and 24, Table 2.	1	1
8. Kennedy, Acheson vs. Stevenson	Stevenson favored early negotiations rather than confrontation during the Cuban missile crisis.	See references 25 and 26, Table 2.	1	1
9. Johnson, Kennedy vs. Rusk	LBJ and JFK were more inclined toward building bridges with Eastern Europe and negotiating with the Soviet Union (e.g., Test Ban treaty, disarmament initiation); Rusk was more cautious and dubious.	Schlesinger, Arthur M., Jr., *A Thousand Days* (New York: Houghton, 1965), p. 285; Geyelin, P., *Lyndon B. Johnson and the World* (New York: Praeger, 1966), p. 83.	2	0
		Totals	11	2
		Percentages	84.6%	15.4%
		Probability	P<.002	

Notes

[1] Later in his study Noble comments that Herter "approved" of a Khrushchev visit (p. 291).

[2] For mean extroversion scores see Table 2, n. 9.

[3] See Table 2, n. 4.

[4] Dropping men not holding formal offices drops 3 comparisons (1 each in cases 5, 7, and 8) leaving 8 supportive and 2 opposing cases or 80 percent agreement ($p \approx .05$).

and considerations, both arising from the individual and from the specific situation, interact (reinforcing, adding to, modifying, opposing each other) to produce a decision. It is not a theoretical contradiction that, in response to one situation, a high dominance person would, *on balance,* decide *against* force while a different situation would give more obvious and freer expression to one of many personal motivations. To draw an analogy, I am seeking to explain a direction in interpersonal variation *about* a mean in *given* problematic situations (problematic by definition–i.e., those producing intraelite disagreement) when this range of variation already encompasses observed advocacy and opposition to a specific policy; what is being tested is *not* a theory which accounts for the mean, for all the other factors (situational and otherwise) which produce a foreign policy. (And I am *not* advancing here a theory of which objective situations yield a crucial role for personality in the policy response.)

Of the questions raised by others, another of importance is the possibility that dominance ratings merely reflect role-appropriate behavior rather than personality traits. There is a possibility that presidents are scored as more dominant or extroverted than secretaries or that high dominance is an artifact of growing administrative responsibilities as these have increased since the turn of the century. If so, then the personality measures may not truly assess individual motives.

A test of these objections is possible. If we exclude advisors House, Hopkins, Kennan, and Stevenson, we have an *N* of 32 men. Of the most dominant 16, 6 are presidents (LBJ, FDR, Wilson, TR, Hoover, JFK) and 10 are secretaries. Of the remaining 16, the same number (6) are presidents (Truman, Taft, McKinley, Eisenhower, Harding, Coolidge) and 10 are secretaries. There is no general tendency for presidents to be more dominant over subordinates than secretaries. But it is true that among the 6 most dominant personalities, there are 5 presidents (LBJ, FDR, Wilson, TR, Hoover) and only one secretary (Dulles), an unlikely occurrence by chance. Thus it is apparently true that extremely dominant individuals tend to become presidents rather than secretaries, but there is no general confounding of the presidential role with dominance rating, and the hypothesis that these ratings are merely a function of role can be ruled out.

Of the 12 presidents, are the more recent ones more likely to be scored as highly dominant? Of the 4 presidents included since World War II, 2 are in the more dominant half (JFK and LBJ), 2 are in the less dominant half (Truman and Eisenhower). Of the 8 presidents included prior to World War II, 4 are in the more dominant half (TR, Wilson, Hoover, FDR), 4 are in the less dominant half (McKinley, Coolidge, Harding, Taft). There is no tendency for post-World War II presidents to be more dominant over subordinates.

Of the 20 secretaries, are the more recent scored as more dominant? Of the 7 recent secretaries, 4 are in the top 10 on dominance (Dulles, Byrnes, Acheson, Stettinius). Of the 13 who held office prior to or during World War II, 6 (Hughes, Stimson, Hall, Root, Lansing, Bryan) are in the top 10 on dominance. The comparison of 4/7 with 6/13 shows only a slight (and not statistically significant) tendency for higher-dominance individuals to be secretaries since World War II. Thus there is no general correlation of time period with dominance rating for the secretaries, and the hypothesis that personality ratings are confounded by increasing role requirements for dominance can be ruled out in this case as well.

However, there is evidence that different role requirements may affect the measure of extroversion. Of 12 presidents, 9 are in the top half of the extroversion rankings and only 3 are in the bottom half. Of the 20 secretaries, only 7 are in the top half of the extroversion ranking and 13 are in the bottom half. So secretaries tend to be scored as more introverted, presidents as more extroverted. The question is whether this is because of true *personality* differences or because of *role* differences. My own inclination, based on the fact that occupants of both the presidential and secretarial roles have shown varying behaviors (e.g., Presidents Wilson, Coolidge, and Hoover were quite introverted) is to attribute such scores exclusively to personality variations, especially so because the rating scale (reproduced in the notes to Table 2) placed weight on use of leisure time and degree of warmth rather than on role-specific behavior like presidential campaign activities and handshaking.

III

Results of the analysis of the data are presented at the conclusion of Tables 2 and 3. If one accepts the personality scoring as valid, and if one accepts the cases presented as an unbiased listing, they show strong and statistically significant support for both hypotheses. In 78 percent of the cases involving issues of the use of force the difference along a personality dimension–everyday dominance of subordinates–is consistent with (and, interpersonal generalization theory would argue, explains) the observed direction of policy disagreement. In 84.6 percent of the cases involving issues of inclusionary initiative toward the Soviet Union or Soviet bloc the difference along a second personality dimension, extroversion, is consistent with (explains) the observed direction of policy disagreement.

These percentages are strikingly high, unusually so for a single variable in social science research, and it will be well to look at the cases again to see how sensitive the analysis is to various aspects of their makeup.

The most obvious fact about the force cases is the strong support given the first hypothesis by Theodore Roosevelt. In 10 instances amounting to 15 disagreements Roosevelt's high dominance score and policy views give support to the theory in 13 of them. Removing Roosevelt leaves 38–13 = 25 cases in support and 11–2 = 9 opposed: 74 percent support, only a slight change.

A second and more subtle characteristic was discussed in a footnote to Table 2. Since I assumed there were multiple intra-administration forces making for policy agreement, I believed it was plausible, when indicated by historians, to think of some individuals as architects and others as "going along." Hence, I scored disagreements only between an outside critic and a principal policy architect, not between a dissenter or outside critic and those members of an administration who tended usually to defer in most areas of foreign policy to a principal architect. This means, for example (in the post-World War II period), that dissents by Kennan are scored against Acheson but not Truman, that dissents by Kennan and Acheson are scored against Dulles rather than Eisenhower, against Johnson or Kennedy rather than Rusk. But Kennan, Acheson, and Stevenson are scored as more dominant than Eisenhower or Rusk; if the "going along" category were to be disallowed, the picture becomes less supportive of hypothesis 1: in case 1 the Roosevelt-Day disagreement would be confirming; but in case 7 the Root-Taft disagreement would be disconfirming; in case 17 there would be a disconfirming instance where a Rusk (Assistant Secretary of State)-Kennan disagreement is counter to the theory. In case 18 a Truman-Kennan disagreement would be confirming but a Kennan-Eisenhower disagreement would be disconfirming. In case 20 the Acheson-Eisenhower disagreement would be disconfirming, but in case 21 it would be confirming. In case 23 the Acheson-Rusk disagreement would be confirming. In case 24 the Stevenson-Rusk disagreement would be disconfirming. In case 25 the Acheson-Rusk disagreement would be disconfirming. In case 27, the Rusk-Kennan disagreement would be disconfirming. In case 28 the Rusk-Kennan disagreement would be disconfirming. So, disallowing the "going along" category would add 13 comparisons of which 5 would be confirming and 8 disconfirming. This would change the totals to 43 confirming cases and 19 disconfirming cases or 69 percent agreement (still statistically significant).

I would argue, however, that the "going along" judgment is realistic, and should be retained. The traditional historian's judgments that Day, Truman, Eisenhower, and Rusk often took a back seat is supported by their ratings as being of lower dominance. But it is true, in general, that "principal policy architects" tend to be high-dominance personalities. And those who "go along" tend to be of lower dominance. So, scoring in the way I have (necessarily excluding low-dominance individuals who agree to use of force), does tend to increase slightly the number of confirming cases.

One might wish as well to drop the confirming Robert Bacon citations in cases 4, 8, and 9 on the argument that he had to dissent because his superior Root dissented. My impression of Bacon is that he was more an independent actor than a lackey of Root. But support for the theory would not be significantly reduced by removing these cases.

From the earlier discussion there still is one important rival hypothesis, not ruled out by the research design, to be considered, namely that secretaries of state "role specialize" in advocating nonmilitary options. If we take the 20 disagreements involving a secretary of state, this turns out not to be the case: in 10 (or 50 percent) the secretary did oppose the threat or use of force (numbers 2, 3, 4, 5, 6, 8, 9, 12, 21, 22) but in 10 (50 percent) numbers 7, 10, 13, 14, 15, 16, 17, 18, 19, 20) he was an advocate. There is, then, no general tendency for differences in these intra-administration roles to affect the observed results. This major rival hypothesis for explaining these findings can be excluded.

An Analysis of Discrepant Cases. There are, of course, discrepant cases. Each could be considered in the search for additional variables. But, since the major interest here is exploring the viability of an interpersonal generalization explanation, let me suggest one possible variable, marked extrapunitiveness, that retrospectively seems important in the relation between interpersonal dominance and a tendency to be inclined toward the use of force. I would nominate this variable as a candidate for consideration by future researchers working with other data bases. My reasoning is as follows: in two of the eleven discrepant cases President Hoover and Secretary Stimson disagreed in the opposite direction than was predicted by interpersonal generalization theory. Hoover, scored slightly more dominant than Stimson, opposed the Secretary's desire to "put teeth" into the Kellogg-Briand pact by invoking economic sanctions against Japanese aggression in Manchuria. Stimson also opposed disarmament proposals which Hoover advocated. In a third case, former Secretary Elihu Root disagreed with an ultimatum issued to Chile by Secretary Knox. In other cases former president Theodore Roosevelt, scored exactly as dominant as President Wilson, was critical of Wilson's "feeble" Mexican policies and Wilson's reluctance to enter World War I at an earlier date. Two additional cases found the slightly less dominant former Secretary of State Dean Acheson urging a harder line in the 1961 Berlin crisis and disagreeing with President Kennedy's decision to use a quarantine rather than an air strike during the Cuban missile crisis.

In each of these seven cases the men involved are close to each other on the rating scales. A revised conceptualization of interpersonal dominance would probably have reversed relative positions so that these cases would, as well, support the generalization hypothesis. In particular, special weight to tendencies to be extrapunitive or to "enjoy a good fight" would increase the dominance ratings of Theodore Roosevelt, Philander Knox, Henry Stimson, and Dean Acheson. All of these men were somewhat more noted for these characteristics than were Elihu Root, Woodrow Wilson, Herbert Hoover, and John Kennedy. For example, Theodore Roosevelt was always sounding-off against someone or something. Elihu Root thought Knox's temper was decisive in the Chilean case:

> Knox was a pappery sort of fellow. He got mad very easily. He did mix into things too much. He got mad at that old pelter of a case (the Alsop claim against Chile); that made no end of trouble.... The arbitration practically decided in favor of Chile in an amount she was ready to pay. Without saying a word to anybody, Knox took it up and gave them an ultimatum–ten days.[14]

Secretary Stimson was noted for his outbursts of temper; he would occasionally have fits of anger in which he threw books and papers at his secretary. Acheson was noted for his studied arrogance, his rapier wit, his aversion to "suffering fools gladly," his liking for a good fight. By contrast Elihu Root and Herbert Hoover were highly self-controlled, generally even-tempered, and disinclined to devastate an opponent. Wilson was a fighter–but less drawn to it as a pastime than the Rough Rider. Kennedy, I think, was less inclined than Acheson to enjoy a scrap, more interested simply in winning.

Still, even if these modifications were made, interpersonal generalization theory as formulated here would not explain all force disagreements. There would be four force cases (two involving Lansing and World War I, an Acheson-Dulles disagreement where Acheson wanted a harder line in Berlin in 1958, and George Kennan's advocacy of American troop movements to Europe at the time of the Soviet invasion of Czechoslovakia) which would still not be explained by a single variable.

IV

Until this point, to rule out alternative hypotheses, comparisons among these men have been restricted to assessing the direction of their differences at the same historical moment. But the two policy tendencies studied may interact in specific situations, and collapsing the earlier 16-cell array of subjects into a four-fold table (Table 4) gives a basis for speculating about a more general psychological typology of orientations toward America's role in the international political system. Since my view of policy making is that multiple personality (situational) factors interact I suspect that such a typology may ultimately be more informative than single bivariate hypotheses in the broader task of giving an empathetic account of the personal coherence which may underlie a man's policies. It is obvious, for example, that people with different personality traits may favor similar policies–if so, however, their rationales may still differ, and one point of the exercise is to provide some general impression of what these differences may be. In each of the four cells (which I have labeled with a summary word or phrase) there may be one or two who do not fit the generalizations offered here–I was guided primarily by those in the extreme (original corner) cells. As well, there is obviously a small *N* in each cell, too small for any systematic test or study to be conducted conclusively. It will probably be another century or more before there are sufficient subjects to make generalizations or tests with statistical rigor. But for the reader or future researcher who is not averse to impressions, let me propose the following ideas.

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[14] Philip C. Jessup, *Elihu Root*, Vol. 2 (New York: Dodd, 1938), p. 250.

The *Bloc Leaders* and the *World Leaders* are, of course, those who are rated high in their tendencies to dominate their subordinates in their everyday lives. They differ from one another in their degree of introversion or extroversion. An example of a *Bloc Leader* (high-dominance introvert) would be Secretary of State John Foster Dulles; an example of a *World Leader* (high-dominance extrovert) would be President Theodore Roosevelt.

The *Bloc Leaders* tend to divide the world, in their thought, between the moral values they think it ought to exhibit and the forces opposed to this vision. They tend to have a strong, almost Manichean, moral component to their views. They tend to be described as stubborn and tenacious. They seek to reshape the world in accordance with their personal vision, and their foreign policies are often characterized by the tenaciousness with which they advance one central idea. Examples would be Woodrow Wilson's "puritanical" Mexican policies and his advocacy of a League of Nations, Elihu Root's dogged efforts to negotiate arbitration treaties and strengthen the rule of international law, Charles Evans Hughes' advocacy of the World Court, Herbert Hoover's pursuit of disarmament, Secretary Hull's determined efforts for reciprocal free trade, Secretaries Acheson's and Dulles' dominant concern with containing what they perceived as the Communist menace.

The World Leaders, more so than the Bloc Leaders, seem to want to be leaders of the entire world rather than just one bloc of the ideologically acceptable. They too have a tendency to use military force. But in general they are more flexible and pragmatic, more varied in the wide range and scope of major foreign policy initiatives. They want to lead rather than to contain. They advocate change, seek to stir up things globally—for example: Theodore Roosevelt getting his hand into arbitrations to end the Russo-Japanese War, the Algeciras Conference, and so forth; Franklin Roosevelt planning for the Four Policemen in the post-World War II period; President Kennedy's Alliance for Progress, Peace Corps, Congo operation, dispatching advisors to South Vietnam, and initiating efforts toward rapproachment with the Soviet Union; Lyndon Johnson's bridge-building with Eastern Europe, the Glassboro summit, the SALT talk initiatives. The *Bloc Leaders* seem relatively preoccupied with themes of *exclusion*, the establishment of institutions or principles to keep potentially disruptive forces in check. The *World Leaders*, while they share some of the same concerns, are relatively more interested in *inclusion*, initiating programs and institutions for worldwide leadership and cooperative advance on a wide range of issues.

People of low interpersonal dominance, the *Maintainers* and the *Conciliators*, are less inclined toward willful and major projects of reshaping the world political system. One element of this (which has already been documented) is their lower inclination to advocate the use of military force. Yet both groups also differ significantly from one another.

Those of low interpersonal dominance who are introverted, the *Maintainers*, evidence a common theme in their foreign policy orientations: a holding action for the status quo.[15] An

Table 4. A Possible Typology of Orientations to the International Political System

	Introvert	**Extrovert**
High Dominance (Reshape)	*Bloc (Excluding) Leaders*	*World (Integrating) Leaders*
	Dulles, Wilson, Hoover, Hughes, Stimson, Acheson, House, Hull, Root.	Johnson, T. Roosevelt, F. Roosevelt, Byrnes, Hopkins, Kennedy.
Low Dominance (Persevere)	*Maintainers*	*Conciliators*
	Herter, Kennan, Marshall, Colby, Knox, Lansing, Coolidge, Day, Sherman, Kellogg, Rusk.	Bacon, McKinley, Stettinius, Stevenson, Truman, Bryan, Taft, Eisenhower, Hay, Harding.

Note: Typology derived by collapsing boxes from Table 1. Characterizations are better thought of as continua than dichotomies. Dulles and Wilson, for example, are more the "ideal type" Bloc Leaders, as are the men from the other corners of the original 16-cell array.

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[15] Thus among the low-dominance individuals the introverts (Maintainers) should be more likely to use force to maintain the status quo balance.

extreme case is Calvin Coolidge who mentions not one word about foreign policy in his autobiography. He was content to leave it in the custody of his secretary of state. Somewhat more representative of this group is Dean Rusk of whom Arthur Schlesinger comments: "If the problem were an old one, he was generally in favor of continuing what Herter or Dulles or Acheson had done before him. If the problem were new, it was generally impossible to know what he thought."[16] Another example would be Secretary Kellogg who, as one scholar notes, "kept the shop running, but few new goods were put on the shelves." I think George Kennan is exemplary of this group as well–his emphasis, for the past 30 years, has centered on maintaining those four out of five key industrial areas of the world now in friendly hands in those hands.

In the final category are the *Conciliators,* the extroverted individuals who have relatively egalitarian relations with their nominal subordinates. My impression is that they tend to respond to circumstances with the sympathetic hope that accommodations can be negotiated. They are not inclined to reshape the world in accordance with a grand vision. But they do seem less comfortable with only the status quo than are the *Maintainers.* They seem more flexible, more hopeful, more open to change. They also seem–in historical perspective–to lack consistent and strong willpower, to be humane but peripheral footnotes. They are the Adlai Stevensons of history. They are backed into a Spanish-American War (McKinley); policy carries the imprint of a Hughes, Acheson, or Dulles (rather than a Harding, Truman, or Eisenhower); they languish unhappily at the United Nations during the Vietnam War (Stevenson). Many of their own policy initiatives seem quickly lost: Hay's paper Open Door policy in China, Eisenhower's Open Skies and Atoms for Peace plans.

To my mind a striking cleavage among all of these men occurs between the introverts and the extroverts. The introverts seem to be drawn to the ideal of a world system operating by *impersonal mechanisms.* During the interwar (post-World War I) swing of the international political system into legalism, this allegiance adopted the superstructure of international law. More recently and at other times a balance of power has been their normative model. But, whether the vision has been alliance of the righteous or separate geographical spheres of influence, the underlying theme has been an ideal of boundaries and impersonal mechanisms for maintaining those boundaries. It is as though these people sought a world order that was less personally engaging, more impersonally and automatically controlled. By contrast, the extroverts seem to transcend (if that is the correct word) only a balance of power orientation. They are more interested in involvement and collaboration. A paradigm case of similar policies favored for these different reasons would be the differing emphases of President Wilson and Secretary Stettinius. Wilson saw the League of Nations as dealing with international aggression by invoking moral rules and sanctions against the transgressor. Secretary Stettinius saw the United Nations operating more as a forum for discussion and negotiation. In the first case a cold, aloof, introverted person sought a mechanism to impose a solution by imposing a principle. In the second case a warm, egalitarian, extroverted person sought an institution for mutual give-and-take.

Conclusion

Psychohistory studies have their share of methodological problems. But by existing standards the present study, I would submit, holds its own and the risk of error in accepting its major conclusions is small. The personality ratings are based on explicit criteria, they converge with available independent measures of similar traits, and their variation is not a product of the subject's role. The listing of cases, while extensive, may not be exhaustive but it did pass several independent checks for bias. The design, by making comparisons at the same point in time, ruled out major alternative explanations, and the data ruled out the remaining rival hypothesis of policy view as a function of intra-administration role. The data are strongly supportive (especially for hypothesis one) and their statistical significance is markedly insensitive to various objections. And the theory stands as consistent with other findings using other subjects, particularly with the State Department study cited earlier which tested interpersonal generalization theory (and found support for it) among a random sample of American diplomats.

One might be reassured if rational choice (and the possibility of intellectual influence on a president) prevailed in American foreign policy decision making, but that view appears, in major ways, to be a fantasy. The good reasons which are sold to the public–and which have been bought by many diplomatic historians and by some political scientists–are not a complete story of the real reasons. Lasswell's pioneering formulation of self-expression is at least partially correct on a frighteningly large scale: personal motives systematically displaced onto policy issues with the results (e.g., war against a small country) rationalized as being in the nation's interest.[17]

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[16] Arthur M. Schlesinger, *A Thousand Days* (New York: Houghton, 1965), p. 435.

The data point toward two further conclusions. First, there are structural sources of war in the nature of the American political system; second, there are structural sources of potential error in the decision to go to war.

The structural source of war and hard-line foreign policy lies in self-selection and political recruitment patterns which, as we have seen, frequently elevate to high office in America high-dominance individuals with greater personal predispositions to threaten or use force. The data do not indicate any historical trend toward diminution in this character of the American political system.

Second, it is possible that these personalized decisions for war may end badly (e.g., the Bay of Pigs, Vietnam) unless there is a fortuitous coincidence between personal predispositions and the nature of external reality. We cannot be confident that there will always be happy outcomes if people enter a laboratory and mix chemicals self-expressively and according to personal style and emotional predisposition.[18]

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[17] Harold D. Lasswell, *Psychopathology and Politics* (New York: Viking, 1960).

[18] See the elaboration of these conclusions in Etheredge, *A World of Men* (to be published in fall, 1978) and Lloyd Etheredge, "Hardball Politics: A Model" (forthcoming). For supporting evidence from anthropology that societies in which males are ambitious and competitive are more likely to go to war, see David Levinson, "What Have We Learned from Cross-Cultural Surveys?" *American Behavioral Scientist*, 20 (1977), 757–92.

Military Advancement: The Visibility Theory of Promotion

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This article examines the dynamics of military promotion and explores its consequences both for individual officers and for military organization. The central argument is that performance, while a necessary standard for acceptability into a rather large pool of officers from which the elite will emerge, is nonetheless a minor influence on promotion and becomes even less discriminating as an officer's career progresses, whereas visibility—the extent to which an individual has developed contacts with peers and superiors who can influence his movement in the organization—begins moderately and eventually becomes the dominant influence. To present this argument, the article first examines the prevailing concepts of performance and seniority and outlines an expanded model of the promotion process. The component variables of that model are applied to three separate military career levels—junior, middle-grade and senior officers—and are traced longitudinally through the entire officer career pattern. Finally, the impact of contextual factors on the model, particularly the differences between wartime and peacetime, is discussed and illustrated with data from general officer promotion in the army. The implications of this theory are significant for civil control of the military. Through the control of access to the elite nucleus represented by the promotion system, especially with the emphasis on visibility, the military is able to sustain a high degree of autonomy. By the time Congress or other civilian actors are able to exercise selection of the military elite, the promotion system has already preselected those individuals who most highly represent military values held by preceding leaders.

The central issue between the two prevailing views of U.S. military professionalism is the degree of institutional autonomy that should be encouraged. Huntington[1] argues for what one author calls the radical tradition of professionalism.[2] This view emphasizes the isolation and autonomy of the military, based on a professionalism that demands obedience to civil authorities but allows complete control over internal organizational matters. Janowitz,[3] on the other hand, supports the tradition of pragmatic professionalism.[4] That tradition denounces autonomy and emphasizes instead integration with civilian society, and even participation by civilian officials in the formulation of professional standards and the evaluation of performance.

In both cases, a critical element affecting institutional autonomy is the system of promotion—the selected advancement of individuals through the organizational hierarchy. Hence, in support of pragmatic professionalism, Janowitz has identified the military promotion system as "a crucial lever of civilian control."[5] From the Huntington perspective, the promotion system can be seen as an equally crucial lever of organizational self-control or autonomy, depending on the degree of influence which can be exercised by individuals outside the organization. Yet, despite its relationship to the question of military autonomy, the promotion system has received surprisingly little attention.

This article examines the dynamics of the promotion process and explores its consequences both for individuals and for military organization.[6] Our central argument is that performance, while a necessary standard for acceptability into a rather large pool of officers from which the elite will emerge, is nonetheless a minor influence on promotion and becomes even less discriminating as an officer's career progresses; whereas visibility, which begins moderately, eventually becomes the dominant influence. Thus, to replace what may now be called the "performance theory" of promotion we offer a "visibility theory" of promotion. We present this argument by dividing our article into five sections. Section 1 examines the prevailing concepts of performance and seniority, demonstrates that they are not sufficient to explain the promotion process and then outlines a broader model of that process. Section 2 applies the model's concepts to each of three general career levels—for junior, middle-grade and senior officers. Section 3 develops each variable of the model longitudinally through the entire officer career pattern. Section 4 analyzes the impact of contextual influences on the model, especially the differences between wartime and peacetime, and provides an illustration drawn from general officer promotion in the army. Section 5 concludes with a discussion of the model's implications for civil-military relations.

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[1] Samuel P. Huntington, *The Soldier and the State* (Cambridge, Mass.: Harvard University Press, 1957).

[2] Arthur D. Larson, "Military Professionalism and Civil Control: A Comparative Analysis of Two Interpretations," *Journal of Political and Military Sociology*, 2 (Spring 1974), 57–72.

[3] Morris Janowitz, *The Professional Soldier* (New York: Free Press, 1971).

[4] Larson.

[5] Janowitz, p. 353.

[6] Much of the analysis in this paper is based on evidence from both authors' participation in the armed forces. Moore graduated from West Point and served in the army on active duty for ten years, while Trout was designated Outstanding NROTC Midshipman at UCLA and served in the navy on active duty for three years. Both have since maintained active reserve commissions. In addition to using evidence gathered in their roles as participant-observers, both authors have conducted interviews with officers in all three major services who have been extensively involved in personnel and promotion matters. While the observations made in this paper are thus based on broad evidence, through both participant-observation and informal interviews, the assertions are not meant as definitive conclusions but rather hypothetical relationships subject to systematic and empirical verification.

1. Prevailing Concepts of Promotion and Seniority

Promotion is commonly treated as a function of two factors: performance and seniority. Thus, the more senior an individual, and the better he has performed relative to others in the organization at the same level, the more likely he is to be promoted. While seniority differs from performance, the two factors are nevertheless related. Seniority, for example, implies experience. And within broad limits, the more experienced an officer the more capable he is of performing various military tasks. A second lieutenant is thus presumed incapable of commanding an army division and an ensign a frigate, no matter how talented these junior officers might be.

Of these two factors, however, performance is taken as the more discriminate means of selection. In this view, while seniority divides the officers into various "year groups," performance determines who in each year group will emerge as the elite. Thus, the most important part of any military promotion system is considered to be the performance evaluation. But performance is exceptionally difficult to evaluate. Unlike seniority, where "time-in-service" provides an inherently objective criterion, evaluations of performance rely on criteria that are less easily measured. Almost any reporting system can therefore be criticized for its faulty design.[7]

For the military, the problems with the performance evaluation system are concentrated in two areas: the nature of the *tasks* and performance report *inflation*. In the first case, *military tasks* are difficult to observe except at the most junior levels, even though they appear to be explicit. In measuring performance against tasks, the military is thus typical of most other bureaucratic organizations. It is, in Anthony Downs' terminology, a non-market organization, which means there is no transaction by which the value of military output can be measured (at least in peacetime).[8] This difficulty is compounded by the fact that the ultimate criteria of performance are perceived by the military to be related to combat capability. Yet, the military operates mostly in peacetime and cannot anticipate the precise conditions in which future combat will take place. So, not knowing precisely what combat skills may be required of an officer and not knowing whether the peacetime performance is directly analogous to combat performance, the military necessarily defines tasks in diffuse rather than specific terms. As such, the task definitions provide only limited guidelines for performance evaluation. As one author notes, such terms "provide at best only vague notions as to which properties and attributes are relevant. The values attained on the properties they do specify are not always observable, and the standards suggested are likely to be either unclear or conflicting."[9] Consequently, a superior officer can and indeed must exercise broad discretion in evaluating a subordinate's performance. And that evaluation may then emerge from a set of activities that are only tangentially related to combat performance, such as personal appearance, style, ability to relate well to other people, and social acumen. Moreover, even where evaluators do not incorporate these activities in their evaluation, there is still a tendency toward faulty reporting because task definitions do not serve as a reliable guide by which to measure competence.

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[7] Chadwick J. Haberstroh, "Organization Design and Systems Analysis," in *Handbook of Organizations*, ed. James G. March (Chicago: Rand McNally, 1965), p. 1182.

[8] Anthony Downs, *Inside Bureaucracy* (Boston: Little, Brown, 1967), pp. 29–30; 92.

[9] Sanford M. Dornbusch and W. Richard Scott, *Evaluation and the Exercise of Authority* (San Francisco: Jossey-Bass, 1975), p. 147.

In the case of tasks, the military organization is thus in a quandary. It cannot define performance standards precisely, in part because most peacetime tasks are rarely directly analogous to wartime operations, and in part because the precise combat operations that will be required are not apprehensible. Yet, if the military outlines only diffuse performance goals for its officers, evaluations are bound to be "faulty" in the sense that the results will be nonstandard and conflicting, and thus unreliable and nondiscriminating indicators of future combat performance.

The second area relating to the faultiness of military performance evaluation is the *inflation* of performance reports experienced by all three major services. While report forms allow the rater to indicate where his subordinates rank along a multi-point scale, generally from adverse to average to outstanding, only a small minority of officers is ever rated as low as "average." It is common to find as many as half of the officers in a given year group receiving maximum performance reports. Such inflation would not necessarily be "faulty" if inflation is uniform across the raters, and if sufficient differences still remained between officers to discriminate among them. However, neither of these conditions is met. As already indicated, too many officers receive maximum ratings to retain meaningful discrimination. And the degree of inflation each rater builds into his report is not uniform. Without access to the wide range of reports, raters do not know how much to inflate their evaluations so that a "true" picture of their subordinates can be presented. Each rater's guess then tends to be different from that of another. Some officers, moreover, are less disposed than others to engage in the inflation game and instead abide by the rating instructions as much as their own superiors will allow. Thus, the performance report necessarily reflects as much the personality of the rater as it does the performance of the rated officer.

To correct this inflation, all three services have modified their report forms over the past several years, but without lasting success.[10] Such a response instead seems to reinforce the problems of performance evaluation. The pattern tends to be the same. At first, many raters try to abide by the stricter guidelines, although a substantial minority will continue to inflate reports. Soon it becomes clear to the stricter raters that inflation is necessary if their subordinates are to remain competitive with officers receiving the inflated ratings, and eventually most raters will adopt an inflationary factor. Thus, the very process of changing forms often creates new inequities among officers' reports. Those raters who initially try to follow the new system will submit reports vastly different from those they would have submitted had they known that once again inflation would dominate.

While the services treat inflation as a consequence of inadequate report forms, the causes of this inflation are probably endemic to the structure of the organization itself. The military organization is large, dispersed, and segmented, while the promotion system is centralized. Thus, although performance reports are prepared in the field, they are applied to promotion (and to rotation of assignments) at the center—by officers who, for the most part, do not personally know the candidates for promotion. The impersonal nature of this system almost inevitably leads to inflation. The rater does not know who will see his report, or how it will be interpreted. The officers reviewing the reports do not know the individual characteristics of the rater and find it difficult to know for sure how well an officer has performed merely on the basis of a written report. If one rater, for example, feels obligated to mention slight problem areas of his subordinates while another rater fails to do so, is that because subordinates under the second rater have no weaknesses? Or has the first rater already softened a description so that problems which appear to be slight are really serious indeed? The reviewers cannot know what the differences between these reports really mean, and thus are forced to compare reports as though they were written by the same rater. As a consequence, raters tend to avoid any negative aspects of a subordinate's performance, and most officers receive reports that should apply to the elite few.

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[10] The most recent and most radical change has occurred in the air force. After dissatisfaction with the inflationary tendencies in the Air Force OER, a new form went into effect in 1975 which imposes quotas on each rater requiring distribution of the rated officers across the entire range of the rating scale (e.g., no more than 22 percent of officers rated can be placed in the top block). Air force officers familiar with the details of this new system, however, have already expressed doubt about its effectiveness in eliminating inflation. (See, for example, Major John D. Jacobcik, "The New OER System and Behavior," Research Study Submitted to the Faculty, Air University, Maxwell Air Force Base, May 1976).

It seems clear from the foregoing discussion that reliance on seniority and performance to explain the system of promotion in the military is insufficient. Seniority is discriminating at only the broadest levels. And performance reporting is faulty or invalid. The literature concerning military careers partially acknowledges these shortcomings by suggesting other factors for consideration. Source of commission, for example, is one of the most prevalent influences offered as a determinant of elite status in the military. Still other less formal factors are also mentioned, such as getting the right assignment at the right time, proper use of existing informal lines of communication or even "organizational accident."[11] There is a tendency, however, to treat these other factors not for their direct impact on promotion, but rather as indirect influences on performance. Most often they are considered as "informal" supplements to "formal" performance evaluation, so that in any hierarchy of variables, performance tends to remain in a superior position.

In contrast, the following model examines these other factors as direct influences affecting an officer's opportunity for promotion throughout his career. The model consists of seven independent variables: (1) seniority, (2) source of commission, (3) performance report, (4) actual performance, (5) billet, (6) chance, and (7) visibility.

Of the first six variables, *seniority* refers both to time-in-service as a commissioned officer and to time-in-grade. In the system of promotion, *source of commission* accounts for the important distinction between graduates of the service academies (the Military Academy, the Naval Academy or the Air Force Academy) and nongraduates (e.g., ROTC, ROC and Officer Candidate or Training School). The next two variables draw an important though fine distinction which should be observed carefully. *Performance report* refers to the official written evaluation of an individual officer's performance by a superior officer (i.e., the fitness report or the officer efficiency report). A separate variable, *performance*, is then used to refer to an officer's actual behavior with regard to organizational goals. Actual performance, which discounts matters of style, may or may not be indicated in an officer's performance report. More likely, the report will reflect such considerations as an officer's discretion or sociability, or perhaps personality traits deemed objectionable by the superior, or even the evaluating officer's predetermined expectations concerning an officer assigned to him (the so-called "water-walker" whose reputation precedes him to a billet). In contrast to actual performance, therefore, the concept of performance report includes the effect of the evaluator's idiosyncracies (listed separately in the accompanying diagrams for the sake of clarity). The fifth variable, *billet*, designates each specific job assignment an officer holds; a series of billets constitutes an officer's career pattern. Next, *chance* is included to account for "organizational accident," the fortuitous or non-fortuitous intervention of circumstances.

The final variable in the model is *visibility*, which relates to observation of performance.[12] Since all tasks are not equally observable nor all observers equally influential, visibility varies to the degree to which an individual is in a position to be observed by peers and superiors who can influence his movement in the organization. Visibility then has two aspects. One reflects the position occupied; the other reflects the contacts established while occupying the position. Thus visibility can be examined both at a single point in time and in terms of its cumulative effects.

Both aspects of visibility have been recognized in the military setting. Janowitz, for example, has noted with regard to the first that "a candidate's ability to rise farther depends on the particular assignment which will permit him to *display* his talents and make a *conspicuous* contribution."[13] With regard to contacts, Janowitz later notes that *"in building a reputation* each younger officer has the task of *coming to the attention of important superiors."*[14] Visibility then applies where certain billets have a premium over others. And it applies to the system of contacts established cumulatively by an individual officer. Visible officers transcend the performance reporting system, since their activities will be monitored relatively frequently by influential or potentially influential officers other than their immediate reporting seniors. Moreover, the criteria by which their activity is evaluated will be increasingly influenced by expectations about their future potential, rather than simply the outcomes of their currently assigned duties.

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[11] Janowitz, p. 125.

[12] Donald I. Warren, "The Effects of Power Bases and Peer Groups on Conformity in Formal Organizations," *Administrative Science Quarterly*, 14 (December 1969), p. 544. See also Dornbusch and Scott, pp. 149–55.

[13] Janowitz, p. xxxi, emphasis added.

[14] Janowitz, p. 145, emphasis added.

2. A Model of Promotion

The relationship among the seven variables of the model changes over time as an officer advances through the promotion system. This change can be illustrated by depicting the model at three representative career stages—junior officer, middle-grade officer and senior officer. Figure 1, for example, pertains to the junior level. The arrows indicate the relative influence of each variable on the others. The succeeding diagrams will show how the variables change.

It is apparent from the first diagram that only two factors directly affect junior officer promotion. And it is apparent that only one of these, seniority, has a significant effect. This reflects the fact that promotion at this level is virtually automatic, depending only on "time-in-grade." There are of course instances where individuals are perceived and reported to be so incompetent that they should not be promoted even to 0–3. Thus there is a small arrow to indicate that such reported incompetence screens out a few officers from the otherwise automatic process.

The remaining variables have only an indirect and minimal effect on junior officer promotion, since the only connection is through the performance report. The strongest impact in this connection is shown to be billets. However, the actual consequences are latent. An officer will be advantaged later in his career by the overall pattern of job assignments, but a prescribed assignment at the beginning will not accelerate promotion at the junior officer level. The later advantage of billet is indicated in the diagram by the relationship to visibility as well as to performance report. At this stage of an officer's career, billets then have the important role of building visibility and establishing a prominent record of performance reports. An aide to an admiral or general, for example, will rarely receive a sub-par performance report. And, in a billet rated above the ordinary expectations of a junior officer's rank, even par will appear as an outstanding report. Thus, for example, an army first lieutenant able to command a company, normally a captain's billet, will probably receive an excellent performance report simply because he is occupying a billet designated above his rank.

Occupancy of such billets tends then to take on the character of a self-fulfilling prophecy. One might reply, of course, that only superior junior officers would be given the opportunity to hold an assignment above their rank or to occupy an aide's billet. However, as indicated by the thin line between performance and visibility, this position seems untenable. First, reiterating the general case, performance is difficult to measure. Second, at the junior officer level there appears to have been insuf-

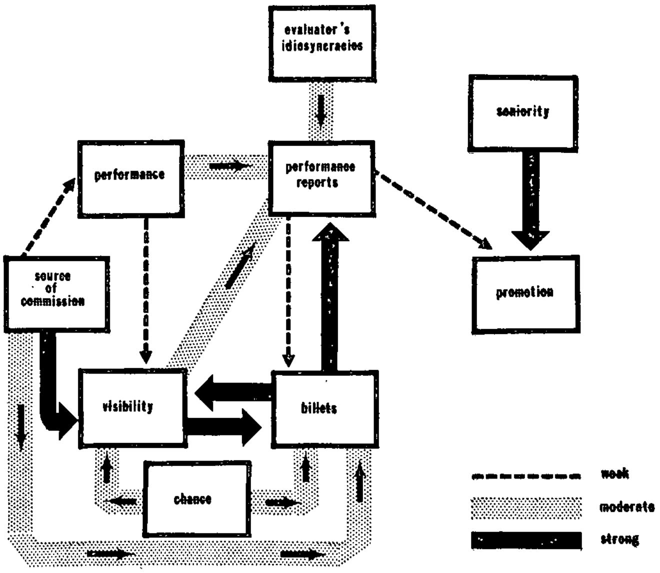

Figure 1. Junior Officer

ficient time to accumulate enough actual evidence of performance to support use of that criterion for assignment. A junior officer is more likely to get the prized billet because he is visible to the person making the assignment, or because, by chance, he is available when an aide or command slot is open, rather than because of superior performance.

What is suggested here is that a high-visibility billet is crucial for an officer at this stage of his career. In the army, command should be accompanied by such highly visible billets as secretary to the general staff or general's aide. In the navy, duty as division officer or department head on a combatant ship should be accompanied by time on an operational staff headed by an admiral or on a squadron staff. The point is that while the command or equivalent shipboard billets are necessary for any junior officer who expects eventually to emerge at the top of the military hierarchy, those prescribed billets are not sufficiently visible in themselves to fulfill requirements for promotion at later stages. A junior officer should occupy such prescribed billets only within the minimal personnel guidelines provided by his service. Beyond that point the added experience only detracts from opportunities to increase his visibility. A junior officer who points proudly to extended company level or shipboard experience, in the belief that this experience enhances his performance potential for later command, overestimates the importance of performance in the system of promotion.

That is not to say that junior officer performance has no impact on performance reports, or that visibility has immense influence on them. Performance does have at least a negative impact, as already suggested in the discussion of junior officers who are too incompetent to be promoted even to 0–3. There is also a positive effect, however, stemming largely from the relative clarity of tasks for junior officers. For example, in the navy, the task of learning to handle a ship for the unrestricted line officer, and to fly an aircraft for the aviator, are basic and reasonably clear (collisions and crashes are always taken as concrete measures of sub-par performance). And thus evaluation can be closely related to actual performance. Still, beyond such basic tasks, the difficulties in defining performance standards remain, and the influence of performance is thus held to modest proportions.

The effect of visibility on performance reports is also modest. At the junior officer level, there is simply less visibility than at succeeding levels, although a small proportion of junior officers are able to develop some degree of visibility. A few cadets, and to a lesser extent, midshipmen, for example, become well known to their officer instructors and tactical officers who, after their academy assignments, may aid the new academy graduates in obtaining prized billets. Sons of high-ranking active duty officers may enjoy a similar advantage.

Visibility at the junior officer level is affected by four influences—performance, chance, source of commission and billets. The weak influence of performance, discussed above, is attributable to the problem of defining standards and to the relative lack of evidence at such an early stage. Chance may play a greater role. The son of a career military officer is likely to be better off as a junior officer for knowing his way around, particularly if he is the son of a general or admiral and known to his father's friends (or by his father's reputation). Social contacts in general, such as those acquired through prominence in sports, may also contribute to the early visibility of junior officers. But, of the four factors affecting visibility, only billet, which has already been discussed, and source of commission have significant impact.

Source of commission has several effects. With regard to visibility it is quite important. An academy graduate, unlike the non-academy junior officer, enters the service carrying four years of prior contacts with peers and superiors. The majority of academy instructors (those in uniform) are regular officers with combat specialties who will rotate back into appropriate operational billets. In part because of these contacts, and because such billets are limited in number, academy graduates are more likely to obtain visible billets among their early assignments. Furthermore, once a visible billet has been obtained, the likelihood of occupying another is significantly better than for comparable officers who have not occupied such a billet. Due to the effect of early contacts, source of commission may be an important factor also in that form of visibility which Janowitz terms "sponsorship,"[15] where a junior officer keeps an interested (and visible) superior informed of his career progress and, as a result, is helped at critical junctures in acquiring further prized billets.

Source of commission has a lesser, though still moderate, effect on billets. Academy background carries a special aura. For example, a flag or general officer may prefer an academy graduate as an aide (perhaps because common background assures like socialization, or because the social experience of the academy makes graduates appear more suitable to perform aides' duties). Additionally, academy graduates tend to have greater leeway in selecting a first assignment. Some, within limits, may choose exact assignments. Because of contextual shifts, to be discussed later, particular billets or particular geographical areas will be more desirable than others. Combat areas, of course, provide significant opportunities, but even in peacetime there are geographical areas, determined largely by the state and focus of international tensions, which are more significant than others. Moreover, not only do academy graduates have greater leeway in obtaining such assignments, they usually have greater and more accurate information as to what constitutes a premium billet. Informed, ambitious and perceptive officers will naturally choose to serve in a prime area. Since academy graduates are more likely to be granted their initial billet preferences, they have a significant advantage not enjoyed by non-academy graduates. Source of commission finally has a slight effect on performance. An academy graduate, having already been exposed to four years of military life, will more readily adapt to the superficial characteristics of prescribed military tasks than officers commissioned through other programs. Non-academy officers commissioned with prior enlisted service may share in this advantage, but to a more limited degree.

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[15] Janowitz, p. xxxi.

Moving to middle-grade officers, Figure 2 presents the same set of variables as they affect officer promotion at that level. The most significant difference between the promotion system for junior officers shown in Figure 1 and that for middle-grade officers shown in Figure 2 is the influence of performance report and billet. Both have strong effects on promotion. Unlike the virtually automatic nature of junior officer promotion (and therefore the strong influence only of seniority), promotion to middle grades is dependent primarily upon performance reports and upon billet. By the time they reach the 0–4 level, officers can be passed over for promotion and eventually separated because of sub-par performance reports. At this level, also, officers are eligible to be selected on "outstanding" or "below-the-zone" promotion lists because of a record of superior reports. Billets at this level are more important for differentiating among potential candidates for early promotion than for identifying officers for non-promotion. Thus, for middle-grade officers a pattern of visible job assignments, early selection for service schools at each level (of less import in the navy), and of course service in command or in an important operational staff slot become critical factors in separating the potential leaders from the pack.

Because of the influence of billets, timing assumes utmost importance in an officer's

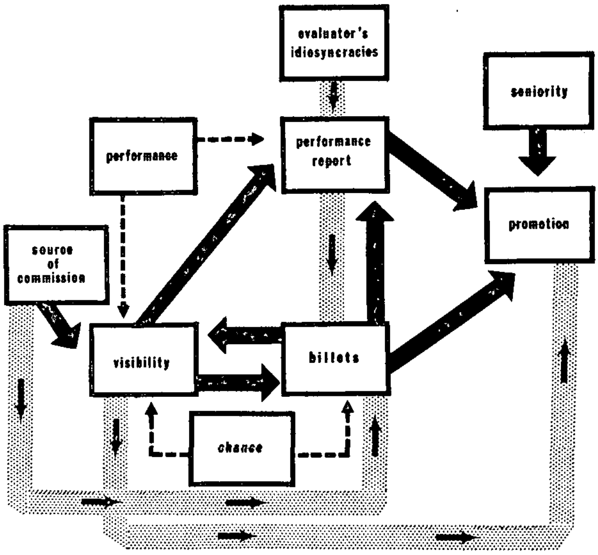


Figure 2. Middle Grade Officer

career, especially during periods of quick promotion when the short time-in-grade does not permit much leeway in meeting the criteria of a prescribed career pattern. In the army, for example, during the latter part of the 1960s, no engineer officers were selected on the outstanding list for major unless they had commanded a company and their efficiency report for the period of command had been filed by the time the selection process was begun. This meant in effect that engineer officers had to command a company within two years of being promoted to captain, or else they would not have been considered for fast promotion. Many officers who would otherwise have been considered—because of a superior record of performance reports—were in the civil schooling program getting advanced degrees and therefore did not meet the criteria. Moreover, for many of these officers, the civil schooling process led to a tour designed to utilize that schooling, followed by a mandatory tour in Vietnam. This sequence delayed assignment to the Command and General Staff College, without which they were unable to serve in some of the most important operational billets while in Vietnam (such as the S-3 or executive officer of a battalion). Hence, they found themselves at a clear disadvantage compared to contemporaries whose careers were much better timed: early command as a captain, followed by Command and General Staff College, Vietnam, and *then* civil schooling; or, if civil schooling came after the command, then a utilization tour deferred until after the Staff College and Vietnam assignments.

As Figure 2 shows, visibility now has a direct influence on promotion. This influence is particularly significant again in selecting officers for fast promotion: an officer who is known by someone on the promotion board, for example, either personally or through reputation, is much more likely to be selected from below-the-zone than an unknown officer. For below-the-zone promotions, however, the board can select only from among those names submitted by the personnel office. Thus, superior performance reports and billets remain essential just to be considered, even for the visible officer.

Another difference in the promotion system for junior and middle-grade officers is the increased importance of performance reports on the assignment of billets. This influence is manifested primarily in the selection to service schools and shore billets, although a differentiation is now clear also at the senior 0-4 level between those officers who will remain in a command track and those who will not. The overall record of an officer's performance reports will strongly influence this critical selection process.

The impact of actual performance on performance reports declines substantially for the middle-grade officer at the same time as the impact of visibility has increased. Performance declines in importance for two reasons: first, the actual job expectations for the middle-grade officer are much more diffuse so that evaluation of performance has grown correspondingly more difficult; and second, largely because it is difficult to evaluate performance, raters will tend to evaluate the "individual." As a consequence, both the past record of the officer being evaluated, as indicated by job assignments, and his reputation will strongly affect the way in which the evaluator writes the performance report.

The role of the evaluator is difficult to depict. However, a special relationship operates on the writing of fitness reports. Evaluators who are themselves outstanding officers, as reflected in the overall pattern of their own performance reports and job assignments, will tend to rate their subordinates higher than mediocre officers rate their subordinates. Both the evaluator's self-image and the billet he occupies will enhance the evaluation of those in his command. This point is quite important for junior officers, and even junior middle-grade officers. Two or three "average" performance reports from mediocre officers can devastate the chances for fast promotion of officers at this level who might otherwise have an opportunity to rise to the top level. At higher levels, the "best" junior officers are presumed to be those who work for the "best" senior officers and therefore outstanding performance reports are expected. Only the rare instance of a clearly incompetent performance (which would be extremely rare), or a personality conflict (which might occur more frequently) would break the anticipated pattern of outstanding performance reports.

The final major difference between the junior and middle-grade officer promotion systems is that, in the latter, chance has a decreased impact on billets and on visibility. The reason for this decline is that billets and visibility are both cumulative factors. After ten years or so of service, it is difficult to generate a continuing sequence of good job assignments or to develop a wide network of contacts simply by chance. Some officers do, of course, get a special opportunity in mid-career, but such occurrences are rare. A change in strategy or technology may significantly raise the importance of a particular skill. Officers possessing that skill may find that their visibility and the visibility of their job assignments rise accordingly. But such chance advantage is not sustaining. The visible billets associated with a skill will either become prized and therefore quickly available to peers, such as the nuclear power program in the navy, or they will be short-lived, such as the rise of army officers trained in helicopters or counterinsurgency during the 1960s. The real advantage lies in visibility. And the point to be stressed is that an officer who has not developed high visibility after ten years or so of service has little chance of being promoted to one or two stars, and virtually no chance to reach three or four stars.

Thus, when turning to the senior officer level in Figure 3, we observe that visibility has become the major element of the promotion system. By the time an officer is at least a senior 0–6, three factors have a direct and significant impact on promotion: visibility, billets and performance reports. But visibility, which strongly affects both the other factors, acquires the greatest overall significance. At the senior level, visibility affects the billets an officer receives and the resulting performance reports in the same manner as described for middle-grade officers. However, visibility also affects senior officer promotion directly, and much more strongly than is the case with middle-grade officers. At the highest levels, the number of officers is relatively small. Senior officers serving on promotion boards are thus likely to know many of the officers being considered for promotion. Individual reputation and personal interactions therefore tend to dominate. Nearly all senior officer performance reports are of extremely high caliber, and the minute differences among them are not in themselves sufficient to distinguish those who should be promoted from those who should not. The exception here, of course, remains the rare bad performance report, perhaps arising out of a personality clash. As is the case throughout, such a report can significantly detract from a subordinate officer's chances of future prized billets and promotion. However, at this level in particular, a bad report would probably be attributable to the evaluator's idiosyncracies rather than to performance.

For senior officers, in fact, performance ceases to be an influence in the promotion process. Any job at this level is so diffuse in its requirements compared to job assignments at lower levels that evaluation of performance against objective and reliable criteria is virtually impossible. Moreover, senior officers are expected to do well. They are, after all, according to their performance reports, the best in the service; unless there are personality conflicts, that high expectation acts again as a self-fulfilling prophecy.

Chance remains of some importance in affecting visibility, but no longer affects billets. With respect to visibility, all officers at the senior level will by this time have developed a network of relationships and a reputation among other senior officers. The greater the

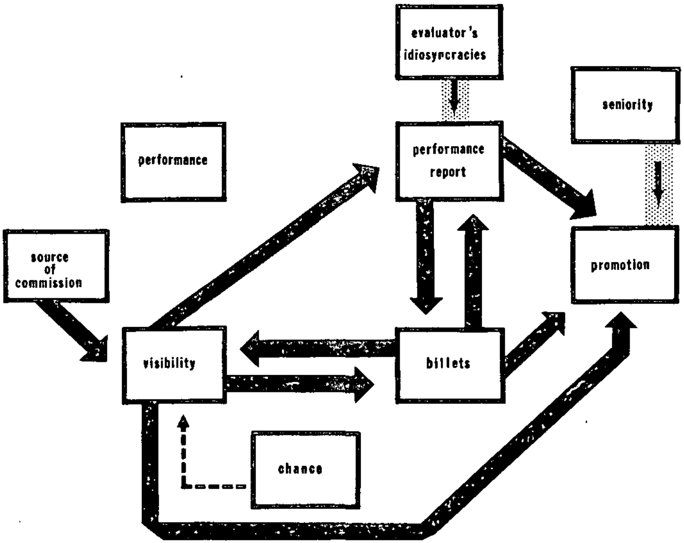


Figure 3. Senior Officer

extent of this network, the greater an officer's visibility. Of course some contacts may be especially fruitful, so that an officer whose visibility may be otherwise low (because of relatively few contacts) may nevertheless be greatly aided by one particularly influential contact. Which contacts may be especially influential? The impossibility of answering that question explains why chance retains some impact on visibility at the senior officer level.

Chance no longer affects billets, however, because job assignments at this level are necessarily few and are therefore handled with much greater attention and concern for the opinions of other senior officers who may be affected by the assignment. A senior officer does not just happen to be available for an unexpected opening and thus move into that position, as might occur with the middle-grade officer and often does occur with the junior officer. In the first place, such openings are seldom unexpected. In the second place, in the rare emergency which leaves such a position open, a replacement can be quickly assigned from among top candidates available anywhere in the service, not just from those who are due for reassignment or are locally available. Assignment to such billets tends to rely heavily on negotiation and interpersonal communication.

Source of commission, which now has no impact on either performance or billet, is still an important influence on visibility at the senior level. Officers who graduate from the service academies maintain contacts among themselves, and especially their classmates, through periodic social functions. These gatherings, as well as loyalty to one's class and alma mater, give the academy graduate a continuing source of visibility not available to the non-academy graduates.

3. The Model in Longitudinal Perspective

Thus far, we have analyzed the promotion system broadly at each of three officer levels in the service, and in so doing have noted relative differences in the impact of the model's variables. Over the course of an officer's career, however, the influence of one or another factor does not suddenly increase or decrease from one level to another as might appear from looking at just three points in time. These differences occur more gradually. In the following paragraphs we will therefore examine each factor longitudinally to observe how changes take place throughout an officer's career.

Seniority. Seniority begins as the single most important influence on promotion. For junior officers (0–1 to 0–2, and 0–2 to 0–3), promotion is essentially a matter of fulfilling the requirements of time-in-grade. At higher levels, because officers are ordinarily considered for promotion only when their dates of rank fall within predetermined zones, seniority continues to be a factor. Beginning with 0–4, however, these zones become more flexible, allowing some relatively junior officers to be promoted ahead of their contemporaries and immediate seniors. By that point, seniority begins to decline in importance. At the 0–6 level, this decline is accelerated by the uncertainty of promotion even with long service and decent performance reports in the grade of 0–5. From 0–6 through the general and flag officer ranks, seniority still remains a criterion which helps to distinguish order of promotion and establish time of consideration; almost never does an officer jump a rank, for example, and most officers serve at least a year or two in one rank before moving to the next. Nevertheless, it is more common at the senior level to override seniority status than to abide by it.

Performance Report and Evaluator's Idiosyncracies. Performance reports are insignificant only at the most junior level, where promotions are virtually automatic (except for the extremely negative report). Thereafter, the performance report is the major factor determining key choices, including early selection for prized billets and of course selection for both regular and past promotion.

At all levels, performance reports are affected by the evaluator's idiosyncracies. The impact of those idiosyncracies is depicted in each case as moderate, a point that may not seem obvious at first. It is widely believed in the military, for example, that good officers give good performance reports, and bad officers give bad reports. One might then conclude that the idiosyncracies of the evaluator are most important at the junior level, where a potentially outstanding officer may have to serve with a bitter, passed-over superior who gets his revenge by torpedoing his subordinates' performance reports. At higher levels, in contrast, the outstanding officers follow such a different career path from their less outstanding contemporaries that the best middle-grade officers serve only with the best senior officers and would thus avoid the possibility of bad reports due to an evaluator's idiosyncracies. However, since reports at that level tend to be uniformly excellent, minor variations are crucial and can seriously detract from future prized billets and fast promotions. An evaluator need not write a disastrous report in order to seriously affect a subordinate's career. At higher levels, then, a report which is merely good or one which falls short of the maximum rating can slow an otherwise zooming career.

The impact of these idiosyncracies remains moderate, however, rather than great, because other factors act at all levels to temper outrageous reports. For junior officers, the review of reports by senior officers exercises restraint on the bitter, passed-over superior who might write too devastating a report. Moreover, promotion boards tend to ignore reports at the junior level that are significantly different from the overall pattern of reports which an officer has received. At the middle and senior levels, where one bad report can more easily devastate an officer's career, the psychological pressures against that kind of report are strong. The evaluator, after all, must also be evaluated, and it is more difficult at these levels to justify a bad report than it is a good one. In addition, as indicated at the beginning, for an outstanding officer suddenly to receive a bad or even average report will no doubt raise questions in the command and in Washington, not just about the officer, but about the evaluator as well.

Billets. A promotion selection board examines two major criteria: an officer's performance report and his career pattern. At the junior level, where promotion is virtually automatic, the career pattern is incidental. After several years of service, however, generally beginning with promotion to 0–4, the series of billets which an officer has occupied becomes more significant. In fact, unless officers have served in the requisite operational and command billets within prescribed time limits, they may not be considered for early promotion regardless of their overall record of performance reports. At higher levels, although the required billets are fewer, greater emphasis is placed on their relative importance. Thus, in the case of a high-level staff assignment for a middle-grade officer, the higher the staff, the better. It is much better, for example, to serve on the staff of the Commander, Sixth Fleet than on the staff of an amphibious command. Not only is the status of the command higher, but—as a corollary—so is the rank of the evaluator. And the higher the rank of the evaluator, the more influence the report is likely to have on promotion. This relative importance of billets is crucial even at the most senior levels. Division commands in the army, for example, are more prestigious than specialized agency commands. Furthermore, a variety of such billets is essential, for the preferred career pattern of the generalist must indicate wide experience with military affairs.

Performance. As indicated throughout, performance is difficult to evaluate in terms of organizational goals. It is true that tasks at lower levels are more clearly related to organizational goals than at higher levels so that junior officer evaluations will tend at least partially to indicate the quality of performance in the achievement of these goals. But here too, style and personality differences are reflected. These differences will enhance or dilute the evaluation of actual performance. And, of course, the role of such extraneous factors depends largely on the character of the evaluator. Hence, they are best categorized not as performance but as the evaluator's idiosyncracies.

The diagrams illustrate that performance in fact diminishes in importance: the higher the level, the less important is actual performance on both performance reports and visibility. Two major reasons account for this change: the increasing diffuseness of job expectations as they relate to organizational goals, and the decreasing variation in the quality of actual performance, since bad performers are identified at the junior level and are thereby screened out of consideration for prized billets and accelerated promotion. The vast majority of middle-grade officers and virtually all senior officers are then competent to serve in any of the billets designated for their rank. Thus, again, differences in behavior appear more as a function of style than of the quality of performance related to organizational goals.

Visibility. At every level visibility is important. At the lowest levels, it affects primarily an officer's opportunities to serve with or for high-ranking officers. Such early high-visibility billets are valuable not only because the accompanying performance reports tend to be higher and to receive greater weight than other reports due to the higher rank of the evaluator, but also because early high-visibility billets lead to subsequent high-visibility billets. Since the development of visibility is a continuous and cumulative process, officers must enter that process early or they will never be able to remain competitive with their peers.

At middle levels, therefore, a screening process has already separated outstanding officers from the rest of their contemporaries. There is by now a clearly understood hierarchy of billets at this and higher levels, and the outstanding officers will serve only in the most important billets. Thus, the "best" middle-grade officers will serve only with the "best" of the senior officers. This separation is not always discernible to the average officer who will work with these outstanding officers when they are in command positions, in the senior service schools, or in Washington.

Visibility thus becomes the major factor influencing which of the better jobs an outstanding officer will get, and the thread of sponsorship runs throughout the assignment pattern. For the average officer, each new assignment is made by an assignments officer in Washington who makes an objective assessment of what the officer needs at this stage in his career. For the outstanding officers, however, each new assignment is influenced by superiors with whom he has previously worked. He will either move with this sponsor to a new assignment or move to an assignment suggested by his sponsor, usually to work for senior officers who are especially well-known by the sponsor. Thus, visibility is an increasingly important factor in the careers of the outstanding officers which persists even at the highest ranks.

Source of Commission. Source of commission was emphasized initially for the beginning stages of an officer's career, helping the academy graduates to get an early start on high-visibility billets and thereby giving them a comparative advantage. Viewed longitudinally, however, it should also be noted that the academy association works to the advantage of academy graduates throughout their career. In the army, for example, with few exceptions only academy graduates can teach at West Point. Just as they were for the cadet, the contacts that are made during a mid-career instructor assignment are often extremely helpful for obtaining prized billets in subsequent years. Also, as mentioned earlier, class reunions, annual academy founders' day dinners, and the general pattern of acquaintanceships and camaraderie which exists among academy graduates all reinforce and expand the contacts that an academy graduate develops. At the highest levels, this academy association is refined to include the class association as well: classmates may help each other more than academy graduates not their contemporaries. The overrepresentation of some classes among general or flag rank and the underrepresentation of other classes suggests this bias among classmates.

Chance. In a social process such as promotion, chance is always a difficult phenomenon to account for. For some, chance is simply unexplained variance, the result of an incomplete model which ultimately, with perhaps more variables, would reduce or eliminate the chance factor. For others, chance may be seen as evidence of unpredictability, a random factor that cannot really be specified or dealt with. In our model, chance is a factor that operates in relatively specific ways and at relatively limited times in the military career. It represents the only area, apart from performance, where an officer may have an opportunity to exert a direct influence on his career.

At the junior officer level, chance may provide opportunity for visibility and billets. Chance affects visibility where the career path of a junior officer happens to have crossed that of a senior officer who can help him, or where the junior officer has seized upon an opening to meet important senior officers—through sports functions, for example, or through active cultivation. Examples of chance affecting billets are the unexpected opening of a command position, or the request for a general's aide from among the junior officers. Such opportunities are few, but they are much more frequent for junior officers than middle level officers.

For a middle-grade officer, a single contact with an important senior officer, though it may lead to a highly visible billet, is unlikely to compete with the succession of billets and the accompanying performance reports which an already highly visible officer has received. Only the recurrence of important, highly visible billets and the extensiveness of one's professional contacts with other outstanding officers can give momentum at that stage. Thus, the cumulative characteristics of billets and visibility militate against chance in the latter stages of a military career.

4. Contextual Influences

We have so far addressed the promotion system as if its variables operate independently of the surrounding political milieu. This of course is not the case. We must therefore consider some of the contextual influences which affect the model. The most relevant of these are the differences between wartime and peacetime. As noted at the outset, the military perceives itself primarily as a combat organization even though not ordinarily engaged in combat. War should then have an effect on the overall evaluation system, for combat assignments, particularly command, presumably represent the true test of competence. The notion of some strategists that the noncombatant, deterrent role of the military is at least as important as the combatant role does not yet seem to underlie the promotion system. As a consequence, the most prized billets at each level are still those involving actual combat. And the "best" and most ambitious officers will naturally gravitate toward combat zones seeking combat command or, if that is not available, a combat staff position.

During peacetime, the same motivation also leads officers to concentrate in one or another geographical area which may approximate combat conditions because of the level of international tensions and the size of the military commitment existing in that region of the world. The greater the tensions and the larger the commitment, the more important the area. If, for example, international tensions in Korea and western Europe are about the same, the army billets in Europe will be more highly prized than those in Korea because there are many more high-ranking officers and billets there, and thus greater opportunity to develop contacts and thereby develop visibility. Assignment to an operational command in the continental United States (CONUS) is almost never as prestigious as an overseas assignment, because international tensions are rarely significant enough to involve CONUS commands. Service in the Sixth (Mediterranean) or Seventh (western Pacific) Fleets, for example, is far more significant than that in the Third (Hawaii) or Second (east coast) Fleets, although the rotation of ship deployments tends to weaken the distinction. Occasions like the Cuban missile crisis and Dominican Republic crisis in which CONUS billets were transformed into more highly prized billets, at least temporarily, are unusual.

Whatever the differences in prestige accorded the various geographical regions in which the military is stationed, the magnitude of these differences never approaches that which exists between peacetime and wartime billets. Hence, although European billets are clearly more important in the army today than either Korean or CONUS billets (exclusive of certain Pentagon assignments), a command billet in the United States remains preferable to any but a command billet in Europe. Moreover, the latter command billet would carry only a little more weight, if any, in a review by a promotion board—nothing like the tremendous difference in importance between a combat command and peacetime command.

Consider now the importance of such contextual influences for visibility. The greater the perceived significance of an area to the potentially elite officers, the greater the number of elite officers who will go there and thus the greater the opportunity for developing contacts with other outstanding officers. Although perhaps a self-fulfilling prophecy, the choice presented to an ambitious officer is clear: follow the elites. During actual combat the basis for that choice is even more evident: the potential elites go to the combat zone and, therefore, the combat zone will provide the greatest opportunities for expanding one's network of associations. Moreover, because of the danger and intensity associated with combat, associations developed at this time will tend to be much stronger and more influential on one's career than those developed during peacetime. In this manner, the visibility afforded by the "old boy network," or the network of associations developed primarily among academy officers, can be modified by a secondary network of associations based on combat experience. For the non-academy graduate, development of this secondary network is quite important, particularly at the senior levels. If he can obtain a senior combat command, such as a battalion commander or commanding officer of an aircraft carrier, and if his performance is perceived as outstanding, he will have achieved in a short time a high level of visibility that can propel his career to the general or flag officer rank. An equally "outstanding" performance in a peacetime command would not have nearly as great an effect on visibility.

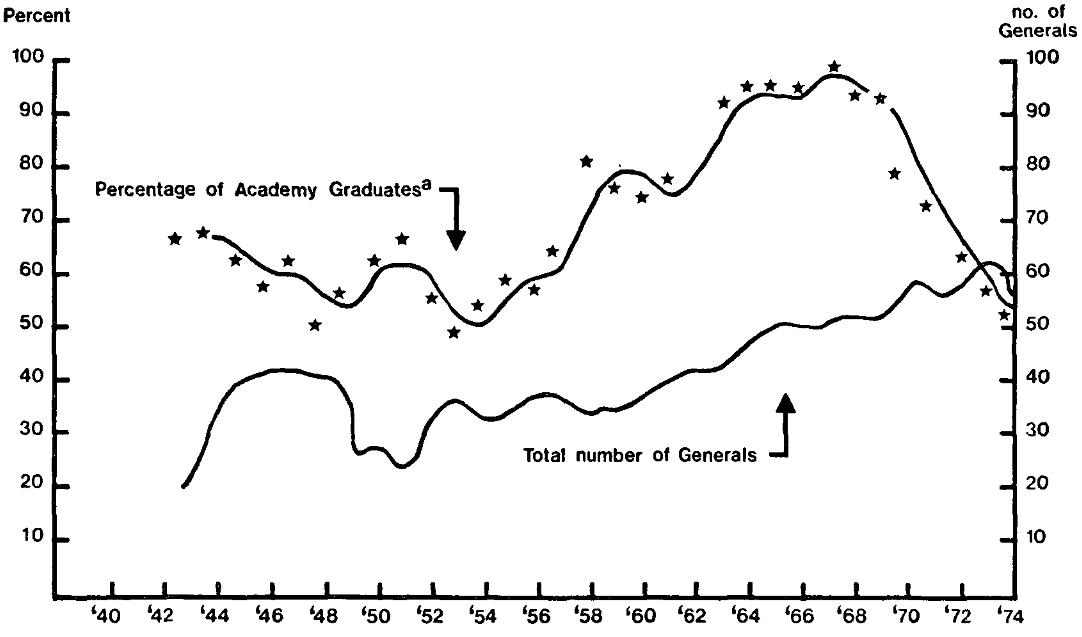
Two factors account for this difference between wartime and peacetime conditions. First, as previously described, combat command is widely believed to be the ultimate test of a military officer's competence. If an officer does well in command of a combatant ship or battalion, for example, one can hardly doubt his ability to command at higher levels. Second, the performance of a commanding officer in combat appears to be much more clearly measurable than during peacetime. Taking specific objectives, achieving high body-kill ratio or completing shore bombardment missions are quantitative indicators that are much more directly related to performance than almost any indicator available in peacetime. Thus, even apart from the aura of combat, the credibility of an officer's combat performance report is higher than an officer's peacetime report.

One might question whether non-academy officers would even get a combat command in view of the influences on their careers portrayed in Figures 1 and 2. Those diagrams, however, are not meant to suggest that non-academy graduates never obtain prized billets.

Some non-academy officers remain in a competitive status up until three- or four-star rank, although in peacetime the proportion of these officers at each succeeding rank diminishes substantially because of the cumulative visibility advantages of the academy graduates. At the level of 0–5 or 0–6, enough non-academy graduates can still obtain the prized combat command billets to get the opportunity to demonstrate their potential and thus counteract some of the visibility advantages enjoyed by academy graduates in peacetime.

The consequences of the contextual conditions just outlined increase the chances for promotion of the non-academy graduate during war, especially to the general officer or flag rank, and decrease the non-academy officer's chances for high level promotion during peacetime. Once the war is over, the network of combat associations attenuates as the highest ranking officers retire, while the primary network, dominated by the academy experience, resumes. The networks overlap considerably, of course, but only in the combat network does the non-academy graduate have an opportunity to overcome his visibility disadvantages and demonstrate a potential for general or flag officer at the three- or four-star level. One way to infer the importance of these contextual influences on promotion is to look over time for fluctuation in the percentage of generals who are academy graduates. As outlined above, that percentage should increase in peacetime because visibility operates primarily in an academy network of associations, while in wartime the percentage should decline because of the operation of a secondary network of associations based more on shared combat experiences than on the shared academy experiences. Figures 4 and 5 present data in support of these suppositions.[16]

In Figure 4, the percentage of army three- and four-star generals who are West Point graduates is plotted across time. Also plotted is the total number of such generals for each year. Note the percentage decline from 1943, when the three- and four-star ranks were first instituted, until 1948, just a few years after the end of World War II. At this time the percentage starts a peacetime increase, declines again in the Korean War, and then begins a long ascent, lasting until 1967 when all three- and four-star generals are academy graduates.[17] The decline



^a To present a smooth curve, the percentage line was drawn to represent a two-year average.

Figure 4. Three and Four-Star Generals

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[16] Data for each year were obtained from the appropriate year's *Army Register* and from downgraded copies of the "General Officer's Alfa Roster."

[17] The figures include only those generals who had never retired. Thus, LTG Hershey, Director of the

in the percentage after 1967 is precipitous, taking just 7 years to reach less than 55 percent, while it took 13 years to climb from that level in 1954 to the 1967 high.

Note that these dramatic changes occur while the absolute number of general officers varies only slightly, and certainly independently of the percentage fluctuations. One might expect that with a great increase in the number of such generals, the opportunities for non-academy graduates would increase, and thus a decrease in the percentage of West Point generals might be expected. Except for the period from 1943 to 1946, however, that hypothesis is not supported. The percentage declines from 1946 to 1953 in spite of the concomitant decrease in the number of generals, and from 1953 until 1974, the gradual increase in the number of generals is accompanied first by a large increase and then an equally large decrease in the percentage figures.

These figures are more easily explained by reference to the visibility theory of promotion than to performance. A performance theory might be used to suggest that in combat the relative incompetence of West Pointers becomes evident, and thus non-West Point officers are more likely to be promoted to the highest levels. However attractive that explanation may be to anti-elitists in the service, the difficulty in measuring performance and the impact of visibility in the promotion process are more plausible factors accounting for the percentage fluctuations. A different network of associations develops during wartime, and after a few years this new network results in a greater proportion of promotions for non-academy officers. Hence, now that the Vietnam war is over, a projection of the percentage figures into the next several years should show an upturn.

The same data plotted in Figure 4 for three- and four-star generals are plotted in Figure 5 for one- and two-star generals. And the same general pattern emerges, except that the changes in the direction of the curve occur several years before the changes plotted for the three- and four-star generals. This delay indicates that the dominant network of associations, whether based on combat or West Point, has to work its way up from the lower levels of general officer, or more likely from the ranks of 0–5 and 0–6, before it eventually affects the three- and four-star level.

The highest percentage figure for one- and two-star generals is 80 percent, significantly below the 100 percent figure of the highest-ranking generals. This difference is attributable to at least two factors. First, there are many

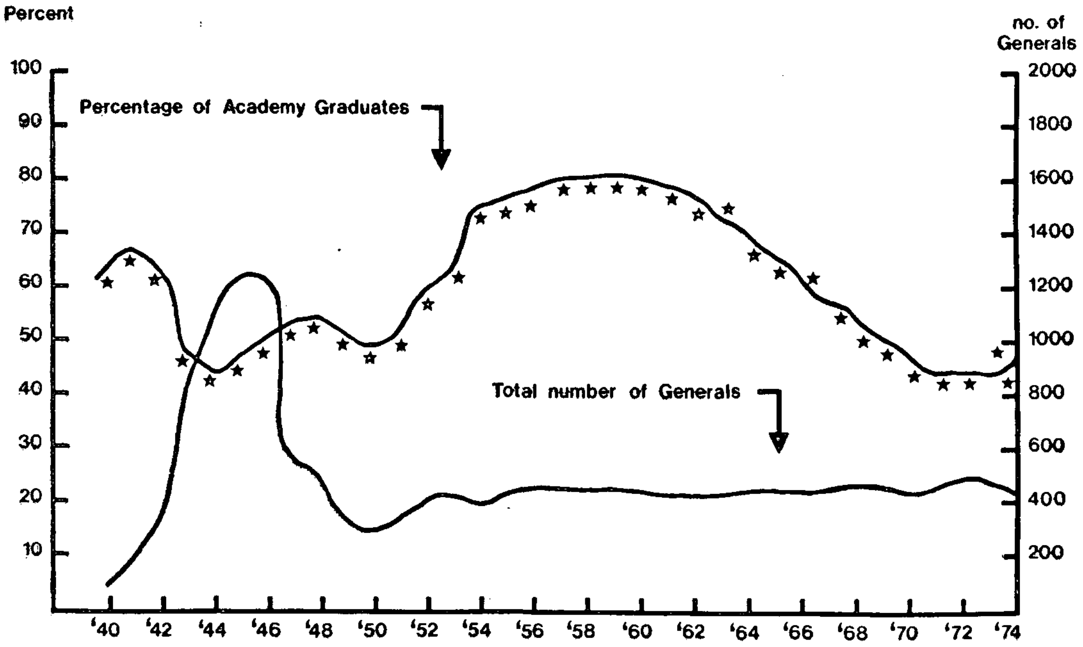

Figure 5. One and Two Star Generals

Draft, is not included because he was a retired officer called back to active duty.

brigadier general and major general positions that West Pointers would be unlikely to fill—the specialized services in the army, such as the Judge Advocate General or the Surgeon General. Second, as indicated earlier, some non-academy officers are able to remain competitive in the promotion process up to the three- and four-star rank, although the number at each level, from 0–4 to 0–10, decreases substantially because of the cumulative visibility advantages of the academy graduates.

The data presented in this section do not of course prove the validity of the visibility theory of promotion. They do, however, provide compelling evidence that the promotion dynamics operating in peacetime are different from those in wartime, a situation that is at least fully consistent with the visibility theory. Moreover, it is difficult to explain the data presented by relying on performance expectations.

5. Conclusion

The implications of the visibility theory are significant for both the individual and the organization. For the individual, it suggests strategies to maximize promotion and it points to constraints that may make elite status unattainable for some officers, no matter how talented. For the organization, it explains the dominance of academy graduates,[18] highlights some weaknesses of the reporting system, and outlines the method by which that system is being partially circumvented. But the most significant implications are those which affect civil-military relations.

As indicated at the beginning of this article, the central issue between the two prevailing views of civil-military relations is the degree of autonomy or self-control accorded to the military organization. And the system of promotion is one of the most crucial levers of control over that organization. The visibility theory suggests that despite civilian involvement in the promotion process—especially at the higher levels, where service secretaries, the president and the Congress must approve general and flag officer promotion lists—the military can maintain virtually complete autonomy over the selection of its officers to the highest levels.

That autonomy is not readily apparent if one presumes that promotion is due primarily to seniority and performance. These, after all, are objective factors, and anyone capable of waiting long enough and performing well enough could rise to elite status. Thus, if these factors were the major criteria for promotion and if the officers initially entering the services from the academies and ROTC encompassed a broad range of political views, and there is evidence for this latter assumption,[19] then the political views of the elite would also be broad, or at least pluralistic. For the winnowing process by which elites emerge from the general pool of officers would be an objective one, completely unrelated to political views. In this case, the tradition of pragmatic professionalism supported by Janowitz, in which the military elite represent a cross section of political persuasions, would apply.

In contrast, the visibility theory of promotion highlights the importance of subjective factors, relegating the objective factors of performance and seniority to minor influences on the process of elite selection. The leeway that raters can exercise in writing evaluations because of diffuse performance standards, the system of sponsorship that circumvents the impersonality and designed objectivity of the promotion system, the visibility requirement and academy connection that emphasize personal contacts—all of these contribute to the subjectivity of the promotion system.

This subjectivity means that the military can indeed maintain institutional autonomy. It can control itself, perpetuate its own ideals through discriminate selection of like-minded individuals, and develop or maintain a dominant political culture. All these traits are characteristic of the tradition of radical professionalism envisioned by Huntington. And, as pointed out by Huntington and others,[20] a salient dimension of the political culture of such a military is political conservatism. Thus, while the initial input of officers may encompass broad political views, those who identify with a conservative political orientation are more likely to receive the better performance reports, establish sponsorship by a general officer, and develop the requisite visibility for elite status. In discussing professionalism, Abrahamsson is most explicit on this point. Drawing partially from evidence in *The American Soldier*,[21] he observes that "there is a continuous selection of people with the 'right' attitudes within the profession . . . ,"[22] and then argues further that professions in general "see to it that persons with congenial rather than uncongenial attitudes are promoted and selected. . . ."[23] It is no doubt true that the process is more complex and more subtle than what is suggested by the above quotations. Political values are rarely made an explicit consideration in evaluation. Nevertheless, those whose values differ substantially from group norms are less likely to be perceived favorably and are themselves more likely to leave the service.

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[18] For a sharply contrasting viewpoint, which argues that the academy officers dominate the higher ranks because of their greater potential and competitiveness, see Charles L. Cochran, "Midshipman and Cadet Profiles and National Norms: A Comparison," *Naval War College Review,* 24 (May 1972), 37–47.

[19] While young men in the academies and ROTC programs are somewhat more conservative than their nonmilitary peers, a broad ideological spectrum still exists. See Cochran; and William A. Lucas, "Military Images in the Army ROTC," *Journal of Political and Military Sociology,* 1 (Spring 1973), 71–90.

[20] Huntington; see also Bengt Abrahamsson, *Military Professionalization and Political Power* (Beverly Hills, Calif.: Sage), 1972).

The debate over the prevailing views of military professionalism and their consequences for civil-military relations has paled somewhat in the past several years. The Huntington approach seems to have been overtaken by events. The military, after all, has become quite involved or integrated into the decision-making process,[24] and physical isolation of the military from society is no longer a feasible or realistic alternative. Yet, the visibility theory of promotion suggests that in at least one important respect the Huntington model remains appropriate. Control of the elite nucleus through the promotion system allows the military a high degree of autonomy. Like values are perpetuated through the premium attached to personal contacts and to the cumulative effects identified as visibility. As a consequence, by the time Congress or other civilian agencies are able to play an active role in the selection of military elite, the promotion system has already preselected those individuals who most highly represent the values held by preceding leaders.

In conclusion, the visibility theory of promotion suggests a much different conception of the military promotion system from that found in the prevailing model, which emphasizes performance and seniority. While our detailed elaboration of the theory has focused on the United States Army and Navy, we expect that the theory applies also to the United States Air Force,[25] and perhaps to other western military organizations—although no doubt with some modifications. Further, we would not be surprised to find a more general application of the theory in large bureaucracies and other "nonmarket organizations" where job performance is not easily measured. When performance criteria are vague, subjective evaluations will inevitably result, since impersonal and standardized performance reports cannot carry the credibility of personal observations—whether made directly by the reviewer or indirectly by those whom the reviewer knows and trusts. It is important, therefore, not only to perform well, no matter how that performance is interpreted by the rater, but also to develop contacts with others who can positively influence one's career. For, ultimately, the characteristic which distinguishes elites from also-rans is not superior performance but greater visibility.

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[21] Samuel A. Stouffer et al., *The American Soldier*, Vol. I (Princeton, N.J.: Princeton University Press, 1949).

[22] Abrahamsson, p. 74.

[23] Abrahamsson, p. 120.

[24] For a review, see John W. Seigle, "Military Advice in Decision-Making: Past and Prologue," in *Issues of National Security in the 1970's*, ed. Amos A. Jordon (New York: Praeger, 1967), pp. 248–71.

[25] The experience with the Air Force OER system supports this expectation. The doubt expressed by Air Force officers concerning this new system centers on the arbitrariness of its quotas, which could work successfully only if all billets were of equal importance. But, as this article points out, certain billets are clearly more important than others and thus attract the better officers. Ordinarily, therefore, an officer who is second or third among those in the Chief of Staff's office or a major headquarters would be perceived as a better performer than an officer rated second or even first in a subordinate command. With the quota system, however, these differences in the importance of billets can no longer be formally recorded. Thus, the system will no doubt be circumvented. And that process of circumvention will stress even more the importance of an officer's visibility.

The Effects of Campaign Spending in Congressional Elections *

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Incomplete understanding of the connection between campaign spending and election outcomes has hindered evaluation of enacted and proposed congressional campaign finance reforms. Reanalysis of the 1972 and 1974 House and Senate campaign spending data using both OLS and 2SLS regression models shows that spending by challengers has a much greater impact on the outcome than does spending by incumbents. A similar analysis of the effects of spending on voters' recall of candidates in the 1972 and 1974 SRC surveys supports the explanation that campaign expenditures buy nonincumbents the necessary voter recognition already enjoyed by incumbents prior to the campaign. The 1974 survey questions on Senate candidates indicate that, although the inability to remember candidates' names does not preclude having opinions about them, voters recalling candidates are much more likely to offer evaluative comments, and these more frequently refer to candidates personally. Aware voters offer more negative as well as positive evaluations (though positive outnumber negative); familiarity is not automatically advantageous. And voters' evaluations of candidates strongly influence how they vote. The implications of these findings for congressional campaign finance policy are readily apparent.

Legislation extending public funding to congressional campaigns was on the agendas of both the House and Senate during the first session of the 95th Congress. The Senate bill, S 926, won majority support but was killed by filibuster; it is not likely that this setback has settled the issue. As with other recent laws intended to alter the way in which congressional campaigns are financed—the Federal Elections Campaign Act of 1971 (PL 92–225) and the Federal Election Campaign Act Amendments of 1974 (PS 93–443)—consideration of this legislation was not informed by any clear understanding of its likely consequences. Nor will be future debates, as long as the crucial question of how campaign expenditures affect the outcomes of congressional elections remains unanswered.

The work reported in this article is intended to clarify the structure of the aggregate relationship between spending and congressional election results and to indicate how campaign spending is linked to the behavior of voters. Specifically, it will show that spending by *challengers* has a substantial impact on election outcomes, whereas spending by *incumbents* has relatively little effect; the evidence is particularly strong for House elections. The much greater impact of the challenger's spending remains when simultaneity bias is eliminated by means of two-stage least squares regression analysis. In simple terms, the more incumbents spend, the worse they do; the reason is that they raise and spend money in direct proportion to the magnitude of the electoral threat posed by the challenger, but this reactive spending fails to offset the progress made by the challenger that inspires it in the first place.

An explanation for these findings is developed from an analysis of the effects of campaign spending on voter recall of incumbent and nonincumbent congressional candidates: campaign expenditures buy nonincumbents the necessary voter recognition already enjoyed by incumbents prior to the campaign. The analysis shows that awareness has an important effect on voter evaluation of candidates and, consequently, on voting behavior in congressional elections. The article concludes with consideration of some salient implications of these findings for enacted or proposed changes in campaign finance policy.

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*I am grateful to Christopher Achen, Forrest Nelson, John Ferejohn, Roger Noll, Stephen Rosenstone, William P. Welch, and Diane Zannoni for their helpful suggestions and critical comments on earlier versions of this article. They are of course free of any responsibility for its remaining shortcomings. The data used here were made available in part by the Inter-University Consortium for Political Research; I am solely responsible for all analyses and interpretations. Some of the material presented here was delivered under the title "Campaign Spending and Voter Awareness of Congressional Candidates," at the Annual Meeting of the Public Choice Society, New Orleans, Louisiana, March 11–13, 1977 and in "The Electoral Consequences of Public Subsidies for Congressional Campaigns," at the annual meeting of the American Political Science Association, Washington, D.C., September 1–4, 1977.

Aggregate Effects of Campaign Spending

A number of recent studies have found that what candidates spend in legislative contests is indeed related to how well they do on election day (Palda, 1973, 1975; Welch, 1974, 1976; Dawson and Zinser, 1976). However, with few exceptions (Glantz, et al., 1977; Jacobson, 1976), these studies have assumed that campaign spending has the same consequences for incumbents and challengers alike. In particular, economic models investigating the "productivity" of campaign spending in terms of winning votes or elections have been grounded on the implicit premise that the marginal productivity of campaign expenditures is the same for all candidates (Lott and Warner, 1974; Welch, 1974; Silberman, 1976). For congressional candidates, a contrary assumption is much more defensible. The advantages of incumbency are well known. The list of perquisites and allowances senators and representatives have granted themselves is too familiar to require reiteration. Incumbents control resources easily worth several hundred thousand dollars annually (Cover, 1977; Perdue, 1977); these resources are unquestionably used to pursue reelection, if only because, for most members of Congress, the campaign never ends (Mayhew, 1974b). In light of the enormous head start therefore enjoyed by incumbents, it would be surprising indeed if campaign spending were not more important to challengers—and to other nonincumbents—than to incumbent candidates.

Evidence from the 1972 and 1974 congressional elections, to be presented in this section, holds no such surprises; it strongly supports the conclusion that what the challenger spends is an important determinant of the outcome, while spending by incumbents makes relatively little difference. Incumbents are apparently able to adjust their level of spending to the gravity of a specific challenge; they spend more when challengers spend more, less when challengers spend less. But the marginal gain in support derived from additional spending does not approach that of the challenger from an equal spending increment; the more *both* candidates spend, the better the challenger does.

The evidence for this interpretation is derived from multiple regression equations in which challenger and incumbent spending are entered, along with appropriate controls, as separate variables, so that their differing impacts are clearly displayed. An important complication is involved, however. Ordinary least squares (OLS) regression models presuppose unidirectional causality—in this case, that spending produces votes. But reciprocal causality is an equally plausible premise—and failure to take this into account is another common deficiency in the literature on campaign spending effects (Palda, 1975, being an exception). The *expectation* that a candidate will do well may bring campaign contributions. Suppose it is possible to make a rough prediction of the outcome prior to the election; if campaign contributors as as "rational investors" who, other things equal, invest more in a campaign they expect to be successful (since one element of risk is smaller), contributions to candidates should increase with their probability of election(Ban-Zion and Eytan, 1974; Welch, 1974, 1977; Dawson and Zinser, 1976).

Or, from a slightly different perspective, campaign spending may help win popular support, and thus votes, but characteristics that also help to attract votes—personal charm or "charisma," political skill and experience—should also ease the job of fundraising. Candidates who are well known and who have political experience (and thus a greater likelihood of success) raise money more easily, spend it, thereby further increasing their popularity (and chances for victory), acquiring in consequence even more money, and so on—the ultimate payoff coming in the form of additional votes on election day.

The ordinary least squares regressions reported in most studies are inappropriate for estimating reciprocal relationships; a simultaneous equation system is required. OLS estimates of parameters when the true relationship is reciprocal are biased and inconsistent because endogenous variables (those which have a reciprocal effect on one another), when treated as explanatory variables, are correlated with the error term (Johnston, 1972, p. 343). The two-stage least squares (2SLS) regression procedure, a standard solution to this problem, is therefore used later in this section to estimate the effects of campaign expenditures by challengers and incumbents within a system of simultaneous relationships.

Despite its potential inadequacies, however, a straightforward OLS regression equation provides a useful starting point for determining the aggregate effects of campaign spending in congressional elections. The equation estimated for the 1972 and 1974 House elections was

$$CV = a + b_1 CE + b_2 IE + b_3 P + b_4 CPS + e \qquad (1.1)$$

where

CV is the challenger's percentage of the two-party vote[1]

CE is the challenger's campaign expenditures in thousands of dollars[2]

IE is the incumbent's campaign expenditures in thousands of dollars

P is the challenger's party (1 if Democrat, 0 if Republican)

CPS is the strength of the challenger's party in the district (approximated here by the percentage of the vote won by the challenger's party's candidate in the last election for this seat)[3]

a is the intercept, the b's regression coefficients, and e the error or disturbance term. The challenger's share of the vote is hypothesized to be a function of what the challenger and incumbent spend, the challenger's party, and the strength of that party in the district. Notice that an equivalent equation with observations on incumbents would produce estimates of the coefficients which mirror those derived from this model; either one would support the same substantive conclusions.

Since our interest is in the effects of campaign spending, variables P and CPS serve primarily as controls in this equation. The party variable accounts for national short-term forces favoring one party or another in a particular election year. District party strength is measured by the vote for the challenger's party in the most recent prior election for that seat; though far from ideal as an approximation of the expected or "normal" vote, it has the advantage over the other possible index (percentage of registrants with the challenger's party in the district) of being available for a much larger proportion of the districts. At one stage of this research I used registration percentages in place of CPS, and the results were essentially the same as those reported in this article, though the number of cases was halved. Both the party and party strength variables are expected to affect a candidate's ability to raise money as well as to win votes and so must be taken into account in this preliminary model.

Challenger and incumbent spending are entered as separate variables rather than as some composite (for example, the challenger's percentage of expenditures by both candidates) because their coefficients are not expected to be the same. The functional relationship between spending and votes is assumed to be linear. This has the advantage of simplicity but the drawback that it fails to allow for the diminishing returns that must apply to campaign spending; no candidate can get more than 100 percent of the vote, no matter how much is spent. An attractive alternative is the semilog form in which spending is entered as the natural logarithm of actual expenditures (Welch, 1976); it permits diminishing returns but does not allow them to become negative as would, for instance, a quadratic model (Silberman and Yochum, 1977).

Both the linear and semilog forms fit the data equally well; the R^2's are identical. But the semilog model has the defect of seriously underestimating the challenger's vote at higher levels of spending; that is, it provides estimates which exaggerate the extent to which returns diminish as spending increases. Examination of the residuals (a residual is, in this case, the difference between the percent of votes actually won by the challenger and that predicted by the regression equation) showed this to be the case. The problem is illustrated by comparing the actual number of winning challengers in both election years with the number predicted by the linear and semilog equations:

Winning Challengers	1972	1974
Actual number[4]	9	39
Number predicted by:		
Linear equation	5	29
Semilog equation	1	2

The linear equation exaggerates the expected vote of challengers at higher levels of spending, but inspection of the residuals indicates that this is not a significant problem until the challenger's spending exceeds $160,000, which occurs in less than 2 percent of the cases in either election year; at this level of spending the equations are less likely to overpredict the number of winning challengers than they are to overstate the size of the challenger's victory. I therefore chose to present the linear equations, but the reader should be aware that a comparable analysis with semilog equations would uphold the substantive conclusions defended below. The regression estimates of equation 1.1 are reported in Table 1.[5]

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[1] The election results are from Scammon (1975).

[2] The 1972 data are from Common Cause (1972); the 1974 data are from *Congressional Quarterly* (1975, pp. 789–96).

[3] The data source is *Congressional Quarterly* (1974b). Previous vote percentages have been adjusted in these data for changes in district boundaries where redistricting has occurred.

[4] Actually 13 incumbents lost in 1972, but three of them were defeated by other incumbents they were forced to run against because of reapportionment and a fourth lost a three-way race running as an independent. Forty incumbents lost in 1974, but one of these had just been elected in a special election and no separate spending figures were available for the second contest.

According to the equations in Table 1, it is clearly the challenger's level of spending that has the greatest impact on the outcome of these elections; challengers are expected to gain a little over 1 percent of the vote for every $10,000 they spend. Incumbent spending apparently makes much less difference. The simple correlation between incumbent expenditures and the challenger's vote is in fact positive (.39 for 1972, .46 for 1974); ignoring other factors, *the more incumbents spend, the worse they do.* With the challenger's spending controlled, the incumbent's spending has a weak negative effect on the challenger's vote; its coefficient is not statistically significant in the 1972 equation. This implies that incumbents are able to expand their financial resources in response to a serious challenge (represented by the challenger's level of spending), but that this additional spending either does them little good or at best does not begin to match the much greater benefit challengers derive from an equivalent increase.

The evidence that incumbents are able to adjust their spending to the gravity of the challenge is convincing. Regression of incumbent expenditures on a variety of explanatory variables shows that the challenger's level of spending has by far the greatest explanatory power. The equation estimated was:

$$IE = a + b_1CE + b_2P + b_3CPS + b_4IP + b_5YRS + b_6PO + b_7L + e \qquad (2.1)$$

where

IP is 1 if the incumbent ran in a primary election, 0 otherwise
YRS is the number of consecutive years the incumbent has been in the House
PO is 1 if the challenger has previously held elective office, 0 otherwise[6]
L is 1 if the incumbent is chair or ranking member of a subcommittee or holds a higher leadership position, 0 otherwise

and IE, CE, P, CPS, and the coefficients are as defined for equation 1.1. The results appear in Table 2. Obviously, the challenger's spending is the most important explanatory variable in these equations. And the other variables work as expected on the assumption that incumbents spend in response to the gravity of the electoral challenge. For example, incumbents spend more if the challenger has held elective office, or–in 1974–if the challenger was a Democrat; they spend less, other things being equal, the longer they have been in office. But all of these variables would be expected to show *opposite* signs if the equations estimated the incumbent's capacity to *raise* money according to political assets and likelihood of reelection.

Table 1. The Effects of Campaign Spending in the 1972 and 1974 House Elections (OLS): Equation 1.1

			Regression Coefficient	*t*-ratio[a]	Standardized Regression Coefficient	
1972 (N=296)	$CV=$	a	20.7			
		b_1CE	.112	9.42	.51	
		b_2IE	–.002	–.14	–.01	$R^2=.49$
		b_3P	–.47	–.61	–.03	
		b_4CPS	.299	6.94	.33	
1974 (N=319)	$CV=$	a	15.6			
		$b1CE$	.121	10.45	.48	
		b_2IE	–.028	–2.34	–.11	$R^2=.65$
		b_3P	9.78	11.19	.42	
		b_4CPS	.351	7.75	.28	

[a] Given the degrees of freedom in these equations, a *t*-ratio of at least 1.98 is necessary for a .05 level of significance, 2.58 for .01, and 3.35 for .001.

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[5] Observations included only those contests in which a major party challenger faced a major party incumbent and for which data on the vote in the previous election were available. This latter requirement forced twenty-three 1972 and two 1974 observations to be dropped.

[6] From information in *Congressional Quarterly* (1972 and 1974a).

Even more to the point, the *difference* in spending between 1972 and 1974 by incumbents who ran and were opposed in both campaigns is an almost identical function of the difference in spending by their opponents in the two elections. This is apparent from an estimate of

$$(IE_{74}-IE_{72}) = a + b_1(CE_{74}-CE_{72}) + b_2IE_{72} + b_3CE_{72} + b_4IP_{72} + b_5IP_{74} + b_6P + e \qquad (3.1)$$

where the variables and coefficients are as defined for equations 1.1 and 2.1 and the subscripts on the variables indicate the election year. The results are shown in Table 3. If the controls for primary elections, party, and 1972 spending by both candidates are omitted, the relationship between the change in challenger spending and the change in incumbent spending scarcely varies. The regression coefficient becomes .505, its *t*-ratio 13.23, and its standardized regression coefficient .61.

Incumbents apparently increase or decrease their spending in reaction to changes in the amount spent by opponents. Any increase, however, does not counterbalance benefits to the challenger from the spending that inspired it in the first place. The difference in spending levels by incumbents between 1972 and 1974 is negatively correlated (–.58) with the differences in the proportion of votes won by the incumbent in the two elections. A significant negative relationship remains even when changes in the challenger's spending are taken into account.

Table 2. Determinants of Spending by Incumbents in the 1972 and 1974 House Elections: Equation 2.1

			Regression Coefficient	*t*-ratio[a]	Standardized Regression Coefficient	
1972 (N=296)	$IE =$	a	28.08			
		b_1CE	.522	10.33	.54	
		b_2P	3.31	.87	.04	
		b_3CPS	.224	1.05	.06	$R^2 = .39$
		b_4IP	10.50	2.69	.13	
		b_5YRS	–.789	–2.69	–.15	
		b_6PO	3.10	.69	.03	
		b_7L	.89	.14	.01	
1974 (N=319)	$IE =$	a	14.11			
		b_1CE	.495	10.30	.51	
		b_2P	12.69	2.88	.14	
		b_3CPS	.673	3.05	.14	$R^2 = .47$
		b_4IP	.10	.02	.00	
		b_5YRS	–.035	–.11	–.01	
		b_6PO	6.79	1.45	.07	
		b_7L	–12.50	–2.00	–.10	

[a] See Table 1.

Table 3. Determinants of Changes in Spending by House Incumbents Between 1972 and 1974: Equation 3.1

			Regression Coefficient	*t*-ratio[a]	Standardized Regression Coefficient	
(N=295)	$(IE_{74}-IE_{72}) =$	a	28.92			
		$b_1(CE_{74}-CE_{72})$	.491	11.73	.60	
		b_2IE_{72}	–.454	–11.96	–.58	
		b_3CE_{72}	.398	7.73	.43	$R^2 = .63$
		b_4IP_{72}	–8.24	–2.35	–.09	
		b_5IP_{74}	–1.35	– .39	–.01	
		b_6P	–13.14	–3.54	–.15	

[a] See Table 1.

Of the 39 incumbents who lost in 1974, 34 (87 percent) spent more in losing than they did in winning in 1972. The mean expenditure for all incumbents who ran and were opposed in both elections was \$61,799 in 1972 and \$63,609 in 1974, an increase of 3 percent. The mean expenditure of the 1974 losers was \$69,218 in 1972 and \$101,645 in 1974, a 47 percent increase. Most were of course Republicans. But in a year electorally disastrous for Republicans and financially hopeless for Republican *challengers* (1974 Democratic challengers spent an average of \$59,352, Republican challengers \$21,463), Republican *incumbents* actually outspent Democratic incumbents by, on the average, about \$35,000 (\$81,437 to \$46,261). Despite the extraordinarily hostile political environment, Republican incumbents were able to increase their spending by almost 55 percent over the previous election; spending by Democrats actually decreased.

Since the circumstances of the 1974 election were rather unusual, evidence from other elections would be pertinent. Unfortunately, 1972 is the first election for which reasonably complete data on spending in House election are available. However, for both 1970 and 1972 data have been published on spending for radio and television time by House candidates in the general election campaign, so some comparisons are possible. In 1970 the losing incumbents spent an average of \$9,628 on radio and television; winning incumbents spent an average of \$4,572. The comparable figures for 1972 are \$16,220 and \$5,727, respectively. Mean expenditures for broadcast time grew from \$4,738 to \$6,097 for all incumbents between 1970 and 1972, an increase of 27 percent; the same expenditures for incumbents who ran and were opposed in both years and who lost in 1972 went from \$8,696 to \$16,220, an increase of 87 percent.[7]

The point is clear—and fundamental to comprehending the role of money in congressional elections. Incumbents are evidently able to raise and spend money in direct proportion to the perceived necessity to do so, this being a function of the gravity of the electoral threat posed by the opposition. None of the contribution or demand functions previously estimated for models of campaign finance processes have taken this into account (Bental, Ben-Zion, and Moshel, 1976, 1977; Dawson and Zinser, 1976; Silberman and Yochum, 1977; Welch, 1977); they therefore require respecification.

* * *

The argument that reactive spending by incumbents does not offset the gains accruing to challengers from the spending that inspired the reaction is based, in part, on possibly unreliable OLS estimates. A more complete model of the money-vote relationship postulates a reciprocal connection between these variables:

$$CE = f(P, CPS, PO, YRS, CP, EV) \tag{4.1}$$

$$IE = f(P, CPS, PO, YRS, IP, EV, CE) \tag{4.2}$$

$$CV = f(CE, IE, PO, CPS) \tag{4.3}$$

$$EV \cong CV \tag{4.4}$$

The variables are as defined for the previous equations, with the addition of CP (1 if the challenger ran in a primary election, 0 if not) and EV, the challenger's expected vote.

Equation 4.1 hypothesizes that the challenger's ability to attract contributions is, to begin with, a function of the challenger's party (consider 1974) and district party strength (measured, remember, as the proportion of votes won by the party's candidate for the seat the last time around; it may also be interpreted here as an indicator of the vulnerability of the incumbent). The variable PO measures the effects of prior electoral success, and the exposure, experience, and contacts that come with holding elective office, on the ability to raise campaign funds. The number of years the incumbents has held office is another indicator of vulnerability, this affecting the attractiveness of the challenger as an "investment."

Since the data do not include separate figures for primary and general elections, CP (and IP in the second equation) is included to pick up the differences in spending brought about by the demands of a primary contest. There is no way to determine accurately how much of a candidate's money was spent in the primary. This is not so troublesome as it may seem. Challengers spend as much as they can raise anyway. Their principal problem is to make themselves known to voters, and this can be done as effectively in a primary as in a general election campaign (Jacobson, 1976); primary election spending should therefore have general election payoffs. And incumbents spend according to what the challenger does. We do not, therefore, expect particularly dramatic changes in spending levels if a candidate does or does not contest a primary. But coefficients on these variables will give some indication of the degree to which primaries do affect spending.

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[7] Data upon which these figures are based are from United States Congress (1971 and 1973).

Finally, challengers are expected to attract contributions in proportion to their probability of being elected, which is here approximated by their expected vote, itself of course related to the actual vote.

Interpretation of the equation for incumbent spending is quite different. Incumbent spending is *not* a positive function of the likelihood of victory at all; rather, the more certain they are of election, the less incumbents spend. This does not mean that rational investors deliberately ignore them—who would not want to invest in a sure thing?—or that they could not raise a great deal more money if they wished. The explanation is simply that incumbents sure of victory feel no need for the money that is available. Soliciting and accepting contributions is hardly something politicians enjoy. Hubert Humphrey called it "a disgusting, degrading, demeaning experience," and others have echoed his sentiments (Adamany and Agree, 1975, p. 8). Indeed, this may be the major reason many members of Congress currently favor public funding of congressional campaigns.

Incumbents, then, acquire funds only in proportion to the felt necessity to do so. And they can usually get all they need. The variables that determine incumbent spending, therefore, indicate how much the candidate is likely to need. And this, in turn, is primarily a function of the strength of the challenger. Since the challenger's strength is indicated in good measure by financial resources, *CE* belongs in equation 4.2 as an explanatory variable. The measure of incumbent expenditures, *IE*, does not similarly belong in the challenger's expenditure equation; challengers are not able to raise money at will to contest an incumbent who may be spending a great deal. Rather, they spend all they can (at least up to very large amounts) independently of what the incumbent is spending.

The other variables in the incumbent's spending equation can be interpreted in the same way; they determine how threatened incumbents are likely to feel and therefore how much they find it prudent to raise and spend.

The third equation is of course the one originally estimated by OLS. The other exogenous variables (those determined *outside* the equation system) are left out on the theoretical premise that they affect *CV* only indirectly (through their effect on spending); empirically, these variables had no statistically significant connection with the election outcomes with the other variables controlled. Since *CV* is approximated with some significant degree of accuracy by *EV* (equation 4.4), the OLS estimates are liable to bias and inconsistency and thus are unreliable estimates of the structural parameters (those of the true causal relationships in equation 4.3). A standard solution to this difficulty is the two-stage least squares procedure. The first step is to regress challenger and incumbent spending on all of the exogenous variables in the system. The equations to be estimated are:

$$CE^* = a + b_1P + b_2CPS + b_3PO + b_4YRS + b_5CP + b_6IP + e \qquad (5.1)$$

$$IE^* = a + b_1P + b_2CPS + b_3PO + b_4YRS + b_5CP + b_6IP + e. \qquad (5.2)$$

The estimated parameters are then used to compute *CE** and *IE** for each observation, and these variables replace *CE* and *IE* in the second stage equation,

$$CV = a + b_1CE^* + b_2IE^* + b_3P + b_4CPS + e. \qquad (5.3)$$

The 2SLS procedure "purges" the explanatory variables *CE** and *IE** of the component associated with the error term. The resulting estimates are still biased estimates of the true structural parameters but are now consistent; the bias decreases as the sample size increases, approaching zero in the limit (Rao and Miller, 1971, p. 214; Johnston, 1972, pp. 380–84). The estimates of equations 5.1, 5.2, and 5.3 appear in Table 4.

The 2SLS results recapitulate the OLS findings in one very important respect: a given amount of campaign spending does not have the same consequence for challengers and incumbents. Spending by challengers has a much more substantial effect on the outcome of the election even with simultaneity bias purged from the equation. Indeed, the regression coefficients on *CE** are larger than those for *CE*. However, the *standardized* regression coefficients for *CE** are smaller than those for *CE* (.36 compared to .51 for 1972, .38 compared to .48 for 1974); the steeper slopes are evidently an artifact of the much smaller range of the instrumental variable *CE**, less than half that of *CE* in both election sets. Even so, OLS does not appear to greatly exaggerate the effects of the challenger's spending, nor does it substantially underestimate the effects of incumbent spending.

Table 4. The Effects of Campaign Spending in the 1972 and 1974 House Elections (2SLS): Equations 5.1, 5.2, and 5.3

			Regression Coefficient	*t*-ratio[a]	Standardized Regression Coefficient	
1972 (N=296)						
First-Stage Equations	$CE^* =$	a	−17.56			
		b_1P	−11.34	−2.60	−.14	
		b_2CPS	1.39	5.92	.34	
		b_3PO	21.20	4.18	.23	R^2 = .23
		b_4YRS	−.40	−1.36	−.07	
		b_5CP	8.28	1.96	.10	
		b_6IP	1.92	.42	.02	
	$IE^* =$	a	18.23			
		b_1P	−2.86	−.66	−.04	
		b_2CPS	.93	3.95	.23	
		b_3PO	13.40	2.64	.15	R^2 = .18
		b_4YRS	−.99	−3.36	−.19	
		b_5CP	8.85	2.10	.12	
		b_6IP	10.73	2.34	.13	
Second-Stage Equations	$CV =$	a	22.3			
		b_1CE^*	.163	2.59[b]	.36	
		b_2IE^*	−.051	−.89	−.10	R^2 = .46[b]
		b_3P	−.01	−.01	−.00	
		b_4CPS	.269	3.55	.30	
1974 (N=319)						
First-Stage Equations	$CE^* =$	a	−16.03			
		b_1P	28.15	5.40	.31	
		b_2CPS	1.07	4.25	.22	
		b_3PO	21.00	3.89	.20	R^2 = .29
		b_4YRS	−.19	−.58	−.03	
		b_5CP	3.77	.77	.04	
		b_6IP	−3.16	−.66	−.03	
	$IE^* =$	a	6.15			
		b_1P	22.55	4.40	.25	
		b_2CPS	1.23	4.94	.26	
		b_3PO	17.00	3.19	.17	R^2 = .28
		b_4YRS	−.42	−1.31	−.07	
		b_5CP	7.37	1.53	.08	
		b_6IP	−2.30	−.49	−.02	
Second-Stage Equation	$CV =$	a	17.1			
		b_1CE^*	.179	1.21[b]	.38	
		b_2IE^*	−.022	−.14	−.05	R^2 = .63[b]
		b_3P	7.51	4.20	.32	
		b_4CPS	.264	2.94	.21	

[a] See Table 1.

[b] The R^2s and *t*-ratios for the second-stage equations are adjusted figures; they cannot be computed directly from the second stage regression runs.[8]

The parameters estimated in the second-stage equations indicate that challengers receive 1.63 percent to 1.79 percent of the vote for each $10,000 they spend; they are expected to lose between 0.51 percent and 0.22 percent for each $10,000 the incumbent spends; therefore, if spending by *both* candidates increases by this amount, the net gain for challengers should approximate at least 1 percent of the vote. These equations also show (as did the OLS equations) that it was very advantageous to be a Democrat in 1974 (it was worth an additional 7.5 percent of the vote), whereas in 1972 the candidate's party made no difference. And they indicate that the challenger's district party strength also contributes significantly to the share of the vote won.

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[8] The R^2's and the *t*-ratios for the second stage equations are not taken directly from the statistics produced by estimating these equations. Rather, they are found by replacing the standard error of the estimate from the equation as computed by a standard error computed from a combination of the 2SLS parameters with the actual spending variables *CE* and *IE* replacing *CE** and *IE**. I am obliged to John Ferejohn for explaining this procedure to me.

One troubling aspect of these equations is that in both election sets CE^* and IE^* are very highly correlated. The reason for this is clear from the first-stage equations; the variables seem to have similar effects on the spending levels of both candidates. This is of course as it should be, if incumbent spending is basically a reflection of challenger spending, as I have been arguing. But it raises the problem of multicollinearity. Multicollinearity destroys the precision of the estimates; notice that the coefficient for CE in the 1974 equation is not statistically significant by the usual criterion (a t-ratio of 1.98 or larger). According to Johnston (1972, p. 163), a very large positive correlation between two explanatory variables is likely to produce large and opposite errors in the estimates of regression coefficients.

Under these circumstances it is useful to run the second-stage equations excluding one of the correlated variables. Estimates of the equations omitting IE^* are found in Table 5. The coefficients for CE^* decrease somewhat, as anticipated. But the precision of the estimates increases, especially in the 1974 equation. If IE^* were left in the equations instead, this variable would also show a strong positive relationship to the challenger's share of the vote; IE^*, like IE, has a significantly positive simple correlation with CV (the adjusted figures are .51 for 1972 and .78 for 1974).

Some further comments on the first-stage equations are in order. Spending by both challengers and incumbents changes by approximately $1,000 for a change of 1 percent in the vote won by the challenger's party in the last election. Challengers who have won political office before spend an average of about $21,000 more than those who have not; incumbents also spend more against these candidates. These two variables make the most difference. The advantages of being the incumbent are clear; notice the intercepts. So are the advantages of being a Democrat in 1974. The R^2's for these equations are not as large as would be desirable; a number of other explanatory variables were tested in earlier stages of the research, but none of them improved any of the equations significantly. The criterion for inclusion, other than theoretical plausibility, was that a variable had to have a regression coefficient at least twice its standard error in at least one of the equations.

Before proceeding to develop an explanation of these results, it will be instructive to consider the effects of campaign spending on the electoral fortunes of candidates running in contests in which neither is an incumbent. The OLS regression estimates of the relationship for Democratic candidates (had Republican candidates been chosen instead, the results would have formed a mirror image of those reported) appear in Table 6; the party variable has of course been omitted, the CE, IE, and CPS are replaced by DE (the Democrat's spending in thousands of dollars), RE (the Republican's

Table 5. The Effects of Challengers' Campaign Spending in the 1972 and 1974 House Elections (2SLS): Equation 5.3 Omitting IE^*

			Regression Coefficient	t-ratio[a]	Standardized Regression Coefficient	
1972 (N=296)	$CV =$	a	20.8			
		b_1CE^*	.117	3.42^b	.26	
		b_3P	–.41	–.48	–.02	$R^2 = .49^b$
		b_4CPS	.289	4.07	.32	
1974 (N=319)	$CV =$	a	16.7			
		b_1CE^*	.160	3.60^b	.34	
		b_3P	7.53	4.21	.32	$R^2 = .62^b$
		b_4CPS	.257	3.49	.21	

[a] See table 1.

[b] See table 4.

spending), and *DPS* (Democratic party strength—measured like *CPS*) in these equations.

The results for 1972 are as we should anticipate. The marginal effects of spending are similar for both candidates; the Democrat's own spending helps, that of the Republican does the opposite. For 1974, however, notice that the Republican candidate's spending makes a much greater difference in the outcome than does that of the Democrat, which is perversely signed and is not significantly related to the outcome at all. In other words, Republican candidates in 1974 were affected by spending in a way we would expect if they were, instead, challenging incumbents. But this is not so surprising. Given the post-Watergate political atmosphere, poisonous for Republicans, Republican candidates found themselves in the heavily disadvantaged position usually reserved for challengers regardless of whether or not the Democrat was really an incumbent. Campaign spending is evidently most useful to candidates suffering severe electoral handicaps, no matter what the source.

All of the foregoing refers to House contests. Analysis of Senate elections will be much briefer. The regression model which clearly fits the data best is the semilog form:

$$CV = a + b_1 lnCEPC + b_2 lnIEPC + b_3 P + e \qquad (6.1)$$

where

lnCEPC is the natural log of the challenger's expenditures in cents per voting-age individual[9]

lnIEPC is the natural log of the incumbent's expenditures in cents per voting-age individual

and the other variables and coefficients are as defined for equation 1.1. Actual spending was divided by the voting-age population of the state in recognition of the widely varying state populations. No measure of party strength was included in the equation because in no model tested did this variable (measured by an index similar to Kostroski's base party vote[10]) show any significant connection with Senate election outcomes. The results of this analysis are found in Table 7. They are somewhat more ambiguous than the results for House elections. The challenger's spending does appear to have a greater impact, but for 1972 the difference is not large, and in both years the incumbent's spending was negatively related to the challenger's share of the vote. The simple correlation between incumbent spending and the challenger's vote was −.17 for 1972 and .48 for 1974, again a mixed outcome.

The 2SLS technique was also attempted with Senate data; for 1972 the first-stage equations explained so little of the variance that even remotely trustworthy 2SLS estimates were out of the question. Estimates were obtained for 1974; they were almost identical to the OLS estimates. The tentative conclusion suggested by these findings is that the challenger's expenditures are more effective in Senate elections, too, but incumbent senators benefit from their own spending to a greater degree than do their counterparts in the House.

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[9] The voting age populations of the states are from United States Congress (1974, p. 18542 and 1977, p. 835).

[10] The index of state party strength was computed as the smallest proportion of the total statewide House vote won in aggregate by House candidates of the Senate candidate's party in any election year from 1968 to 1974. See Kostroski (1973).

Table 6. The Effects of Campaign Spending in the 1972 and 1974 House Elections for Open Seats (OLS)

		Regression Coefficient	*t*-ratio[a]	Standardized Regression Coefficinet	
1972 (N=52)	$DV = a$	34.7			
	$b_1 DE$	.045	2.56	.31	
	$b_2 RE$	−.077	−2.84	−.34	$R^2 = .46$
	$b_4 DPS$	.308	4.15	.47	
1974 (N=53)	$DV = a$	51.4			
	$b_1 DE$	−.002	−.10	−.01	
	$b_2 RE$	−.130	−5.07	−.54	$R^2 = .59$
	$b_4 DPS$	.328	3.95	.41	

[a] Given the degrees of freedom in these equations, a *t*-ratio of at least 2.01 is necessary for a .05 level of significance, 2.68 for .01, and 3.50 for .001.

Campaign Spending and Candidate Familiarity

An attractive theoretical explanation for the findings reported in the first section begins with the observation that incumbents are already familiar to voters at the outset of the campaign, whereas nonincumbents probably are not. The resources of office provide ample means for acquiring voter recognition and, beyond that, building a favorable reputation (Cover, 1977; Abramowitz, 1975). Additional information on incumbents disseminated during the campaign should have less impact on an electorate which has already been subjected to a barrage of messages from the candidate. Nonincumbents normally have much more to gain in the way of voter awareness in the course of the campaign, implying that the more extensive—and therefore expensive—the campaign, the better known they will become.

Voter recognition appears to be an important component of electoral success. The pioneering work of Stokes and Miller (1962) showing the connection between voter awareness of House candidates and the frequency of partisan defection in 1958 has been reinforced by studies of more recent House elections (Arsenau and Wolfinger, 1973). The effects of differential awareness of candidates on partisan-voting patterns in the 1972 and 1974 House elections and the 1974 Senate elections (which will provide the data base for this section) are shown in Table 8. The candidate familiarity question was asked about Senate candidates for the first time in 1974; notice that partisan voters respond to differential awareness of Senate candidates in essentially the same way as to differential awareness of House candidates.[11]

Table 7. The Effects of Campaign Spending in the 1972 and 1974 Senate Elections (OLS): Equation 6.1

		Regression Coefficient	t-ratio[a]	Standardized Regression Coefficient	
1972 (N=25)	$CV = a$	41.8			
	$b_1 lnCEPC$	5.55	4.15	.78	
	$b_2 lnIEPC$	−4.81	−2.77	−.49	$R^2 = .47$
	$b_3 P$	2.49	.81	.14	
1974 (N=22)	$CV = a$	35.4			
	$b_1 lnCEPC$	3.77	3.69	.78	
	$b_2 lnIEPC$	−1.39	−.98	−.20	$R^2 = .81$
	$b_3 P$	8.26	5.18	.55	

[a]Given the degrees of freedom in these equations, a t-ratio of at least 2.08 is necessary for a .05 level of significance, 2.85 for .01, and 3.85 for .001.

Table 8. The Effects of Awareness of the Candidates on Partisan Defection in House and Senate Elections

	Voter Was Aware of:			
Percent Who Defected in:	Both Candidates	Own Candidate	Other Candidate	Neither Candidate
House Elections				
1972	23 (151)[a]	7 (100)	62 (26)	21 (242)
1974	29 (281)	1 (154)	58 (79)	15 (374)
Senate Elections				
1974	25 (317)	2 (138)	62 (87)	10 (205)

[a]Number of cases from which percentages were computed. The 1974 sample is weighted.

Source: The 1972 and 1974 SRC surveys.

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[11] The candidate-recognition questions, coded as variables 2174, 2175, and 2176 of the 1974 SRC Survey, were "Now let's talk about the campaign for Senator. Do you remember what the candidates' names were? What were they?" (Miller, Miller, and Kline, 1975, pp. 103–05).

The same data provide information on the advantage in voter recognition typically enjoyed by incumbents over their challengers. Table 9 lists the proportion of voters in the sample able to remember a candidate's name according to the candidate's incumbency status. The incumbent's advantage here is clear. And Senate candidates are better known, in general, than House candidates, hardly a counterintuitive discovery. A much more telling point can be made if information from both tables is combined: in those instances in which the contest is between an incumbent and a challenger and only one of the candidates is known, that candidate is the incumbent *96 percent* of the time in all three election sets. When the effects of awareness are most pronounced, the advantage is almost entirely to the incumbent.

Challengers and other nonincumbents clearly have more to gain by vigorous campaigning. In general, then, the more nonincumbents spend, the greater should be their saliency. Spending by incumbents should, by comparison, have a weaker effect on how well they are known. This, if true, would provide an explanatory link between the observed aggregate effects of campaign spending by incumbents and challengers and the survey findings on voting behavior in congressional elections.

The relationship between campaign spending and candidate saliency may, of course, also involve reciprocal causation. Candidates who are well known are able to raise more money, which, spent judiciously, increases their renown even further. If this is the case, a model of these relationships should consist of two simultaneous equations with two endogenous variables. Theoretically, candidate saliency is expected to be a function of campaign spending plus some exogenous variables, while campaign spending is, in turn, a function of candidate saliency and some exogenous variables. Identification of the equations (and thus the possibility of estimating their parameters) depends on the available exogenous variables and the assumptions that can be made about them. Consider a preliminary specification:

$$CR = a + b_1E + b_2PO + c_{1\ldots5}X_{1\ldots5} + e \qquad (7.1)$$

$$E = a + b_1CR + b_2PO + b_3NPS + b_4NI + e \qquad (7.2)$$

where

CR is candidate recall, measured as 1 if the respondent remembers the candidate's name, 0 otherwise[12]

E is the nonincumbent candidate's campaign expenditures (in thousands of dollars)

PO is 1 if the candidate has previously held elective office, 0 otherwise

NPS is the strength of the nonincumbent candidate's party in the constituency (measured as was CPS in the first section)

NI is 1 if the nonincumbent is running against another nonincumbent, 0 if opposing an incumbent

$X_{1\ldots5}$ are respondent variables: social class, education, attentiveness to the mass media, political interest, and whether or not the respondent shares the candidate's partisan affiliation.[13]

Table 9. Percent of Voters Aware of Candidates by Incumbency Status

	Percent of Voters Recalling Name of Candidates Who Were:		
	Incumbents	Challengers	Neither (Open Seat)
House Elections			
1972	50 (498)[a]	27 (498)	41 (220)
1974	57 (856)	31 (856)	35 (374)
Senate Elections			
1974	73 (595)	44 (595)	54 (428)

[a] Number of cases from which percentages were computed. The 1974 sample is weighted.

Source: The 1972 and 1974 SRC surveys.

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[12] The data are from the SRC surveys for 1972 and 1974; the 1974 sample is weighted.

[13] The construction of these variables is reported in Jacobson (1976).

The a's are intercepts, the b's and c's regression coefficients, and the e's the error terms.

The reasoning underlying this specification should be apparent. Observations on all nonincumbents, not merely challengers, can be included because, theoretically, spending should affect popular awareness of all nonincumbent candidates in about the same way. The small quantity of empirical evidence available suggests that it does (Jacobson, 1976). This also augments the number of observations, particularly at the upper end of the expenditure scale. Whether or not respondents remember names of candidates depends on how much candidates spend, their prior political exposure, and characteristics of the respondents themselves.

Candidates' spending levels depend on how well they are known, their prior political experience, which party they belong to (especially important in a year like 1974), the strength of that party in their districts, and whether or not they are running against incumbents. The variables *P, NPS,* and *NI* are expected to affect the ability to raise money—primarily because they are closely related to presumed chances of victory—but should not, in theory, affect the likelihood that a voter will recognize a candidate independently of the voter's individual partisan orientation. Equation 7.2 is similar to equation 5.1 in the first section; *NI* replaces *YRS* as one measure of the effects of incumbency (necessarily because not all nonincumbents are challengers), and the primary election variables are dropped because they had no statistically significant effect in any of the regressions examined.

Both equations are identified (overidentified, in fact), but this specification is unsatisfactory. The first equation involves a categorical dependent variable and therefore raises some difficulties to be addressed shortly. Before we proceed to that, we will find it helpful to simplify the equation. The first simplification is to ignore the respondent variables $X_{1\ldots5}$. Although some of these variables are indeed related to the likelihood that the respondent will remember a candidate's name,[14] none is correlated with the other independent variables as high at .1, so their omission should not affect the regression coefficients of those variables (Kmenta, 1971, pp. 392–95). Furthermore, we can assume that $b_2 = 0$ in equation 7.1. Although on theoretical grounds we might expect that candidates who have held prior political office would have a greater probability of being known by voters, empirically this does not seem to be true. If spending is taken into account, the relationship between this variable and candidate recognition, weak to begin with, disappears.[15]

Even though the dependent variable in equation 7.1 actually takes only two values, 1 and 0, the equation can be interpreted as estimating the conditional probability that a respondent remembers a candidate's name. A problem is that least squares estimates of the parameters may predict values of more than 1.0 or less than 0.0 for this probability for some observations. In addition, the error term cannot have a zero expectation, invalidating one of the assumptions required for unbiased least squares estimation (Theil, 1971, pp. 632–33). Logit analysis avoids these difficulties. The odds on a voter's knowing a candidate are defined as $P_{cr}/(1-P_{cr})$, where P_{cr} is the probability that a voter knows the candidate. This term can take any value from zero to infinity; a logarithmic transformation of the term, $ln(P_{cr}/(1-P_{cr}))$, restricts the possible values of P_{cr} to a range of from 0 to 1 as the transformed term varies from minus infinity to plus infinity (Theil, 1971, p. 632). The transformed term, designated L_{cr}, replaces *CR* in equation 7.1.

In order to get observations for P_{cr} it is necessary to group the survey observations and estimate P_{cr} as F_{cr}, the proportionate frequency with which respondents in each group remember the candidate's name. The standard procedure, followed in this instance, is to group observations on intervals of the independent variables. For each group the average expenditure (in thousands of dollars) is calculated along with the recognition frequency. The equation is now

$$L_{cr} = a + b_1\bar{E} + e. \qquad (8.1)$$

Since the groups providing observations for this equation are of different sizes, the errors are heteroscedastic, and so weighted least squares are used to compute the estimates.[16]

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[14] The only respondent variable that has a consistent and significant impact on candidate recall is attentiveness to the mass media; voters who follow the election regularly in at least one mass medium are significantly more likely to remember the candidates' names. See Jacobson (1976).

[15] The simple correlations between the previous office variable (*PO*) and the candidate recall variable (*CR*) for 1974 Senate and 1972 and 1974 House voters are, respectively, .11, .09, and .13; with campaign spending controlled, the corresponding partial correlations are .01, .03, and –.01.

Ordinary (weighted) least squares estimates of equation 8.1 would still be biased and inconsistent because it is part of a simultaneous equation system in which $\bar{E}$ is assumed to be endogenous and therefore not independent of the error term. The 2SLS procedure is again in order. First E is regressed on the exogenous variables,

$$E^* = a + b_2 PO + b_3 P + b_4 NPS + b_5 NI + e \qquad (9.1a)$$

and the results are used to compute expected values, E^*, for each observation. Grouping these observations on intervals of values of E^* and taking the recognition frequency and mean value of E^* $(\bar{E}^*)$ for each group, the second stage equation,

$$L_{cr} = a + b_1 \bar{E}^* + e \qquad (9.2)$$

may be estimated using OLS.

This was the procedure followed for the two House election sets. For the Senate elections, the grouping was done by states, and the observations on expenditures and the 2SLS instruments for spending are therefore not averages but rather figures for the separate contests in each individual state. Because the states vary widely in population and hence presumably in the cost of conducting an equivalent campaign, expenditures were divided by the voting-age population in each state and were entered as cents per voting-age individual (*EPV*). But per-voter spending declines as the size of the population increases (campaign spending enjoys economies to scale); therefore, the equation estimating Senate campaign spending includes as a conditioning variable the natural logarithm of the voting-age population (in thousands). It replaces *NPS*, which was dropped because it had no effect whatever on spending when the other variables were controlled.[17] The first-stage equation for Senate elections is thus

$$EPV^* = a + b_2 PO + b_3 P + b_4 \ln VAP + b_5 NI + e \qquad (9.1b)$$

where *EPV* is spending in cents per voting-age individual, *ln VAP* is the natural log of the voting-age population (in thousands), and the other variables and coefficients are as defined previously.

The regression estimates for equations 9.1a, 9.1b, 9.2, and, for comparison, 8.1 are listed in Table 10. Equation 8.1 was also used to estimate the equivalent parameters for incumbents in the three election sets and these also appear in the table. Results in this form are not easy to interpret, so they are displayed graphically in the three figures. The estimated curve for 1972 House candidates is shown for spending up to \$160,000, that for 1974 up to \$200,000, and the scales are adjusted to assure comparability; inflation must be acknowledged.

The coefficients of determination (R^2's) and the standard errors are not listed for the logit equations because they are rendered meaningless by the grouping. Estimates based on individual rather than grouped data (from OLS and reduced form versions of equation 7.1 indicate that the relationship between spending and candidate saliency is significant at .001 for nonincumbents in all election sets and for incumbents in the 1974 elections. The relationship for 1972 House incumbents is not statistically significant even when the data *are* grouped for the logit analysis.

* * *

The hypothesis that campaign spending is more useful to nonincumbents than to incumbents because of its greater effect on how frequently they are remembered by voters is, in general, well supported by these data. The evidence is strongest for the 1972 House elections; the amount spent by incumbents had little apparent effect on the probability they would be remembered; the expected gain is only .02 as spending increases from \$0 to \$160,000 whereas awareness of nonincumbents more than doubles over the same range of expenditures. Those few nonincumbents fortunate enough to spend over \$150,000 were as likely to be remembered as incumbents.

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[16] Grouping the observations on interval values of the independent variable minimizes the loss of efficiency engendered by grouping. The groups are of different sizes because fixed intervals of the explanatory variable were employed in their formulation. For this reason, equations using the grouped observations were estimated by weighted least squares, the weights being proportional to the reciprocal of the approximate standard deviation of the error term, e, where the variance of e is estimated as $1/(NF_{cr}(1-F_{cr}))$, N being the number of observations in a group and F_{cr} the proportionate frequency a candidate is known for that group. The procedure is from Theil (1971, p. 635). The number of observations (groups) in the several logit equations varies from 13 to 29. The mean number of cases in the Senate groups is 34; that in the House groups, 61.

[17] *NPS*-for Senate elections was measured as explained in footnote 10. This variable, when included, was not a statistically significant determinant of spending and had a perverse sign. One extreme case had to be dropped from both the incumbent and nonincumbent Senate groups. This was South Dakota, where the challenger spent twice as much per eligible voter as any other nonincumbent and the incumbent more than four times as much as any other incumbent in the states covered by the survey (N=24). Both candidates were known by all the voters surveyed in the state (N=26).

In 1974, spending by both incumbents and nonincumbents had a positive effect on the probability that voters would remember them. Nonincumbents did benefit more than incumbents from the same amount of spending, but the difference is not so great as it was in 1972; they gain about .13 more than incumbents as spending increases from \$0 to \$200,000. We are reminded that 1974 was an unusual year for Republican incumbents. Four whose districts were covered in the survey spent over \$200,000; three of them had also spent over \$100,000 in 1972; their collective frequency of recall by voters was .93 (N=29). Very high levels of spending evidently do make a difference, even for incumbents. Parenthetically, three of them lost and the fourth squeaked by with 51.1 percent of the vote.[18] Saliency is obviously not the only important factor determining candidate success (a point ably argued by Abramowitz, 1975).

Table 10. OLS and 2SLS Logit Regression Equations Estimating the Effects of Campaign Spending on Voter Awareness of House and Senate Candidates

Equation 9.1a: House Elections

1972
$$E^* = 2.28 + \underset{(4.17)^a}{17.11}\,PO + \underset{(3.58)}{5.19}\,P + \underset{(.128)}{.682}\,NPS + \underset{(4.14)}{60.97}\,NI$$
$$N = 718 \qquad R^2 = .36$$

1974
$$E^* = -46.34 + \underset{(3.22)}{26.37}\,PO + \underset{(2.74)}{33.27}\,P + \underset{(.139)}{1.77}\,NPS + \underset{(3.72)}{21.62}\,NI$$
$$N = 976 \qquad R^2 = .47$$

Equation 9.1b: Senate Elections

1974
$$EPV^* = 205 + \underset{(2.89)}{41.8}\,PO + \underset{(2.51)}{12.6}\,P + \underset{(2.27)}{.04}\,NI + \underset{(1.15)}{24.8}\,\ln VAP$$
$$N = 997 \qquad R^2 = .46$$

Equation 9.2: 2SLS

House Elections
1972 $L_{cr} = -1.25 + .0087\,\overline{E}^*$
1974 $L_{cr} = -1.20 + .0104\,\overline{E}^*$

Senate Elections
1974 $L_{cr} = -.615 + .0178\,\overline{EPV}^*$

Equation 8.1: OLS

Nonincumbents

House Elections
1972 $L_{cr} = -1.22 + .0076\,\overline{E}$
1974 $L_{cr} = -1.10 + .0094\,\overline{E}$

Senate Elections
1974 $L_{cr} = -.750 + .0444\,\overline{EPV}$

Incumbents

House Elections
1972 $L_{cr} = -.036 + .0005\,\overline{E}$
1974 $L_{cr} = -.129 + .0078\,\overline{E}$

Senate Elections
1974 $L_{cr} = .521 + .0209\,\overline{EPV}$

[a] Standard error of regression coefficient.

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[18] The candidates were Samuel Young, Illinois 10th, spending \$251,200 in 1974 and \$206,166 in 1972; William Hudnut III, Indiana 11th, with \$201,700 in 1974 and \$163,442 in 1972; Joel T. Broyhill, Virginia 10th, with \$248,700 in 1974 and \$141,290 in 1972; and Sam Steiger, Arizona 3rd, who spent \$203,900 in 1974, but only \$37,691 in 1972. Steiger was the only winner.

Spending was notably more effective in increasing the awareness of House candidates of all kinds in 1974 as compared to 1972. An inescapable inference is that in presidential election years, messages from congressional candidates are crowded out by those coming from the presidential campaigns; at midterm, with less competition, congressional campaigns reach the intended audience more consistently. Information on future elections will be necessary to test this interpretation.

For both the 1972 and 1974 House elections, the 2SLS and OLS estimates are almost identical; by this evidence, simultaneity bias was not a problem in the OLS estimates of the relationship. These findings suggest that the structure here is actually recursive; spending affects saliency, but saliency has little effect on spending.

The results for Senate elections, however, indicate that simultaneity bias was present in the OLS estimate. The slope of the 2SLS regression coefficient for nonincumbents is much less steep than the OLS slope; comparing the 2SLS estimate to the OLS estimate for incumbents, the conclusion must be that spending affects both groups similarly. This corresponds to the finding reported in the first section that Senate incumbents benefit from their own campaign spending more than do House incumbents. No easy explanation for this difference comes to mind; it may have to do with the greater prominence, intensity, or technological sophistication of Senate campaigns; an answer awaits further research.

A few points dealing specifically with incumbents and their challengers are in order. It should be emphasized that even if spending has the same marginal effect on the ability to recall names of challengers and incumbents, incumbents begin with such a great advantage in saliency that an equal increase in spending may still benefit the challenger. For one thing, it will decrease the *proportionate* advantage in awareness enjoyed by the incumbent. For example, incumbent senators are remembered 1.8 times

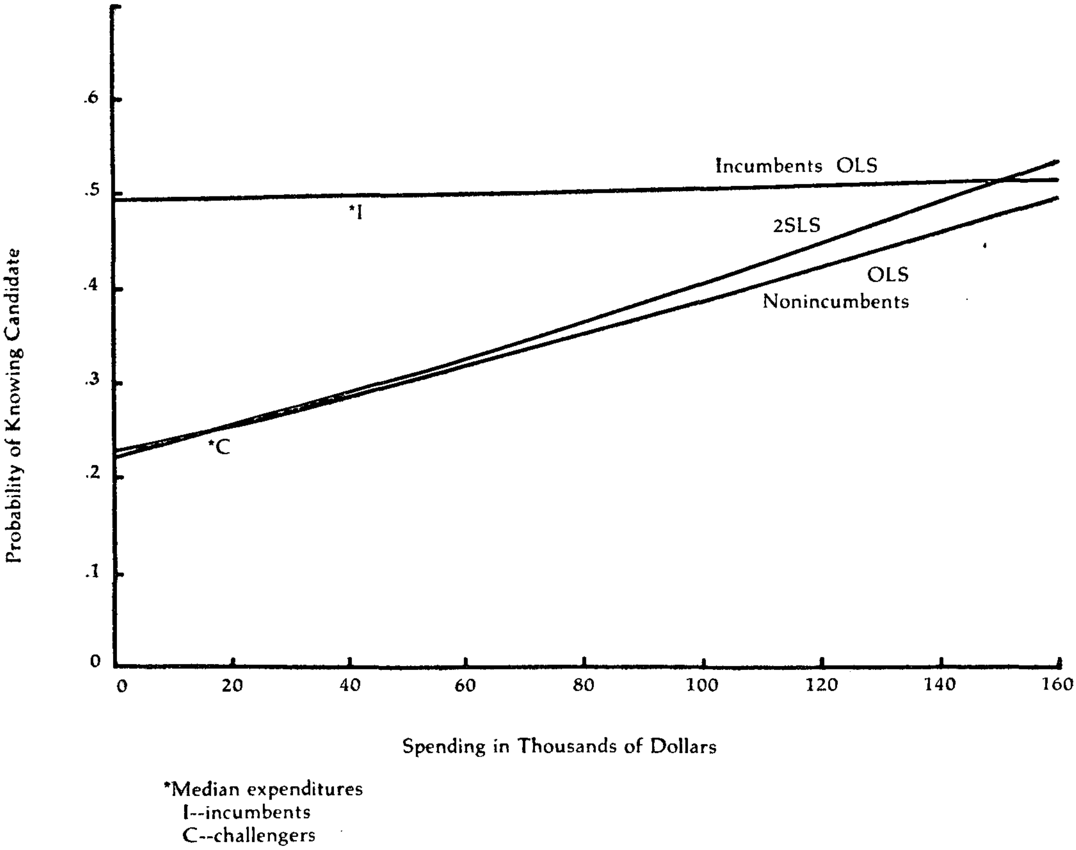

Figure 1. U.S. House of Representatives 1972 Elections

as frequently as nonincumbents (by the 2SLS estimates) if no money is spent; the figure drops to 1.4 at 70 cents per eligible voter even though the absolute gain in saliency is about the same for both groups. In addition, very few voters know a challenger without also knowing the opposing incumbent (about 1 percent in these surveys), while over a quarter of the respondents typically know the incumbent without knowing the challenger. Greater spending might therefore help challengers more than incumbents by increasing the number of instances in which *both* candidates are known.

The points at which the estimated curves intersect the mean expenditure level for challengers and incumbents are indicated on the figures. This information reiterates what is already known about the incumbent's spending advantage and displays its connection with the saliency advantage. Clearly, challengers must spend much more than they typically do—and much more than incumbents—if they hope to match the incumbent's saliency.

What's in a Name?

Campaign spending is important to candidates who need to make themselves known to voters; voters are more likely to vote for candidates whose names they recall. Since remembering a candidate's name has itself been given no weighty theoretical significance, it must be interpreted as an indicator of some sort. In earlier work I have argued, without supporting evidence, that candidate recognition should be considered a threshold indicator. That is, we should not assume that respondents who answer positively and correctly know nothing but the name of the candidate (although for some this may be true), but rather the ability to remember a candidate's name is best understood as a sign that the respondent "has crossed a minimal threshold essential to the acquisition of further information and to the elaboration of opinions about the candidate" (Jacobson, 1976, p. 17).

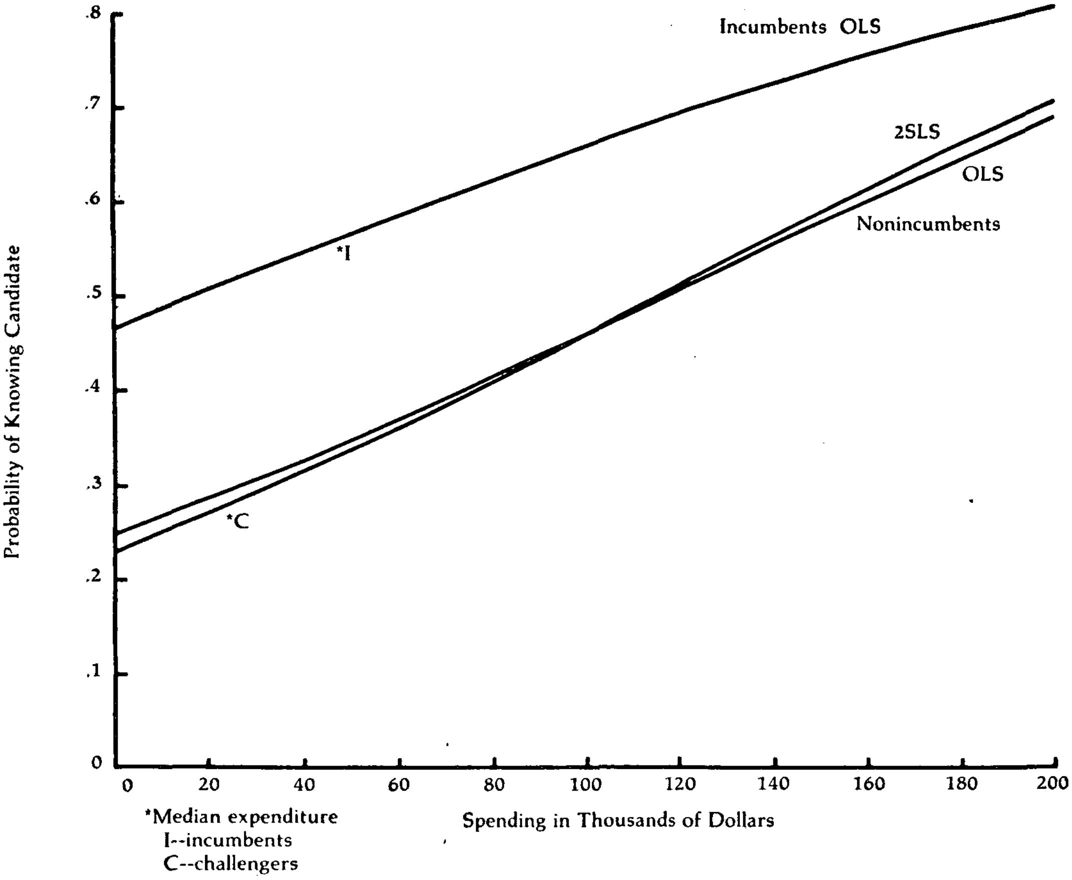

Figure 2. U.S. House of Representatives 1974 Elections

More recent studies suggest that the issue is more complicated. Abramowitz (1975) found that many of his respondents were quite ready to offer and amplify opinions on how incumbent members of Congress had performed their jobs without being able to recall their names. Ferejohn's investigation (1977) led him to conclude that incumbents are now enjoying an electoral advantage extending beyond what can be explained by greater familiarity to voters, since incumbents are favored even by voters who remember neither candidate's name.[19] Evidently it is not necessary to remember politicians' names in order to have an opinion about them. What does name recall indicate, then? The 1974 survey contains some items through which the question can be explored. Regarding the Senate races, respondents were asked, in addition to the name-recall question, a series of questions that can be summarized as "Was there anything in particular about the Democratic (Republican) candidate that made you want to vote for (against) him (her)? What was that?"[20] Responses to these questions were first recorded simply as positive (if anything made the respondent want to vote for the candidate), negative (if anything made the respondent want to vote against the candidate), or no response (the respondent mentioned nothing for or against the candidate) and were crosstabulated with responses to the recognition question. The results, broken down by partisanship, appear in Table 11.

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[19] This finding may be an artifact of the way "candidate familiarity" is measured. Surely voters may *recognize* the incumbent's name when they see it on the ballot without necessarily being able to *recall* it when asked by an interviewer.

[20] See variables 2177 to 2192 in the 1974 SRC survey codebook (Miller, Miller, and Kline, 1975, pp. 105–12).

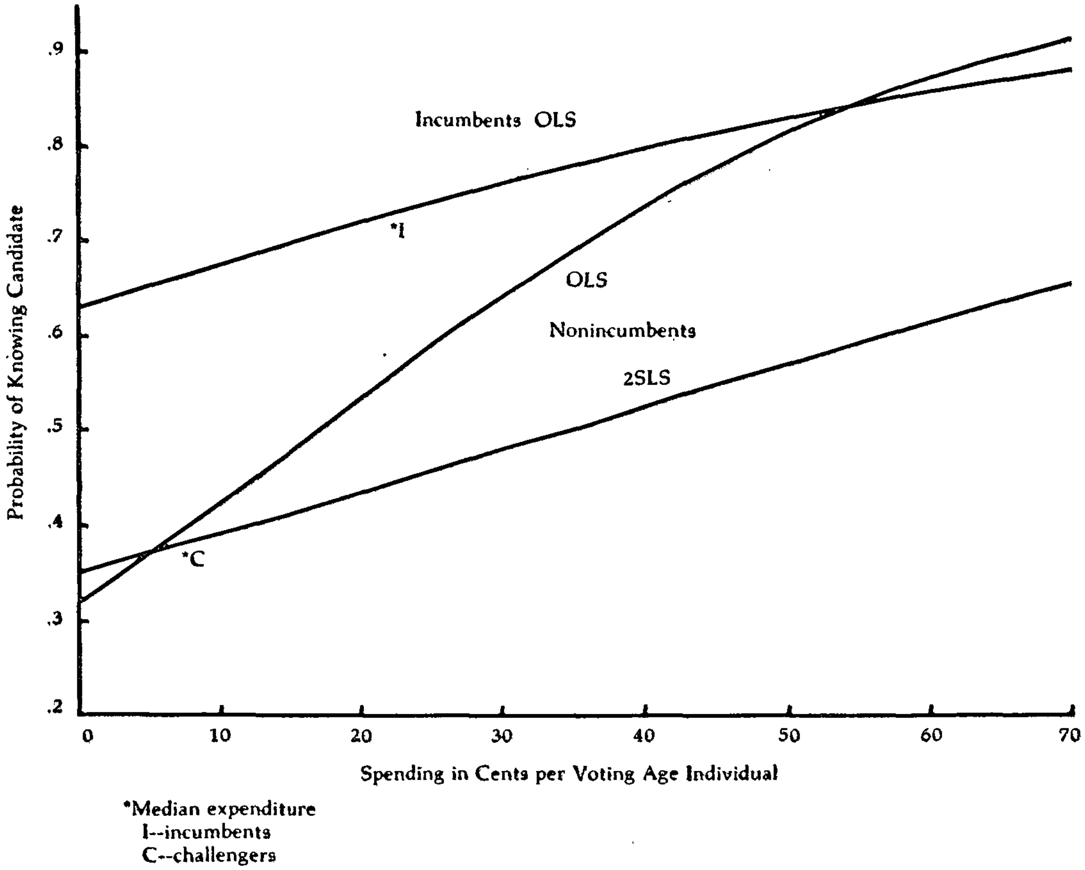

Figure 3. U.S. Senate 1974 Elections

Respondents (and only those who reported voting are included in the sample) who know the names of candidates are also much more likely to have something to say about them. Nearly two-thirds of the voters who could not recall the candidates' names had nothing to say about them either. Less than one-fifth of those accurately naming a candidate had no further comment. Although the effects of partisanship are quite apparent, aware voters more frequently find something good *and* bad about both their own and the other party's candidate. Familiarity does not invariably produce a favorable evaluation by any means. The relative gain in positive evaluations associated with recognition occurs primarily among a candidate's fellow partisans and, to a lesser extent, independent voters; among the other party's supporters the greater proportion of positive responses arising from awareness is more nearly matched by the increase in proportion of negative responses.

From another perspective, Table 11 makes it clear that ignorance of a candidate's name does not preclude expressing an opinion about that candidate; a third of the voters in this category were willing to do so. Do respondents who do not remember the candidate's name use different evaluative criteria than do respondents who are aware of the candidate? A more detailed recoding of the answers reported in the survey provides a way of finding out. All positive and negative responses (and up to three of each were recorded by the interviewers) were classified as *personal* (those referring specifically to characteristics of the candidates themselves), *party* (references to one of the parties or to the candidates themselves exclusively as partisans), or *mixed*.[21] These were cross-tabulated with responses to the recognition question; the results are found in Table 12. Not surprisingly, voters who do not remember candidates' names more readily resort to partisan criteria to evaluate them; personal comments are much more frequent among voters knowing candidates. Still, nearly a third of the unaware group employ purely personal criteria without remembering who the person is. Notice, incidentally, that aware voters are more likely to have multiple comments about the candidate; they average 1.7 responses, the unaware group 1.2.

Two tentative conclusions are warranted. Although the ability to remember a candidate's name is not a precise threshold–voters are often able to evaluate candidates without this piece of information–there is a substantial difference in both the frequency and character of evaluative comments between voters who do and do not recall the candidate. And the gains to be made from campaigning–and thereby making oneself known to voters–derive from gathering support among one's own partisans and independent voters rather than from con-

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[21] Responses coded 00, 01, and 05 in the hundred series for these questions were considered party references; those coded 02, 03, and 04 were classified as personal references; and those coded 06 through 12 were considered mixed. See Miller, Miller, and Kline (1975, pp. 400–15) for the complete coding categories.

Table 11. Voter Awareness and Evaluation of 1974 Senate Candidates

	Evaluation				
Percent Evaluating:	Positive	Negative	Positive and Negative	None	
Own Party's Candidate					
Known	56.4	12.5	15.4	15.7	(479)[a]
Not Known	38.1	4.6	1.5	55.8	(260)
Difference	18.3	7.9	13.9	–40.1	
Other Party's Candidate					
Known	21.9	40.0	15.7	22.4	(402)
Not Known	5.7	27.3	1.0	67.7	(300)
Difference	16.2	12.7	14.7	–45.3	
Independent Voters, Candidate is					
Known	36.4	27.3	18.2	18.2	(77)
Not Known	10.0	8.3	1.7	80.0	(66)
Difference	26.4	19.0	16.5	–61.8	

[a] Number of cases from which percentages were computed. The sample is weighted.

Source: The 1974 SRC survey.

verting the opposition. This, of course, is part of conventional wisdom.

An overview of the connection between the simplified candidate evaluation index and the reported vote for senator in 1974 will complete this part of the analysis. Table 13 displays this relationship. Evaluation of both candidates has a noticeable impact on voting behavior. And the effects are as we would anticipate: positive evaluations increase the likelihood of voting for a candidate, negative evaluations decrease that likelihood, and the effect in either case depends also on the evaluation of the other candidate. Defections are therefore concentrated in the upper right-hand corner of the partisan table, party loyalty is predominant in the lower left-hand corner. A comparable pattern occurs among independent voters.

To sum up briefly, then: our evidence is that campaign spending helps candidates, most particularly nonincumbents, by bringing them to the attention of voters. It is *not* the case that

Table 12. Voter Awareness and Criteria for Evaluation of 1974 Senate Candidates

	Evaluative Criteria			Number of Comments	Number of Respondents
Percent of Evaluations	Personal	Party	Mixed		
Positive					
Knowing Candidate	60.7	8.9	30.4	957	538
Not Knowing Candidate	37.1	31.1	31.7	167	134
Negative					
Knowing Candidate	44.4	14.5	41.1	601	393
Not Knowing Candidate	24.6	43.0	32.5	114	108

Source: The 1974 SRC survey. The sample is weighted.

Table 13. Evaluation of Senate Candidates and Voting Behavior in 1974

Percent of Partisan Voters Defecting

	Evaluation of Own Party's Candidate			
Evaluation of Other Party's Candidate	Positive	Positive and Negative	Negative	Marginal Totals
Positive	28.6 (21)[a]	77.1 (35)	77.1 (35)	65.9 (91)
Positive and Negative	4.5 (139)	31.1 (45)	56.0 (25)	16.3 (209)
Negative	4.2 (167)	3.4 (59)	40.0 (5)	4.8 (231)
Marginal Totals No Evaluation: 16.5 (115)	5.8 (327)	30.9 (139)	66.2 (65)	19.8 (531)

Independent Voters: Percent Voting for Democrat

	Evaluation of Democrat			
Evaluation of Republican	Positive	Positive and Negative	Negative	Marginal Totals
Positive		0.0 (11)	20.0 (5)	6.3 (16)
Positive and Negative	88.9 (9)	66.7 (3)	0.0 (5)	58.8 (17)
Negative	100.0 (2)	83.3 (6)	100.0 (1)	88.9 (9)
Marginal Totals	90.0 (11)	35.0 (20)	18.2 (11)	45.2 (42)
No Evaluation: 40.0 (15)				

[a] Number of cases from which percentages were computed. The sample is weighted.

Source: The 1974 SRC survey.

well-known candidates simply attract more money; rather, money buys attention. Voters who are aware of candidates are also more likely to have opinions about them, both positive and negative, and the net gain in positive evaluations for candidates who do succeed in getting the attention of voters comes primarily from adherents of their own party and from independents. And voters' evaluations of the candidates strongly influence how they cast their votes. Since nonincumbents have the most to gain from campaigning, it is not surprising that their level of spending has a greater impact on the outcomes of elections than does that of incumbents.

Implications for Campaign Finance Policy

The findings reported here have important implications for campaign finance policy reform. For House elections, both the OLS and the 2SLS models indicate that the marginal gains from a given increase in campaign spending are much greater for challengers than for incumbents. The unmistakable conclusion to be drawn from this is that, in general, any increase in spending by both candidates will help the challenger. Public subsidies—or any other policy which gets more money into the hands of challengers—should therefore make House elections more competitive. Incumbents will also get more money under such circumstances, but since for them raising money is not the problem it is for challengers and because their additional spending does not counterbalance the effects of greater spending by challengers, this will not work to their benefit.

On the other hand, any reform measure which decreases spending by the candidates will favor incumbents. This includes limits on campaign contributions from individuals and groups as well as ceilings on total spending by the candidates. Even though incumbents raise money more easily from all sources, limits on contributions will not help challengers because the problem is *not* equalizing spending between candidates but rather simply getting more money to challengers so that they can mount competitive races. Anything that makes it harder to raise campaign funds is to their detriment.

Ceilings on permissible spending, if they have any effect on it at all, can only lessen competition. The consequences of subsidies combined with limits—constitutional by the decision in *Buckley* v. *Valeo* (1976)—depend on the size of the subsidy provided and the limit imposed. For example, the major public financing bill before the House in the 95th Congress, HR 5157, would provide partial public funding on a matching basis and impose a ceiling of $150,000 on general election expenditures in House contests. Ignoring the problem that the data include primary election spending, and adjusting for inflation, the equations can be used to estimate the hypothetical effects of equivalent subsidies and limits in 1972 and 1974. The results of this exercise have been reported in detail elsewhere (Jacobson, 1977). They suggest that the legislation would have had very different consequences in the two election years. It would have diminished the expected number of successful challenges in 1972 because the limit would have been too low; but in 1974 the same financing system would have increased the number of predicted challenger victories substantially, with an even greater increase had the ceiling not been in force.

The subsidies and spending limits proposed for Senate elections under S 926, the Senate public funding bill, would have had slightly different consequences. They would have made no difference at all in 1972, but might have increased the number of successful challenges in 1974. The limits are evidently set high enough to avoid harming challengers. In 1974, the law would have reduced incumbent spending and increased challenger spending, to the definite benefit of the latter (Jacobson, 1977).

An explanation of the divergent effects of these laws in 1972 and 1974 doubtless lies in the different political circumstances of the two election years. In 1972, despite the decisive Nixon victory, national short-term forces do not seem to have been important in House elections; note the coefficients on P for 1972. Quite the opposite is true of 1974. Republicans were at a serious disadvantage because of Watergate, the collapse of the Nixon administration, and Ford's unpopular pardon of the former president. Democrats could therefore unseat incumbent Republicans with much greater ease (and with a good deal less money) than would have been the case in 1972. Almost all of the predicted 1974 winners are Democrats under any financing system; the partisan balance is nearly even for 1972.

Thus public funding and spending limits of the kind proposed in HR 5157 and S 926 would have different consequences according to varying political circumstances. If strong short-term forces favor the candidates of one party or another (or of different parties in different localities), less money will be needed by challengers of the advantaged party for them to win; subsidies will be more effective, even in relatively small amounts, and ceilings on spending will not be particularly damaging to their chances. In the absence of strong short-term forces, however, the subsidies may not be high enough to help significantly, and the ceilings may actually diminish the possibility of unseating an incumbent. Legislation of this kind has the potential to exaggerate whatever trends exist at the time of the election. Incumbents may be given an added margin of safety in election years when short-term forces are weak, but when these forces are strong, the turnover in congressional seats should be greater.

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Uncertainty and the Formal Theory of Political Campaigns *

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Many empirical investigations indicate that information is scarce and therefore costly to holders and seekers of public office. Indeed, some studies suggest that imperfect information may account for important aspects of the behavior of politicians. Nevertheless, there is as yet no theoretical investigation of political decision making that illustrates the impact of costly information or behavior.

In this paper the authors develop a model of electoral competition in which the candidates are only imperfectly aware of public preferences over issues and in which they may have the opportunity to increase the amount of information they hold at some cost. It turns out that the absence of perfect information profoundly affects the strategic structure of candidate competition. If information is costless, two-party electoral contests are naturally modeled as symmetric two-person zero-sum games. However, if candidates have distinct beliefs about voter behavior, the natural game-theoretic representation becomes a non-zero-sum game. This article is concerned mostly with analyzing the consequences of this transformation.

1. Introduction

Since the publication of Downs' seminal book two decades ago, the concepts and theories that he introduced have led to a large and expanding literature on the theory of electoral competition and voter decision making. While some of this work has abandoned the assumption that voters have perfect information about candidates' issue positions (see Shepsle, 1972), nearly all of it assumes that candidates know how the voters will respond when any given platform is offered. Yet many empirical works on Congress and the presidency find that political decision makers are uncertain about voters' responses to their actions (see Bauer, Pool and Dexter, 1972; Kingdon, 1968), and claim that this fact is fundamental to understanding their behavior.

This paper explores the neglected issue of candidate uncertainty. It examines the strategic decision making of candidates in duopolistic elections in which they are uncertain about the outcomes of majority-rule decisions in pairwise contests of alternative positions on political issues.

Part of the motivation for this paper lies in the relatively unrobust predictions of existing theoretical models of political campaigns. It is well known that the likelihood of cyclic majorities is very high if individual preferences are randomly distributed over all possible orderings of the relevant alternatives (see DeMeyer and Plott, 1970; Kramer, 1973). As a result, Downs predicted that without severe restrictions on the manner in which alternatives are put before the electorate, political outcomes should be unstable. The winning issue position and candidate in one election should be defeated in the next; further, with certainty of defeat facing incumbents, the connection between actions after an election and campaign positions before the election could be expected to be slight.

Even if there are no cycles under majority rule, equally implausible predictions arise. If a single majority-rule winning position is known to exist, both candidates will take it, and voters will be indifferent as to who wins. Elections will end in ties and voters will have no incentive to vote.

Collective choice theory based upon rational individual behavior can be extended in two ways to make its predictions more in line with reality. The first, which we will not explore here, is to constrain each candidate's feasible set of election strategies in such a way that majority rule cycles are not exploited.[1]

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*An earlier version of this paper was presented at the 1974 Mathematical Social Science Board Workshop on "Uncertainty, Political Processes and Public Policy." We are grateful for the comments and encouragement provided by Morris Fiorina, David Grether, Melvin Hinich, Richard McKelvey, Charles Plott, William Riker, Norman Schofield, Kenneth Shepsle, and an anonymous referee. We are also grateful to John Allen and Barry Weingast for research assistance.

[1] For example, candidates might care about the positions they take for reasons other than the impact of positions on election outcomes, might have monopoly rights to some issue positions (e.g., party

A second possible motivation for stable yet different platform choices is that candidates may have conflicting beliefs about voter behavior. If information is so costly that they cannot obtain new information too easily, strategic equilibria could exist which vanish in the absence of uncertainty.

In this article, we assume that two candidates, engaged in a duopolistically competitive election, wage a campaign that can be represented as a two-person game. The strategies of the game are the elements of the feasible set of alternatives that can be put before the electorate. A candidate selects one of the set of alternatives, and the election consists of a majority-rule choice by the electorate of one of the two alternatives offered by the candidates. Each alternative can be interpreted as a vector of positions on the issues in a campaign. Although in reality the set of feasible alternatives may differ between two candidates, in order to investigate the implications of uncertainty alone, we assume throughout that any alternative may be offered by either candidate.

At least one candidate is assumed to be unsure of the majority-rule outcome of at least some of the pairwise contests of alternatives. Candidates deal with this uncertainty by estimating the likely outcome of the uncertain contests, based upon whatever information they have about the distribution of opinion. If the candidates differ with respect to the amount of information they have and the manner in which they process it, the expectations of the candidates about the outcome of a particular pairwise contest can differ. In fact, if opinions are sufficiently different, both candidates might expect to win an election involving some particular choice of strategies by each candidate.

In the presence of uncertainty, candidates might wish to acquire additional knowledge about voter preferences before selecting a strategy. An obvious mechanism for obtaining this knowledge is the public opinion poll.[2] One issue examined in this article is the desirability to a candidate of acquiring better information about the true response of an electorate to a particular strategy pair.

The key element in the following analysis is the observation that uncertainty about electoral behavior can alter the game-theoretic structure of the campaign. A campaign in which outcomes are known with certainty is a game of perfect opposition since payoffs are symmetric (one person's win is another person's loss). But, ex ante, two candidates can both expect to win if at least one is uncertain about the true social-preference relation. In such a milieu, information can have a negative value, in the same sense that information about the true outcome of a football game before bets are placed can reduce the welfare of both of the participants in a wager. Both bettors on the outcome of a football game, if their bets are consistent with their own subjective probabilities over possible states of the world, believe, ex ante, that additional information is more likely to confirm their own beliefs than those of the other person, and hence both would believe that more information would be likely to cause the other person to change his or her mind about entering the bet. Translated to campaign decision making, this means that under some circumstances, depending upon the structure of their subjective expectations about electoral behavior, both candidates can attach a negative value to certain kinds of information since both expect that this information would only cause the pre-poll equilibrium strategy selections to be upset in a manner running counter to the perceived self-interest of each.

2. The Model and Some Examples

In this section we formalize what is meant by partial or incomplete information. In an electoral campaign each candidate communicates a sequence of messages to the electorate and the opponent. Somehow these messages are decoded by the voters in order to determine how (or whether) to cast their votes on election day. Voters may, of course, ignore many of the messages and "misinterpret" some as well, but when the campaign concludes they either vote for some candidate or abstain. The electorate

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identification, incumbency, etc.), or might be constrained in their selection of strategies by the past behavior of themselves or of other candidates of the same party.

[2] Two generic types of polls are possible. A candidate could take a hypothetical poll by asking voters to state a preference for one of the two issue positions, without either position being taken by or being identified with a particular candidate. Or a candidate could gauge the response of the electorate to an issue position by expounding a position publicly and then using the change in his or her standing in a popularity poll to estimate the effect of the position on his or her electability. In this article we abstract from the differences in these polls.

can be viewed in aggregate as a function mapping sequences of messages from the candidates as well as other information into a distribution of votes for the candidates. This function can be represented as a matrix, with the rows being the strategies available to one candidate, the columns being the strategies open to the second contestant, and the elements indicating whether the first candidate won, tied or lost the election run on that strategy pair. We call such a function a majority-dominance relation.

In this article voters are assumed to respond only to the most recent message in the sequence. These messages may be thought of as "issue positions" or "platforms." Further, the electorate is assumed to respond in an *unbiased* manner to the announced platforms of candidates. That is, if two candidates switch platforms, their vote totals switch.

Suppose that the candidates are not certain which of many possible majority-dominance relations truly represents the electorate. Given a set of three platforms (x, y, z), a total of 27 outcome matrices may reflect the electorate's true response to all possible campaigns.

This situation may be represented as a game by regarding the matrix representation of each majority-dominance relation as a symmetric, two-person, zero-sum game. Each element of such a matrix is +1, 0, or −1 according to whether the candidate playing the rows—candidate A—wins, ties or loses an election run on the corresponding pair of strategies.

Suppose that the candidates do not know which majority-dominance relation is true, but use the information available to them to produce a subjective probability distribution over all the possible dominance relations. Each candidate then selects an election strategy on these beliefs.

The reason for introducing alternative states of the world and probabilities over them, rather than hypothesizing payoff matrices directly, is to permit some generality in discussing the role of information. In particular, this formulation permits better information about the outcome of one strategy pair to influence other elements of the payoff matrix in a systematic fashion, as will be shown in section 3.

The following example illustrates the candidate game resulting from our assumptions. Assume that the candidates, A and B, have subjective probabilities, P_A^k and P_B^k, over $k = 4$ possible states of the world as follows:

$$\begin{array}{lcc}
\text{State of the World} & \text{I} & \text{II} \\
P_A^k & 1/4 & 1/2 \\
P_B^k & 1/4 & 0 \\
\text{Payoff Matrix} & \begin{bmatrix} 0 & -1 & 1 \\ 1 & 0 & -1 \\ -1 & 1 & 0 \end{bmatrix} & \begin{bmatrix} 0 & 1 & 1 \\ -1 & 0 & -1 \\ -1 & 1 & 0 \end{bmatrix}
\end{array}$$

$$\begin{array}{lcc}
\text{State of the World} & \text{III} & \text{IV} \\
P_A^k & 1/4 & 0 \\
P_B^k & 1/2 & 1/4 \\
\text{Payoff Matrix} & \begin{bmatrix} 0 & 1 & -1 \\ -1 & 0 & -1 \\ 1 & 1 & 0 \end{bmatrix} & \begin{bmatrix} 0 & -1 & -1 \\ 1 & 0 & 1 \\ 1 & -1 & 0 \end{bmatrix}
\end{array}$$

Assuming both candidates are expected value maximizers, candidate A behaves as if he or she were playing the following game, which is constructed by summing the state-of-the-world matrices weighted by the P_A^k's, and is denoted P_A.

$$P_A = \begin{bmatrix} 0 & 1/2 & 1/2 \\ -1/2 & 0 & -1 \\ -1/2 & 1 & 0 \end{bmatrix}$$

By similar construction, B is playing:

$$P_B = \begin{bmatrix} 0 & 0 & -1/2 \\ 0 & 0 & -1/2 \\ 1/2 & 1/2 & 0 \end{bmatrix}$$

Essentially what has happened is that uncertainty has transformed the candidate game into a nonzero-sum game. The elements of P_A and P_B, when incremented by one and divided by two, are, respectively, A's and B's subjective probabilities that A will win. The unique pure-strategy equilibrium point in this game is for candidate A to play platform (row) one and candidate B to play platform (column) three. The value of the game to A is 1/2 and the value to B is −1/2 (i.e., both believe they are likely to win the election).

If both candidates knew that the true state of the world was state I, the minimax strategy for each would be to play each platform with probability 1/3. The value of the game would then be zero for both candidates. The introduction of uncertainty has created a pure-strategy equilibrium and has made the value of the game nonzero for both candidates. This example would seem to capture Downs' essential idea in claiming that uncertainty could create stability in electoral competition. Of course, it is easy to come up with a case in which the opposite phenomenon occurs. The true state of the world might be II, which has a pure strategy equilibrium, while both candidates might believe that all states are equally likely, in which case the equilibrium strategy would be to play each strategy with probability 1/3.

A few important principles may be drawn from this very simple model. First, disagreement between the candidates can create new equilibria. Second, new information that changes the beliefs of the candidates can upset equilibria. Third, equilibria can have the property that the candidates adopt different platforms. This last phenomenon cannot occur in a classical spatial model unless the electoral response function is biased or the candidates place value on features of an election other than who wins.

While it is of interest to note that uncertainty may generate stable platform choices, it is perhaps even more surprising to learn that uncertainty may induce candidates to collude with each other. The following applications of some simple strategic structures that can arise in two-person games illustrate this point.

A "Prisoner's Dilemma" Campaign. Suppose candidates A and B accord subjective probabilities over four possible states of the world as follows:

State of the World	I	II
P_A^k	.7	.2
P_B^k	.05	.05

$$\begin{bmatrix} 0 & 1 & 1 \\ -1 & 0 & -1 \\ -1 & 1 & 0 \end{bmatrix} \quad \begin{bmatrix} 0 & 1 & -1 \\ -1 & 0 & 1 \\ 1 & -1 & 0 \end{bmatrix}$$

State of the World	III	IV
P_A^k	.05	.05
P_B^k	.7	.2

$$\begin{bmatrix} 0 & 1 & 1 \\ -1 & 0 & 1 \\ -1 & -1 & 0 \end{bmatrix} \quad \begin{bmatrix} 0 & 0 & 1 \\ 0 & 0 & -1 \\ -1 & 1 & 0 \end{bmatrix}$$

The resulting expected payoff matrix, written in bimatrix form, is then:

$$(P_A, P_B) = \begin{bmatrix} (\ \ 0, \ \ 0) & (.95, \ \ .8) & (\ .6, -.9) \\ (-.95, -.8) & (\ 0, \ \ 0) & (-.5, \ \ .5) \\ (-\ .6, -.9) & (\ .5, -.5) & (\ 0, \ \ 0) \end{bmatrix}$$

Recall that A seeks positive payoffs and B negative. This game is a prisoner's dilemma with payoff (0, 0), corresponding to each candidate taking the first platform. Each would be better off if the candidates played platform pair (three, two). But if the candidates were to take this latter position and obtain (.5, −.5) each would have an incentive to switch to platform one.

This sort of strategic structure might underlie a tacit or explicit agreement between two candidates not to campaign on a certain issue. Both candidates agree in this case that platform one is the most popular platform among the voters. Candidate A believes that platform one is unbeatable with probability .8, and that there is a cycle with probability .2, while B believes that with probability .95 platform one is unbeatable. Precisely because they agree that platform one is popular, they would prefer to agree to restrict the campaign to platforms two and three over which their beliefs conflict.

Scholars and editorialists have sometimes observed that an issue about which the public holds strong beliefs (busing or corruption in office, for example) failed to arise in a campaign. This phenomenon is usually ascribed to the presence or absence of the candidates' "public responsibility." But the prisoner's dilemma example indicates that the failure of a "dominant" issue to arise could be due to the same phenomenon that leads bettors not to wager on football games about which their expectations are identical. If so, "public responsibility" is really quite something else—collusion to prevent a public policy that is a clear majority-rule winner.

The implications of these phenomena for democratic theory depend, of course, on the ability of candidates to make and enforce agreements on campaign strategies. This ability depends, among other things, on the cost of information to the candidates, as is demonstrated in section 3. It also depends upon each candidate knowing the ranking by the opponent of alternative campaigns in the subset of possibilities that constitutes the prisoner's dilemma. In the absence of this knowledge, the candidates will not perceive the possibility of advantageous collusion.

A "Battle of the Sexes" Campaign. Another set of incentives can arise if the candidates have somewhat different beliefs. For example, assume that the candidates put all subjective probability on the following five states of the world:

$$\begin{array}{cccc} & \text{I} & \text{II} & \text{III} \\ P_A^k & 1/4 & 3/8 & 3/8 \\ P_B^k & 1/4 & 0 & 0 \\ & \begin{bmatrix} 0 & +1 & -1 \\ -1 & 0 & -1 \\ +1 & +1 & 0 \end{bmatrix} & \begin{bmatrix} 0 & -1 & +1 \\ +1 & 0 & -1 \\ -1 & +1 & 0 \end{bmatrix} & \begin{bmatrix} 0 & -1 & -1 \\ +1 & 0 & -1 \\ +1 & +1 & 0 \end{bmatrix} \end{array}$$

$$\begin{array}{ccc} & \text{III} & \text{IV} \\ P_A^k & 0 & 0 \\ P_B^k & 1/2 & 1/4 \\ & \begin{bmatrix} 0 & +1 & +1 \\ -1 & 0 & +1 \\ -1 & -1 & 0 \end{bmatrix} & \begin{bmatrix} 0 & +1 & -1 \\ -1 & 0 & +1 \\ +1 & -1 & 0 \end{bmatrix} \end{array}$$

The expected payoff matrix is then the following:

$$(P_A, P_B) = \begin{bmatrix} (\ 0,\ \ 0) & (-\tfrac{1}{2}, +1) & (-\tfrac{1}{4},\ \ 0) \\ (+\tfrac{1}{2}, -1) & (\ 0,\ \ 0) & (-1, +\tfrac{1}{2}) \\ (+\tfrac{1}{4},\ \ 0) & (+1, -\tfrac{1}{2}) & (\ 0,\ \ 0) \end{bmatrix}$$

This game has two pure-strategy equilibria, strategy pairs (two, one) and (three, two), with payoffs (1/2, −1) and (1, −1/2). Candidate B prefers the first, and candidate A the second, producing the "battle of the sexes" structure discussed in Luce and Raiffa. The important feature of this expected payoff matrix is disagreement over which equilibrium should be achieved, disagreement which could lead to strategies and threats that produce disequilibrium outcomes.

The information structure that produces this result has two elements: candidates differ in their assessment of the likely outcome of two strategy pairs, and the strategy pair about which the first candidate is more certain is the one about which the second candidate is less certain. In this case, candidate B is certain that if the electorate is faced with a choice between platforms one and two, one will prevail. Candidate A is less certain about the outcome of this contest, but believes the outcome is likely to be the opposite of B's belief. Similarly, A is certain that platform three would beat platform two in a pairwise contest, while candidate B is less sure of the outcome of such a contest, but disagrees with A. Each would prefer to fight the campaign on platforms which appear to guarantee victory rather than take a chance.

3. Polls and Information

Candidates who are uncertain about the behavior of voters on election day may find it worthwhile to seek information about the preferences of the electorate. This section examines some conditions under which information would be valuable to candidates. We restrict ourselves to a particular class of information-gathering processes which we call, suggestively, polls.

We define a *poll* as information about the true value of the cells of the subjective probability matrices P_A and P_B defined in section 2. A poll is *private* if only one of the candidates learns the result. If both candidates learn of the true value, the poll is called *public.* We assume that the outcomes of polls are certain so that if a public poll is taken, the corresponding elements of P_A and P_B are replaced by 1 or −1. This hypothesis may, of course, be relaxed. In particular, the outcome of the poll could be a probability of victory anywhere from zero to one, depending upon the nature and timing of the poll. Considering outcomes other than 1 or −1 greatly complicates the examples considered in this section, but does not change the essence of the results.

Candidates are assumed to revise their subjective probabilities in a Bayesian manner. That is, if a poll is taken on a particular element of the matrix of outcomes of strategy pairs, candidates revise their probability estimates over the possible majority-dominance relation so as to assign all probability to the subset of majority-dominance relationships consistent with the poll and to increase the probability assigned to each relationship in that set by the same proportion.[3]

The following example illustrates the effects of a poll on the subjective probability matrices and the equilibrium pair of strategies. This example is based upon the first example presented in section 2, in which, before a poll is to be taken, the only pure-strategy equilibrium is for A to play platform one and B to play three, with payoff vector (1/2, −1/2). Suppose that a poll is to be taken on element (one, two). The following table gives the revised subjective probabilities, strategies and outcomes for both candidates, given that the true value of (one, two) is either +1 or −1.

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[3] Poll outcomes between 1 and −1 would assign probabilities such that the expected outcome of the polled cell equaled the poll result, with probabilities associated with matrices having entries of a given sign in the polled cell incremented or decremented proportionately.

True Value of (1, 2)

Poll result	1	−1
State of the World	I II III IV	I II III IV
P_A^k	0 2/3 1/3 0	1 0 0 0
P_B^k	0 0 1 0	1/2 0 0 1/2
Equilibrium strategy of A	(1, 0, 0)	(0, 0, 1)
Equilibrium strategy of B	(1, 0, 1)	(0, 1, 0)
Payoff to A	1/3	1
Payoff to B	−1	0

In this case after the poll, A's expected return either drops to 1/3 or rises to 1, depending on the outcome of the poll. A's ex ante expectation of the change in the value of the game due to the poll is the sum of these outcomes weighted by the subjective probabilities over the possible results of the poll, e.g., $3/4(-1/6)+1/4(1/2) = 0$. That is, A is indifferent between having a poll and not having one. In this particular example, B is also indifferent about a public poll on (one, two).

A rather surprising result occurs if a public poll is proposed on (one, three). Going through the same operations as above, the ex ante evaluations of this poll are negative for both candidates. In particular, the expected payoff of the game to candidate A drops by 7/20 if a poll is taken on (one, three) while B is forced to surrender an additional 5/12.[4] In this case, both candidates are better off ex ante if they can prevent a public poll on this element. As it turns out, this "ignorance is bliss" phenomenon occurs under fairly general circumstances. Section 4 establishes sufficient conditions for both candidates to prefer preventing a public opinion poll on a given strategy pair. For the present we restrict our attention to some illustrative examples that provide a little insight into the effect on campaign strategy of increasing the amount of information held by candidates in an uncertain election.

The prisoner's dilemma example in section 2 was constructed to show that uncertainty may induce candidates to behave in rather curious and, one might say, unfortunate ways. We now consider the possibility that, rather than forming their strategies on the basis of limited information, one or the other of the candidates might choose to purchase additional information or that a third party—a polling agency—might introduce new information to the candidates.

The impact of a public poll on cell (three, two), the collusive outcome of the prisoner's dilemma, depends upon whether the candidates are colluding. If the candidates collude their expected pre-poll payoff is (1/2, −1/2). If a public poll is taken on (three, two) and Bayesian updating takes place, the expected post-poll payoff is (1/4, −3/4) so that, given pre-poll collusion, candidate A would oppose a poll while candidate B would favor it. If, on the other hand, the candidates failed to collude, both players would favor a public poll on (three, two).

The effects of a private poll are less clear because of the possibility of strategic behavior with respect to the selective release of private poll results. If B takes a private poll on (three, two), regardless of the outcome B's best strategy is to choose platform one. But candidate A, still being in the dark about the outcome of the poll, will interpret B's choice as an unwillingness to collude. In the absence of information, A may switch to platform one. But if candidate B thinks that A will react in this manner, and if before the poll the collusive outcome is chosen, B will find the poll information valueless.

Another possibility can also arise. If the result of a public poll on (three, two) is −1, the post-poll equilibrium is for A to choose platform three and B to play platform one, with payoff (1, −1). B could take a private poll and, if the outcome is −1, publicize it and play platform one while, if it is +1, suppress the information and switch to platform one. In this case, assuming that A believes the information that B distributes, A will choose platform three when B announces the poll result and the choice of platform one, but will play one if B picks platform one without announcing the result of the poll. If B adopts this strategy, the expected payoff of a private poll is −3/4, just what a public poll would have reported.

The "battle of the sexes" example illustrates still another strategic aspect of information. One possibility is for one candidate to threaten the other with the possibility of buying more information as a device to force a preferred strategy pair. In this example, candidate B can threaten to undertake a poll on the pairwise contest, platform two versus platform one, the results of which are as follows.

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[4] The poll result +1 leads to a mixed strategy by both candidates.

Note that the results of a poll on (two, one) are the negative of the results of a poll on (one, two).

Result = −1 ⇒

$$\begin{array}{cccccc} & \text{I} & \text{II} & \text{III} & \text{IV} & \text{V} \\ P_A^k & 1 & 0 & 0 & 0 & 0 \\ P_B^k & 1/4 & 0 & 0 & 1/2 & 1/4 \end{array}$$

$$P_A, P_B = \begin{bmatrix} (\;0,\; 0) & (+1,\; +1) & (-1,\; 0) \\ (-1, -1) & (\;0,\; 0) & (-1, +1/2) \\ (+1,\; 0) & (+1, -1/2) & (\;0,\; 0) \end{bmatrix}$$

Equilibrium (three, two), payoff (+1, −1/2)

Result = +1 ⇒

$$\begin{array}{cccccc} & \text{I} & \text{II} & \text{III} & \text{IV} & \text{V} \\ P_A^k & 1 & 1/2 & 1/2 & 0 & 0 \\ P_B^k & 0 & 1/2 & 1/2 & 0 & 0 \end{array}$$

$$P_A, P_B = \begin{bmatrix} (\;0,\; 0) & (-1, -1) & (\;0,\; 0) \\ (+1, +1) & (\;0,\; 0) & (-1, -1) \\ (\;0,\; 0) & (+1, +1) & (\;0,\; 0) \end{bmatrix}$$

Equilibrium (three, three), payoff (0, 0)

Expected Payoff to A with Poll =
1/4(+1) + 3/4(0) = 1/4

Expected Payoff to B with Poll =
1(−1/2) + 0(0) = −1/2

If A refuses to play B's preferred platform pair (two versus one), steadfastly sticking to platform three, B expects to lose nothing by polling on (two, one); however, A would stand to suffer a substantial loss. The worst A can do without a poll is platform pair (two, one) with payoff +1/2, which is preferable to the expected result of the threatened poll.

In this example, A has no effective information threat comparable to B's. Although the calculations are not presented here, it is easily shown that polls on the other two off-diagonal strategy pairs produce ambiguous results no more damaging to B than to A. Likewise, A has no effective counterthreat to B's threat to poll on (two, one). Thus, in this example, the strategic threat to gather new information resolves the ambiguity in the "battle of the sexes" campaign, creating a stable equilibrium.

4. The Formal Model

We assume that candidates select campaign strategies from a finite set of X alternatives, $X = \{a, b, c, \ldots, y, z\}$ and that each of a finite number, n, of voters has preferences on X that weakly order the alternatives. Let $n(xPy)$ be the number of voters preferring strategy x to strategy y. We define xMy if and only if $n(xPy) > n(yPx)$, and call M the majority-dominance relation. Numerous majority-dominance relations are possible, of course, over any set of alternatives. Any particular majority-dominance relation can be represented as a symmetric, two-person, zero-sum game with a payoff matrix defined as follows:

$$M(x, y) = \begin{cases} 1 \text{ if } n(xPy) > n(yPx) \\ 0 \text{ if } n(xPy) = n(yPx) \\ -1 \text{ if } n(yPx) > n(xPy) \end{cases}$$

Each candidate has a matrix of expected payoffs, P_A for candidate A and P_B for candidate B, which gives the expected payoff for each combination of platforms. Candidates are assumed to select Nash equilibria in choosing strategies. The interpretation of the payoff matrices that was given in section 2 was that:

$$\sum_{k \in K} P_A{}^k M^k = P_A$$

where K contains the indexes over states of the world, M^k is a skew-symmetric matrix representing the majority-dominance relation in a possible state of the world, and P_A^k is A's subjective probability that state k will occur. We assume that the elements of M^k are zero on the diagonal and +1 or −1 off the diagonal. As long as each element in P_A is less than or equal to 1 in absolute value, a probability distribution $\{P_A^k\}$ exists such that P_A can be expressed as in the above equation, so that no loss of generality is entailed by preventing off-diagonal elements of M^k from being zero.[5]

The first theorem illustrates that uncertainty can lead to pure-equilibrium strategies even if both candidates agree that the majority-dominance relation contains a cycle.

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[5] Proved in an unpublished appendix, available from the authors on request.

Theorem 1: Suppose in a three-platform election each candidate assigns subjective probabilities to majority-dominance relations such that the expected payoff matrix of each is cyclic. The campaign has a pure-strategy equilibrium involving two of the cycling elements if, and only if, the candidates disagree about which order of the cycle is more likely.

Proof. The formal proof of Theorem 1 follows from two observations: (1) cycles occur when off-diagonal elements of the payoff matrix alternate in sign, and (2) pure strategy equilibria require that the payoff for the equilibrium campaign is non-negative for both candidates, since either candidate can guarantee a zero payoff by playing the same strategy as the opponent. It can easily be shown that these requirements yield the proof of the theorem.

For a strategy pair satisfying Theorem 1 to be an equilibrium in the larger campaign depends upon the structure of subjective probabilities over outcomes of contests involving other strategies and, consequently, of the entire nature of subjective payoff expectation. One interesting case is the following:

Corollary 1.1. In an $n > 3$ platform election, if three platforms are known to cycle, the conditions of Theorem 1 obtain for cycling platforms and the candidates agree qualitatively on the outcome of all campaigns other than those involving the cycling elements, then strategy pairs satisfying the conditions of Theorem 1 are the only ones that give both candidates the expectation of winning.

Proof. Let $\{a,b,c\}$ be the cycling platforms. Since for all strategies $\{r,s\} \not\subset \{a,b,c\}$ and $r \neq s$ the candidates agree that either $r M s$ or $s M r$, each pair must be associated with an expectation of losing on the part of one candidate.

Q.E.D.

The strategy pair of Corollary 1.1 will not be a pure-strategy equilibrium, however, unless the structure of the game is such that neither candidate, given the choice of strategy by his or her opponent, has a strategy of greater expected payoff (e.g., a prisoner's dilemma situation). If, for example, both candidates agree that platform $r \not\subset \{a,b,c\}$ defeats all three cycling platforms, and if one candidate chooses a platform from $\{a,b,c\}$ satisfying Theorem 1, the other can guarantee a win by playing r. Only collusion between candidates can then result in a campaign run on platforms from $\{a,b,c\}$.

Define a full transitive subordering of an asymmetric binary relation over a set as a transitive ordering of all the elements of the set consistent with the binary relation. For example, the cycle $aQbQcQa$ contains three full transitive suborderings:

$$aQbQc,\ bQcQa, \text{ and } cQaQb.$$

Also, define the inverse of a transitive ordering as the inverse ordering, e.g., the inverse of $aQbQc$ is $cQbQa$. For a three-element set, all of the possible transitive suborders (and inverses) are:

$R_1: aQbQc$
$R_2: bQcQa$
$R_3: cQaQb$
$R_4: cQbQa$
$R_5: aQcQb$
$R_6: bQaQc$

Note that R_1, R_2 and R_3 are the full transitive suborderings of cycle C_1: $aQbQcQa$; R_4, R_5 and R_6 are the full transitive suborderings of cycle C_2: $aQcQbQa$; R_4, R_5 and R_6 are the inverse orderings of R_1, R_2 and R_3; and C_1 and C_2 are the only possible three-element cycles. Finally, define an n-element cycle as a cycle of the form $a_1 Q a_2 Q \ldots Q a_n Q a_1$ for $n \geqslant 3$.

If the conditions of Theorem 1 are satisfied, the three-platform campaign has three pure-strategy equilibria, one for each pairwise contest. The resulting game resembles the "battle of the sexes" structure discussed in the preceding section. We offer without proof the following corollary:

Corollary 1.2: A sufficient condition for the three-platform two-candidate campaign of Theorem 1 to have the structure of the "battle of the sexes" game is that both candidates believe that the majority-dominance relation is cyclic, that the candidates disagree about the order of the cycle, and that

$$\exists\, i,j \in \{1,2,3\} \ni : P_A^i > P_B^{i+3} \Leftrightarrow P_A^j < P_B^{j+3}.$$

The last condition of the theorem requires that, although the candidates disagree whether particular transitive orderings (R_i and R_j) or their inverses (R_{i+3}, R_{j+3}) are more likely, the ranking of transitive orderings by A according to the likelihood that each represents the true state of the world is not the same as the ranking by B of the inverse orderings. If the conditions of Corollary 1.2 are generalized to include positive subjective probabilities for cyclic majority-dominance relations, the corollary remains true if *i* and *j* are allowed to vary over the three transitive orderings within a cycle and the cycle itself.

Just as the certainty that a cycle exists does not preclude a pure-strategy equilibrium involving the cycling elements, the certainty that no cycle exists does not preclude the possibility that the payoff matrix facing the candidates contains a cycle.

Theorem 2. A candidate who believes that cycles are not possible will face an *n*-element cycle in his or her subjective payoff matrix for the campaign if, and only if, every full transitive subordering of the *n*-element cycle is regarded as more likely than its inverse ordering.

Proof. If *P* is a complete, asymmetric relation, every $n > 3$ element cycle contains a three-element subcycle, so that the theorem need only be proved for the three-element case. Suppose *a, b,* and *c* are the three strategies. Candidate A believes that only transitive majority-dominance relations (the R_i's defined above) are possible.

Using the notation introduced above, candidate A, with subjective probabilities P_A^i over the R_i, faces a payoff matrix as indicated in Figure 1.

P_A contains a cycle over $\{a, b, c\}$ if and only if the off-diagonal elements of P_A have alternating sign, or:

$$P_A^1 - P_A^2 + P_A^3 - P_A^4 + P_A^5 - P_A^6 > 0 \Leftrightarrow \qquad (1)$$

$$P_A^1 - P_A^2 - P_A^3 - P_A^4 + P_A^5 + P_A^6 < 0 \Leftrightarrow \qquad (2)$$

$$P_A^1 + P_A^2 - P_A^3 - P_A^4 - P_A^5 + P_A^6 > 0. \qquad (3)$$

Pairwise combinations of these inequalities produces the following equivalent relations:

$$P_A^3 - P_A^6 > 0 \Leftrightarrow \qquad (4)$$

$$P_A^1 - P_A^4 > 0 \Leftrightarrow \qquad (5)$$

$$P_A^2 - P_A^5 > 0. \qquad (6)$$

Similarly, linear combinations of (4), (5) and (6) can be constructed that yield (1), (2) and (3). Hence P_A cycles on $\{a,b,c\}$ if and only if (4), (5) and (6) are all true or all false.

Q.E.D.

The conditions of Theorem 2 are strong, as examination of the R_i demonstrates. For example, assume that Candidate A believes R_1 is the most likely of the six orderings listed above. For a cycle to come about in A's payoff matrix, A must also believe $P_A^3 > P_A^6$. Now R_6 differs from R_1 only by interchanging the first two elements, while R_3 moves the last-place finisher in R_1 to first place. Thus, the conditions of the theorem require that A believe that if he or she is wrong about R_1, it is more likely to be because the last-place element really ranks first while the others retain their relative ordering than because of an error about the ordering of adjacent elements in the ordering R_1—i.e., it is less likely that *b* is preferred to *a* than it is that *c*, the third-place element in the most likely relation, is preferred to both *a* and *b*.

Theorems 1 and 2 demonstrate that there is no direct connection between whether candidates behave as if the majority-dominance relation is known to be cyclic and whether they believe it is cyclic. If a cycle results because the conditions of Theorem 2 are met, Theorem 1 may still be satisfied. A pure-strategy equilibrium is still possible if the candidates do not agree which cycle has the property that its transitive suborderings are all more likely than their inverses.

The remainder of this section explores the process by which candidates gather information about the majority-dominance relationship and alter their strategies accordingly.

A *poll* supplies information about the true value of the polled cell, p_A^{ij} and p_B^{ij} of the matrices P_A, P_B, and by symmetry, the obverse elements of the payoff matrix. According to assumption, after a poll p_A^{ij} and p_B^{ij} are replaced by 1 or −1, depending upon which certain outcome transpires.

$$P_A = \left[\begin{array}{cccc} a & 0 & P_A^1-P_A^2+P_A^3-P_A^4+P_A^5-P_A^6 & P_A^1-P_A^2-P_A^3-P_A^4+P_A^5+P_A^6 \\ b & -P_A^1+P_A^2-P_A^3+P_A^4-P_A^5-P_A^6 & 0 & P_A^1+P_A^2-P_A^3-P_A^4-P_A^5+P_A^6 \\ c & -P_A^1+P_A^2+P_A^3+P_A^4-P_A^5+P_A^6 & -P_A^1-P_A^2+P_A^3+P_A^4+P_A^5-P_A^6 & 0 \end{array}\right]$$

Figure 1. Payoff Matrix

After a poll, candidates revise their subjective probability distributions in a Bayesian manner. If a poll is taken on the ij^{th} element and candidate A learns that the true value of this element is $x \in \{1,-1\}$, then for all k such that $M_{ij}^k = x$, if $\sum_{l\in L} P_A^l$ is nonzero:

$$P_A^{k*} = \frac{P_A^k}{\sum_{l\in L} P_A^l}$$

where $L = \{k \mid M_{ij}^k = x\}$. *If* $\sum_{l\in L} P_A = 0$, then $P_A^{k_1^*} = P_A^{k_2^*}$ for all $k_1, k_2 \in L$ and $\sum_{l\in L} P_A^l = 1$. In both cases $P_A^{k*} = 0$ for all k not in L.

First we prove a lemma that establishes a straightforward mechanism for calculating revised probabilities.

Lemma 1. If the candidates A and B have probability distributions $\{P_A^k\}$, $\{P_B^k\}$ such that strategy (i,j), $i \neq j$, in the game (P_A, P_B) has payoffs $(\alpha, -\beta)$, if a poll is taken on the ij^{th} element with outcome $x \in \{1, -1\}$, and if subjectively both 1 and -1 are possible (e.g., $\alpha < 1$, $\beta > -1$), the revised probabilities of the two candidates may be computed as follows:

$$P_A^{k*} = \frac{2}{1+x\alpha} P_A^k \quad \forall k \in L = \{k \mid M_{ij}^k = x\}$$

$$P_A^{k*} = 0 \qquad \forall k \notin L$$

$$P_A^{k*} = \frac{2}{1-x\beta} P_B^k \quad \forall k \in L$$

$$P_B^{k*} = 0 \qquad \forall k \notin L$$

Proof. Since candidates update probabilities in a Bayesian fashion, for all $k \in L$,

$$P_A^{k*} = \frac{P_A^k}{\sum_{l\in L} P_A^l}, \text{ and } P_A^{k*} = 0 \quad \forall k \notin L.$$

Now $\alpha = \sum_{k\in L} P_A^k x - \sum_{k\notin L} P_A^k x$

And since $x^2 = 1$

$$\sum_{k\in L} P_A^k - \sum_{k\notin L} P_A^k = x\alpha$$

By adding

$$\sum_{k\in L} P_A^k + \sum_{k\notin L} P_A^k = 1$$

we obtain

$$2 \sum_{k\in L} P_A^k = 1 + x\alpha$$

or

$$\sum_{k\in L} P_A^k = \frac{1+x\alpha}{2}$$

which, when substituted into the formula for P_A^{k*}, produces the hypothesized result. The same argument obtains for candidate B.

Q.E.D.

For convenience let t_{ij} denote the true outcome of the contest between i and j. For example, $t_{ij} = 1$ means that platform i beats platform j. We continue to assume that $t_{ij} = 1$ or -1 for all $i \neq j$ and that $t_{ii} = 0$ for all i.

Theorem 3.[6] If (i, j) is a pure strategy equilibrium with payoff $(\alpha, -\beta)$, a public poll on (i,j) will increase the value of the campaign to candidate A and if there are post-poll pure equilibria strategies (r,s) and (t,u), depending on the poll outcome, then they have the property that *Prob* $A(t_{rs} = -1, t_{ij} = 1) +$ *Prob* $A(t_{tu} = -1, t_{ij} = -1) < \frac{1-\alpha}{2}$, where Prob $A(z)$ means A's subjective, ex ante probability that z will transpire.

Proof. Let $L(i, j) = \{k \mid M_{ij}^k = 1\}$

$$\bar{L}(i, j) = \{k \mid M_{ij}^k = -1\}$$

From the way probabilities are revised by the candidates, the result of the poll will be to tell the candidates that the true state of the world is in $L(i,j)$ or $\bar{L}(i,j)$. Let (r,s) be a post-poll strategy equilibrium if $t_{ij} = 1$. Then

$$\sum_{k\in L(i,j)} P_A^{k*} M_{rs}^k \geq \sum_{k\in L(i,j)} P_A^{k*} M_{\bar{r}s}^k \qquad \forall \bar{r}$$

$$\sum_{k\in L(i,j)} P_B^{k*} M_{rs}^k \leq \sum_{k\in L(i,j)} P_B^{k*} M_{r\bar{s}}^k \qquad \forall \bar{s}.$$

By Lemma 1

$$\sum_{k\in L(i,j)} P_A^{k*} M_{rs}^k = \frac{2}{1+\alpha} \sum_{k\in L(i,j)} P_A^k M_{rs}^k$$

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[6] Theorem 3 and its corollary are stated in terms of one candidate, but owing to the symmetry assured between the two candidates, they are obviously true for the other as well.

Now

$$\sum_{k\in L(i,j)\cap L(r,s)} P_A^k = \frac{1+\alpha}{2} - \delta$$

for some $\delta \geqslant 0$ since, by Lemma 1, the sum over $k \in L(i,j)$ is $\frac{1+\alpha}{2}$.

Hence

$$\sum_{k\in L(i,j)\cap \bar{L}(r,s)} P_A^k = \delta = Prob\ A(t_{rs} = -1,\ t_{ij} = +1),$$

so that

$$\sum_{k\in L(i,j)} P_A^k M_{rs}^k = \frac{1+\alpha}{2} - 2\delta.$$

Repeating the same argument, if $t_{ij} = -1$,

$$\sum_{k\in L(i,j)} P_A^k M_{tu}^k = \frac{1-\alpha}{2} - 2\epsilon$$

where

$$\epsilon = Prob\ A(t_{tu} = -1,\ t_{ij} = -1).$$

Therefore, if the poll is taken, A expects to receive

$$\frac{1+\alpha}{2} - 2\delta + \frac{1-\alpha}{2} - 2\epsilon = 1 - 2\delta - 2\epsilon.$$

If this quantity exceeds α, the expected return without a poll, then

$$\delta + \epsilon < \frac{1-\alpha}{2}.$$

Q.E.D.

Corollary 3.1. If (i,j) is a pure strategy equilibrium with payoff $(\alpha, -\beta)$ and if there are post-poll pure-strategy equilibria (r,s) and (t,u), then a poll on (m,n) will increase the value of the campaign to candidate A only if

$$Prob\ A(t_{rs} = -1,\ t_{mn} = 1) + Prob\ A(t_{tu} = -1,\ t_{mn} = -1) < \frac{1-\alpha}{2}.$$

Proof. First, note that

$$\sum_{k\in L(m,n)} P_A^k = \frac{1+\alpha}{2} + \gamma$$

for some $\gamma \in \left[-\frac{1+\alpha}{2},\ \frac{1-\alpha}{2}\right]$.

Proceeding as in Theorem 1, since

$$\sum_{k\in L(m,n)\cap L(r,s)} P_A^k = \frac{1+\alpha}{2} + \gamma - \delta,$$

for some $\delta \geqslant 0$, then

$$\sum_{k\in L(m,n)} P_A^k M_{rs}^k = \frac{1+\alpha}{2} + \gamma - 2\delta. \qquad (7)$$

Similarly,

$$\sum_{k\in \bar{L}(m,n)} P_A^k = 1 - \frac{1+\alpha}{2} + \gamma = \frac{1-\alpha}{2} - \gamma$$

so that

$$\sum_{k\in \bar{L}(m,n)\cap L(t,u)} P_A^k = \frac{1-\alpha}{2} - \gamma - \epsilon$$

and hence

$$\sum_{k\in \bar{L}(m,n)} P_A^k M_{tu}^k = \frac{1-\alpha}{2} - \gamma - 2\epsilon. \qquad (8)$$

Hence the ex ante expected payoff to A if a poll is to be taken is just as in Theorem 3, since the γ's cancel in the sum of (7) and (8).

Q.E.D.

Theorem 3 and its corollary indicate that, in a situation in which there are pre- and post-poll strategy equilibria, if candidate A expects to be made better off by a poll, certain joint probabilities must be "small." Since $\frac{1-\alpha}{2}$ is A's subjective probability that A will lose if no poll is taken, the theorem and its corollary reduce to an algorithm for computing the desirability of a poll by partitioning the state-of-the-world matrices according to which will be relevant under each poll outcome, calculating the outcome under each circumstance assuming an equilibrium strategy is adopted, and computing the sum of these outcomes weighted by the subjective, ex ante probability that the corresponding poll result will transpire. Thus, *Prob* $A(t_{rs} = -1,\ t_{mn} = 1)$ is the weighted sum of elements t_{rs} in the subset of matrices for which t_{mn} is +1, weighted by the ex ante probabilities that each matrix is the true state of the world, and *Prob* $A(t_{tu} = -1,\ t_{mn} = -1)$ is a similar calculation from the subset of matrices for which $t_{mn} = -1$.

We can give another result that does not depend on there being a post-poll pure-strategy equilibrium.

Theorem 4. If (i,j) is a pre-poll pure-strategy equilibrium with payoff $(\alpha, -\beta)$, a poll on (i,j) can increase the expected value of the campaign to both candidates only if

1. $\exists$ strategies (r,s) and (t,u) which, if adopted when $t_{ij} = 1$ or -1, respectively, have a total expected payoff exceeding $\alpha + \beta$.

2. Either

(A) $\mathit{Prob}\ A(t_{rs} = -1, t_{ij} = 1) + \mathit{Prob}\ A(t_{tu} = -1, t_{ij} = -1) < \frac{1-\alpha}{2}$ or

(B) $\mathit{Prob}\ B(t_{rs} = 1, t_{ij} = 1) + \mathit{Prob}\ B(t_{tu} = 1, t_{ij} = -1) < \frac{1-\beta}{2}$ and

3. The sum of (A) and (B) is less than $1 - \frac{\alpha+\beta}{2}$.

Proof. #1 Is obvious.

#2 Consider the two candidates jointly maximizing payoffs. The maximum joint payoff that can be obtained with a poll is

$$V^* = \underset{\overline{r},\overline{s}}{\mathrm{Max}}[P_A(t_{ij} = 1) \sum_{k \in L(i,j)} M^k_{\overline{rs}} P^{k*}_A$$

$$-P_B(t_{ij} = 1) \sum_{k \in L(i,j)} M^k_{rs} P^{k*}_B]$$

$$+\underset{\overline{t},\overline{u}}{\mathrm{Max}}[P_A(t_{ij} = -1) \sum_{k \in \overline{L}(i,j)} M^k_{tu} P^{k**}_A$$

$$-P_B(t_{ij} = -1) \sum_{k \in \overline{L}(i,j)} M^k_{tu} P^{k**}_B]$$

$$= \underset{\overline{r},\overline{s}}{\mathrm{Max}} \sum_{L(i,j)} (P^k_A - P^k_B) M^k_{rs}$$

$$+ \underset{\overline{T}'\overline{u}}{\mathrm{Max}} \sum_{\overline{L}(i,j)} (P^k_A - P^k_B) M^k_{tu}$$

Now $V^* > \alpha + \beta \Rightarrow$ payoff to A exceeds α or the payoff to B exceeds $-\beta$. Applying Theorem 3, if the expected payoff to A exceeds α, (A) must hold. The same argument holds for B, establishing #2.

#3 Let (r,s) and (t,u) maximize the expression for V^*. The payoff to A is

$$V^*_A = \sum_{k \in L(i,j)} P^k_A M^k_{rs} + \sum_{\overline{L}(i,j)} P^k_A M^k_{tu}$$

$$= 1 - 2\delta - 2\epsilon \text{ by Theorem 3.}$$

By the same argument the payoff to B is

$$V^*_B = -(1 - 2\gamma - 2\rho)$$

$$V^* = V^*_A - V^*_B > \alpha + \beta \Rightarrow$$

$$2 - 2\delta - 2\epsilon - 2\gamma - 2\rho > \alpha + \beta$$

$$\text{or } \delta + \epsilon + \gamma + \rho < 1 - \frac{\alpha+\beta}{2}.$$

The symbols on the left are simply the joint probabilities in #3 and thus the theorem is proved.

Theorem 4 supplies us with some conditions which, if violated, imply that at least one candidate will not be made better off by a public poll on the $(i,j)^{\text{th}}$ element, where (i,j) is the pre-poll pure-strategy equilibrium. We do not give theorems for more general situations here but it seems that similar results could be easily obtained. The conditions in Theorem 4 amount to saying that if the candidates "disagree enough" so that α and β are near 1, at least one will not want to have a public poll.

The reason for this phenomenon seems clear. If candidates A and B have two different prior distributions on the states of the world, the candidates think that a public poll will reveal more information to the opponent than to themselves and thereby will induce the opponent to change strategy from the present poor choice to one based on better information.

While candidates frequently express hostility to public opinion polls, most candidates for major office spend a good deal of money generating information on public preferences through privately commissioned polls. In his 1968 campaign, Richard Nixon employed the Opinion Research Corporation to conduct "opinion panels" in 14 states in addition to several "spot" surveys during the election to test public reaction to new issues. When all the costs were totaled, the Nixon polling effort in 1968 allegedly cost more than $200,000. It would be strange, indeed, to find that major candidates would spend such sums on opinion research if they felt that it would not be beneficial.

Theorem 5. A private poll cannot reduce the expected value of the campaign to the candidate who receives the information.

Proof. Let $\bar{x}$ and $\bar{y}$ be the optimal mixed strategies of A and B without a poll. Then $\bar{x}'P_A\bar{y} \geqslant x'P_A\bar{y}$ for all x such that $x \geqslant 0$ and $\Sigma x_i = 1$. The expected payoff to A is $V_A = \bar{x}'P_A\bar{y}$. Suppose a poll is to be taken on the ij^{th} element. Then the value of the election to A is

$$V_A^* = P_A(t_{ij} = 1)x^{*\prime}P_A^*\bar{y}$$
$$+ P_A(t_{ij} = -1)x^{**\prime}P_A^*\bar{y}$$

where P_A^* is the revised payoff matrix if $t_{ij} = 1$ and P_A^{**} is the revised payoff matrix if $t_{ij} = -1$. x^* and x^{**} are A's optimal strategies for the two situations. Using Lemma 1 we can write V_A^* as follows:

$$V_A^* = x^{*\prime}\Big(\sum_{k \in L(i,j)} P_A^k M^k\Big)\bar{y}$$
$$+ x^{**\prime}\Big(\sum_{k \in \bar{L}(i,j)} P_A^k M^k\Big)\bar{y}\,.$$

Note that $x^* = \bar{x}$, $x^{**} = \bar{x}$ is a feasible strategy after the poll and, if it is substituted in the above expression, $V_A^* = V_A$. Thus, $V_A^* \geqslant V_A$.

Q.E.D.

Now private information never hurts a candidate, but, since such information is costly, many candidates may not find it worthwhile to purchase it. One potential cost is that the poll will not stay private and, therefore, may reduce the value of the election to the candidate that purchased it. Not surprisingly Kingdon found that candidates for lower (and less valuable) offices relied on private polls to a lesser extent than candidates for more prestigious positions. All of the politicians he studied relied on less expensive information, even though such information was frequently known to provide biased estimates of various characteristics of the electorate. The candidates felt, nevertheless, that they could "correct" these biases and come up with reasonable guesses about the relevant features of the election. We do not investigate the circumstances in which cheap but inaccurate or biased information will be purchased by a candidate in preference to polls. The methods used here could be applied to study that situation.

5. Discussion

We have explicated a simple model of electoral competition with candidate uncertainty illustrating the strategic complexities introduced by lack of information. The model indicates that uncertainty with respect to voter preferences profoundly affects electoral strategies. Uncertainty can produce pure-strategy equilibria in which candidates adopt different platforms, rather than the identical pure and mixed strategies of traditional theory. Furthermore, under some plausible conditions, candidates will prefer not to increase the amount of information available on voter responses to campaign strategies.

Perhaps the most interesting result is that if both candidates are sure that a given platform would defeat all alternative platforms and if they "disagree enough" in their beliefs about the likely outcomes of other possible contests, they may find it in their interest to conspire not to campaign on the dominant platform. The example we provided of this phenomenon runs counter in spirit to much of the work in the theory of electoral competition. After all, the theorems of "median dominance" encourage the belief that a reasonable" position will generally prevail in the sense that if one alternative defeats all others, both candidates will adopt it as a platform. While in our model there is no necessary connection between candidate beliefs and the "true" majority-dominance relation, if both candidates agree that there is a dominant platform, that belief probably (but not necessarily) has some basis in reality. But, in just that sort of case, the candidates may find it desirable to collude against the will of the majority.

It seems quite possible that candidate uncertainty explains why in some campaigns politicians do not campaign on certain issues, even though they regard those issues as popular with the electorate. If so, several potentially testable propositions emerge that might fruitfully be examined. In the example of a prisoner's dilemma, some sort of cooperation between candidates is necessary in order to achieve an outcome more desirable than the pre-poll equilibrium. The ability of candidates to agree to coordinate their strategies should depend on (1) how difficult it is for the candidates to police defections from the collusive strategy; (2) the cost of information (a candidate has an incentive to collect information which favorably alters the strategic structure of the game); (3) the presence of public polls revealing information on public choices over which the candidates disagree; and (4) whether a "dominant issue" exists and is recognized by the candidates. We are aware of no empirical research into collusion in two-party competition but it seems that if such a phenomenon were widespread, and some researchers seem to think it is, the model introduced here may explain it.

The model presented in this article is obviously no more than a rudimentary first step towards explaining and predicting candidate behavior in uncertain situations. Our extremely simplified conceptual model of a campaign does not incorporate many important features of an actual election. We have not, for example, dealt with the problems of conveying issue positions, hypothetical or real, to the electorate or the subjects of the poll. The positions of candidates are assumed to be instantly promulgated in the electorate at no cost to the candidates. No cost is associated with switching actual positions, so that candidates are allowed to move around as much as they want in the course of a campaign. And no difficulty arises in deriving useful information from hypothetical poll questions.

Finally, while this article is partly addressed to the question of how candidates value public opinion polls, it does not provide a complete answer. One reason that some candidates do not like polls has been expressed by former California Assemblyman Walter Karabian. Karabian introduced a bill designed to limit the use of public polls in elections because he felt that a poll showing that a candidate was far behind would limit his or her ability to attract support. Our model of the campaign is too simplified to shed light on this issue.

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Explaining Presidential Popularity

How Ad Hoc Theorizing, Misplaced Emphasis, and Insufficient Care in Measuring One's Variables Refuted Common Sense and Led Conventional Wisdom Down the Path of Anomalies*

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Within the last ten years a new conventional wisdom has surfaced in political science which tells us that presidents inexorably become less popular over time. Not much else matters. Neither the economy, nor the Vietnam War, not even Watergate seems to have had much independent effect on presidential popularity once time is taken into account. Before embracing these conclusions we need to reconsider the method that produced them. I argue that previous research too willingly accepted time as an explanatory variable, enshrouding it with theoretical meaning. To preserve its explanatory power alternative, substantive variables were shortchanged in their operational definitions and measurement. In this article I reverse the emphasis. Here, time is rejected as an explanatory variable and is employed only as a diagnostic indicator of the adequacy of the equations. A variety of alternative representations of real-world forces such as the economy and war are tested and some considerably improve the time-series correlation between the environment and presidential popularity. With these substantive variables I propose a simpler, if less glamorous, theory of presidential popularity consisting of two hypotheses: first, popularity is related to real events and conditions, and second, that it responds slowly to environmental change. Popularity is then both experiential and incremental. The findings for Presidents Truman through Nixon support this common-sense view: The Korean War (measured by U.S. casualties), the Vietnam War (measured by the number of bombing missions over North Vietnam and the U.S. war dead), the economy (especially six-month changes in consumer prices), Watergate, international "rally" events, and early term surges of approval all contribute independently to short-term fluctuations in presidential popularity. Moreover, as predicted, popularity appears to be autoregressive even when represented by an instrumental variables surrogate measure to minimize serial correlation. When the equations are specified in this way, time proves to be unnecessary in order to explain trends in presidential popularity.

Q. Could you tell me, please, the basis for your optimism going into the election? I mean, why are you confident that you will not only be your party's nominee, beating Reagan, and why you'll beat the Democrats?

A. You couldn't have asked a better question, Wally. I'm happy and I'm optimistic about the nomination and the election because I am convinced the American people feel that we've been successful in foreign policy, the Middle East, Europe, etc. I'm convinced that we're well on the road to a good economic situation in 1975, so when you combine peace and prosperity, any incumbent president ought to be very happy.

Televised Press Conference with President Ford, November 3, 1975

Time may heal lovers' broken hearts, but it only opens new wounds for presidents. This is the main finding of several studies on the public prestige of presidents. Since the monthly opinion surveys on the president's performance began in the mid-1930s, every president except Eisenhower has left office less popular than when he entered. Regardless of what other variables are taken into account, the length of time since election or reelection to office uncannily remains the strongest correlate of the president's monthly trend in public approval. Why should this variable seemingly unrelated to a president's performance have such a powerful influence on popularity? This question has resulted in some highly imaginative theorizing.

John Mueller's study of presidential popularity from Truman through Johnson is the most compelling work on the subject to date.[1] Since the findings presented here dispute those of Mueller, it is important to examine his analysis in some detail. Mueller relies heavily on time as his chief explanatory variable which requires that he identify some theoretically meaningful variable which time represents. He borrows a theme from Downs and argues that the president frequently loses public support even when his policy follows the preferences of the majority of the citizenry because the minority feels more intensely about the issue.[2] If one assumes that, as a president makes decisions he alienates more citizens than he pleases, it follows that the longer the president is in office, the less popular he becomes. Following Downs, Mueller calls this the "coalition-of-minorities" effect. If the president runs for a second term, he rebuilds his electoral coalition and the downward drift begins all over again. Time, Mueller argues, reflects presidential decision making, and the secular decline of approval reflects the accumulation of presidential decisions. The plausibility of this explanation is enhanced by the one president who successfully maintained his popularity. It may not be coincidental that among recent presidents Eisenhower championed the fewest causes or policies.[3]

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*There are many people to thank for their contribution to this paper at different stages of its development. I am especially indebted to John Mueller for giving me the popularity data. For their helpful comments I wish to thank Neal Beck, Robert Holt, Frank Lerman, H. Douglas Price, Craig Swan, W. Phillips Shively, Ed Tufte, and Aaron Wildavsky.

Having suggested a theoretical underpinning for time, Mueller employs it to the fullest in explaining monthly variations in popularity. Each president's popularity is allowed to start at a different level and to decline at its own rate. This considerably improves the explanatory power of time. Next he permits different trends for each term of the two-term presidents and arrives at the equation presented in Table 1. If one defines and tailors the "coalition-of-minorities" variable in this way, any trend in the president's popularity will be measured by time which, not surprisingly, now explains nearly 80 percent of the variance in popularity. But what has been explained? All we have in the end is a set of straight lines measuring the separate trends for each term. Does the steep decline in President Johnson's popularity during his second term ($b = -8.13$) mean simply that he was making more decisions than the other presidents, or does it reflect the special effect of the Vietnam War? Since whatever trend is present in the president's public standing is obviously captured by time, there is little variance left to be explained by more substantive variables. In Johnson's case, for instance, after controlling for the downward trend, Mueller declares that "the Vietnam War had no independent impact on President Johnson's

Table 1. Mueller's Final Equation Using Time as an Explanatory Variable (Unstandardized Regression Coefficients)[a]

Intercept	72.38
Independent variables	
Rally round the flag	– 6.17
Economic slump	– 3.72
Dummy variables for terms	
Truman, second	–12.42
Eisenhower, first	– 2.41
Eisenhower, second	– 4.35
Kennedy	7.18
Johnson, first	4.02
Johnson, second	– 1.06
Coalition-of-minorities measured in years for each term	
Truman, first	– 8.92
Truman, second	– 2.82
Eisenhower, first	2.58
Eisenhower, second	0.22
Kennedy	– 4.75
Johnson, first	2.53
Johnson, second	– 8.13
Dummy variables for wars	
Korea	–18.20
Vietnam	0.01
$R^2 = .86$	

[a] From John Mueller, *War, Presidents and Public Opinion* (New York: Wiley, 1973), Table 9.3, p. 224. It should be noted that these estimates differ from those reported in Mueller's *APSR* article and lend less support for the coalition-of-minorities thesis than do the earlier figures. Rather than becoming less popular, Johnson during his first term apparently increased his public support. See Mueller, "Presidential Popularity from Truman to Johnson," *American Political Science Review*, 64 (March 1970), 28.

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[1] John Mueller, "Presidential Popularity from Truman to Johnson," *American Political Science Review*, 64 (March 1970), 18–34.

[2] Mueller, 20; *An Economic Theory of Democracy*, ed. Anthony Downs (New York: Harper and Row, 1957), pp. 55–60. Mueller also notes "minus-sum games" which are described by Aaron Wildavsky in "The Empty-head Blues: Black Rebellion and White Reaction," *The Public Interest*, 11 (Spring 1968), 3–16.

[3] In 1956 when respondents were asked in the open-ended SRC interviews what they liked and disliked about Eisenhower, the ratio of personal evaluations to political judgments among positive responses was 2.74 to 1. This compares with ratios of 1.35 and 1.33 to 1 for Johnson in 1964 and Nixon in 1968, respectively. From James A. Stimson, "Public Support for American Presidents: A Cyclical Model," mimeographed, 1974, Table 2.

popularity at all."[4] Of the various substantive short-run forces that should have a continuing effect on popularity, Mueller found only the Korean War during Truman's administration and increasing unemployment during Eisenhower's tenure correlated with the president's popularity. Hibbs reanalyzed these data, correcting for serial correlation in Mueller's estimates, and dismissed even these substantive relationships as unimportant.[5]

James Stimson has recently reexamined time, and although it remains the primary determinant of presidential popularity, its functional form is different, and a wholly different explanation for the relationship is proposed. While Mueller reports a linear relationship for time, Stimson finds that a quadratic relationship fits better for each term of every president except Kennedy.[6] For a while presidents become less popular in a near-linear fashion, but eventually the decline bottoms out and their popularity may even improve although not to its former high level. Stimson suggests that Kennedy's popularity, for which a linear trend works better, was about to level off at the time of his assassination.

To explain this relationship Stimson discards Mueller's political explanation and relies instead on a psychological model of public opinion. He identifies persons who are "less partisan, less knowledgeable, and less involved in politics" as the segment of the public which should be most "fickle" in its support of a president.[7] They initially hold a "naive admiration" for the new president because of his campaign promises and the uncritical media treatment during the honeymoon period. The "unrealistic expectations are followed by an inexorable disillusionment," and thus the president's popularity declines. Stimson views this pattern as inherent in the setting and largely beyond the president's influence, and therefore differs sharply with Mueller who views the president's decline as a direct result of decision making. Stimson's explanation for the small gain in public approval at the end of the president's term depends on whether he seeks reelection or is a lame duck. In the former instance the president can stage events and advocate enticing policies to generate campaign momentum. If the president is about to leave office, Stimson suggests that the "disillusioned cynic perhaps recovers some of his early admiration when the president's motives are no longer open to cynical interpretation."[8]

Using a quadratic equation to test his curvilinear model, Stimson is able to explain over 87 percent of the variance in popularity with time. This is done, as with Mueller, by employing the statistical fit which works best for each term of each president. If the same statistical relationship were assigned all terms, adjusting only for individual differences among presidents in their initial level of approval, the quadratic model would explain about half of the variance.[9] Thus, the special tailoring of the different trend lines to each term is important for the performance of the trend variable. Again Mueller's war variable and unemployment rate have negligible additional effect on Stimson's curvilinear popularity trends once time is taken into account.

The great difference between Mueller's and Stimson's explanations reveals the slippery nature of time. Time as a variable has no inherent theoretical meaning; time can be used to confirm any theory's validity which predicts that a president's popularity will tend to decline from its initial high level. If the "coalition-of-minorities" and "expectation-disillusionment-forgiveness" theories are to be persuasive, more compelling evidence is needed. The "coalition-of-minorities" could be more substantively measured by an index of the number and type of specific presidential policies. Another strategy might be to find different "minorities" dropping out on different occasions. Did President Kennedy lose the support of the business community, for example, during the steel price crisis in 1962? Stimson's thesis rests primarily on the behavior of an identifiable group. Finding change in approval in the predicted direction more pronounced for citizens who are poorly informed and who participate less would make his theory far more persuasive.

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[4] Mueller, 28. Italics in original.

[5] Douglas A. Hibbs, "Problems of Statistical Estimation and Causal Inference in Dynamic, Time-Series Regression Models," paper prepared for the annual meeting of the American Political Science Association, Washington, D.C., 1972.

[6] Stimson, 6, Figures 1, 2. Also see James A. Stimson and Caroline LeGette, "Public Support for American Presidents: Does Anything But Time Matter?" paper presented at the annual meeting of the American Political Science Association, Washington, D.C., 1975.

[7] Stimson, 8.

[8] Stimson, 11.

[9] Stimson's final preference to follow Mueller's strategy of letting popularity for each term change at its own pace results in a table similar to Table 1 except the estimates are quadratic, rather than linear. See Stimson's Table 4.

A third interpretation of time—and the one preferred here—is that time measures time. Measuring a president's trend in popularity is descriptive rather than explanatory. A variety of short-run forces could produce a downward (or upward) trend in approval; time is a summary measure of these forces. As such it cannot serve as a discrete measure of any one of them. As more substantive variables are employed to explain variations in approval, time can usefully serve as a diagnostic to determine whether all relevant trend-producing variables have been identified. If the trend term remains after the effects of other forces have been controlled, this may indicate that there are either additional variables present in the environment which have been left out of analysis or that the variables under study have been improperly measured.[10]

Later I shall propose and test an alternative theory of the short-term movement of the president's popularity which suggests why presidents tend to become less popular. It is valuable for the subsequent analysis to ask here why such seemingly consequential forces as the Vietnam War and the state of the economy fail to show up in Mueller's and Stimson's studies as important determinants of popularity. The presence of the trend term in their regression equations supplies part of the answer, but I suspect that the failure of these variables also reflects the way in which they are measured. My complaint with their work is not that they have studied inappropriate events and conditions; to the contrary, we shall investigate the effects of precisely the same environmental forces. Rather my disagreement concerns how these variables are measured and related to popularity.

Measuring the Independent Variables. Often what appear to be and are treated as purely technical, methodological choices in defining variables are in fact substantive decisions which implicitly reflect investigators' assumptions and beliefs about how these variables operate in the real world. For each substantive variable in their analyses there is reasonable doubt as to whether it has been adequately measured. In this section we shall scrutinize their operational definitions of the independent variables in some detail and test alternative measurements which seem to me to reflect more reasonable assumptions about the real world. Not only will this exercise provide better representations of environmental forces for the more elaborate analysis in the next section, but it should also yield a better description of the process by which experience is converted into political judgments. The environmental forces can be categorized under three headings: chronic problems, the economy, and short-term surge.

Chronic Problems. During the period under study three major chronic problems appeared to plague the incumbent president: Korea, Vietnam, and Watergate. Because the Korean and Vietnam Wars, as measured by most indices such as the number of American casualties, government expenditures, or visibility in the media, grew both in size and in political importance, they should be closely correlated with the trend term.[11] If the wars are measured as continuous variables and placed in the same equation with time, they would be so highly multicollinear, according to Mueller, that statistically meaningful estimates of their relative strengths would be impossible.[12] Mueller resolves this problem by measuring war as a qualitative, dummy variable. The variable becomes the "presence of war" and it is either switched "on" or "off." In defining war as dichotomous, Mueller tacitly decides to ignore the politically most relevant aspect of the Vietnam and Korean Wars, that is, their increasingly negative effect on the president's popularity. A dichotomous index assumes that their impact was the same from beginning to end. By 1968 Vietnam appears to have virtually destroyed Johnson's public prestige, quite unlike its effect during the summer of 1965. Although Vietnam's increasing significance for Johnson's popularity goes unmeasured by the dummy variable, it is not wholly ignored. The impact of the war is conveniently picked up by the trend term representing the "coalition-of-minorities." Given these procedures, it is not at all surprising that the war variable fails to show up as an important determinant, while "coalition-of-minorities" does.

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[10] For an interesting discussion on the value of time as a diagnostic variable, see Potluri Rao and Roger LeRoy Miller, *Applied Econometrics* (Belmont, Calif.: Wadsworth, 1971), pp. 99–104.

[11] Richard Brody and Benjamin I. Page performed a content analysis of daily news stories and found that Vietnam news had the greatest impact on both Johnson's and Nixon's popularity. "The Impact of Events on Presidential Popularity: The Johnson and Nixon Administrations," in *Perspectives on the Presidency*, ed. Aaron Wildavsky (Boston: Little, Brown, 1975), pp. 143–45.

[12] Mueller, 23–24; Mueller only considers cumulative war indices such as the number of U.S. war casualties since the war began. Using cumulative totals rather than each month's increment obviously increases the degree of multicollinearity with time. Later we shall examine noncumulative indices of the war.

Since time is being intentionally ignored as a substantive variable in the present study, we do not face Mueller's dilemma of multicollinearity and are free to look for variables which accurately measure each war's effects without being constrained by their relationship with time. For the Korean War I selected the number of U.S. casualties during the month to index the war's intensity.[13] Such a measure does not require public scrutiny of the casualty charts to be a meaningful cause of declining popularity.[14] Instead casualty figures are designed to index the overall level of military conflict. After its initial phase, as the war continues to heat up, public support should drop off. As expected, this variable correlates at –.68 with popularity which indicates that the more casualties incurred in Korea, the lower Truman's public support became.[15]

For the Vietnam War two variables are available for measuring the progress of the war. The first is the number of monthly U.S. war dead which (like the Korean casualties variable) indexes the buildup of the ground war in the mid-1960s as well as the subsequent Vietnamization program with its reduction of U.S. combat responsibility. The second variable is the number of bombing missions over North Vietnam during the month which measures another stage of the war's intensification.[16] These variables correlate with Johnson's popularity at –.78 and –.84 respectively, and together they explain 77 percent of the variance in approval. Although they are rather strongly correlated with each other at .69, each contributes independently to popularity.

Stimson retains Mueller's war dummies and apparently follows the same procedure for Watergate. After employing the best-fitting curve, he notes there are no additional "discontinuities" in the downward trend after McCord's letter on the Saturday night massacre. I tested alternatives to the dichotomous variable but each presented special problems and none performed any better than a Watergate dummy variable which correlates with Nixon's popularity at a remarkably strong –.89.[17]

The Economy. Given the role of modern government and in particular the modern presidency in monitoring and regulating the economy, it would be surprising to find that the public ignores economic conditions in evaluating the current incumbent's job performance. After all, as Katona observes,

> Whenever unfavorable developments in the economy are discussed, the American people speak of the responsibilities of the government. The remedies for unemployment, recession, or inflation are to be found, in the opinion of most people, in the government's "doing something."[18]

Mueller's analysis, however, discovered that only for Eisenhower did economic conditions affect his popularity in the hypothesized manner. Dissatisfied with the overall +.39 correlation between the current unemployment rate and popularity which is in the wrong direction, Mueller recalculated this variable as the difference between the current level of unemployment and the level when the president's term began. Still he found that the economy did not systematically contribute to popularity. Not wholly persuaded by these findings, Mueller closed his article with the suggestion that different economic indicators and different measurement techniques might work better.[19] His afterthought clearly seems correct.

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[13] The monthly war casualties are drawn from Figure 2.1 of John Mueller, *War, Presidents and Public Opinion* (New York: Wiley, 1973), p. 36.

[14] Mueller reports survey evidence showing, in fact, that the public has generally inaccurate knowledge about war casualties. Mueller, *War*, pp. 35–62.

[15] The correlation is for the logarithmic transformation (base 10) of the casualty index. This works marginally better than the untransformed variable suggesting that comparatively small numbers of U.S. casualties during the early stage of the Korean War had a disproportionate negative impact on President Truman's popularity. Because the job performance question was asked intermittently between 1945 and 1949 and because data for some of the other independent variables are missing during this period the analysis will be restricted to Truman's second term commencing in January, 1949.

[16] Both of these indices are drawn from Jeffrey Milstein, *Dynamics of the Vietnam War* (Columbus: Ohio State University Press, 1974), Appendix.

[17] After Watergate became recognized as a serious threat to President Nixon, the Gallup Poll began asking respondents their opinion about Nixon's involvement. Did he participate in the burglary and/or the cover up, just know about it, or "did he have no known knowledge about the bugging and spoke up as soon as he learned about it?" Over the year and a half period during which the question was asked the percent answering "no knowledge" shrunk from 23 percent to 11 percent. *The Gallup Opinion Index*, September, 1974, p. 16. The percentage believing the president was innocent correlated at .61 with Nixon's popularity.

[18] George Katona, *The Mass Consumption Society* (New York: McGraw-Hill, 1964), p. 143.

A variable of such theoretical importance deserves more careful consideration. Three monthly economic indices have been tested. In addition to the unemployment rate, I examined the monthly consumer price index and total personal income. This last variable is similar to the one successfully employed by Kramer, Tufte, and Bloom and Price in their separate research into the economic determinants of congressional voting.[20] Largely duplicating the effects of unemployment and inflation, total personal income performs weakly under statistical controls and therefore will be dropped from subsequent analysis.

The underlying variable which these "hard" economic indicators represent is the public's perception of the economy. Mueller persuasively argues that public perceptions of how the economy is changing is more important in evaluating the president's performance than is the absolute level of the unemployment rate or consumer price index. A six-percent unemployment rate may benefit an incumbent president if it had been hovering around a much higher figure or tarnish his prestige if it represents a significant rise. If we accept that *change* in these monthly indices more closely parallels the public's evaluative process, the central issue becomes one of discovering the appropriate time intervals for measuring this change. Mueller prefers to use the unemployment rate at the beginning of the president's term as the benchmark for public evaluations. While this base has the attractive quality of being the first point of the president's responsibility, such a comparison requires that the public be able to recall what the unemployment (or inflation) rate was as long as four years ago. Perhaps his failure to find meaningful relationships between unemployment and popularity reflects this feature of the operational definition. The public more likely evaluates the relative change in the economy over a much briefer interval.

To find a more proximate benchmark for economic evaluations, I have tested several different moving average comparisons.[21] Unemployment and consumer prices generally correlated better with popularity when change was measured over a six-month period than for other time spans. To test their validity on an external criterion I correlated these indices with the public's perceptions of the economy. The national Harris survey taken during a three-year period from 1971 until early 1974 asked respondents if they thought the U.S. was in a recession and whether prices were rising more or less rapidly than a year ago. In Table 2 we see that six-month changes in unemployment and consumer prices are closely correlated with perceptions of the current economic conditions.

Table 2. The Correlation of "Hard" Economic Indicators with the Public's Perception of the Economy

	Percent Change in Index Over 6 Months	
	Consumer Prices	Unemployment Rate
Prices rising more rapidly[a]	.84	.05
In recession[b] (N=16)	.07	.64

[a] "Do you feel that prices of most things you buy are rising more rapidly than a year ago, about as rapidly as they were then, less rapidly than a year ago, or are they going down?" Scored percent saying "more rapidly."

[b] "Do you feel the country is in a recession today or not?"

Responses to these questions were obtained from monthly publications of *Current Opinion* in 1973 and January, 1974.

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[19] Mueller, *War*, p. 238.

[20] Their studies used real income which is not available monthly; see Gerald Kramer, "Short-term Fluctuations in U.S. Voting Behavior, 1896–1964," *American Political Science Review*, 65 (March 1971), 131–43; Edward Tufte, "Determinants of Outcomes of Mid-term Congressional Elections," *American Political Science Review*, 69 (December 1975), 812–26; Howard S. Bloom and H. Douglas Price, "Voter Response to Short-run Economic Conditions: The Asymmetric Effect of Prosperity and Recession," *American Political Science Review*, 69 (December 1975), 1240–54.

[21] Three different averages were tested for unemployment and consumer prices: 2, 3, 4; 5, 6, 7; and 11, 12, 13 previous month averages. The six-month moving average was found to work best. This agrees with Henry C. Kenski's report after testing a large number of economic measures. "The Impact of Economic Conditions on Presidential Popularity," *Journal of Politics*, 39 (August 1977), 764–73.

When unemployment is measured by percent change over the last six months rather than simply its current rate, it generally behaves better in its zero-order relationship with popularity. The improvement is not uniform, however. In Table 3 the signs of the transformed rate run in the wrong direction for the Truman and Nixon ratings. Under adequate statistical controls, the strong positive correlation of change in unemployment with Truman's popularity is eliminated.[22] Even for Kennedy the bivariate correlations fail to reveal the full impact of this transformation on the relationship. In the regression equations to be examined later, percent change in unemployment emerges as an important correlate of Kennedy's popularity. Transforming unemployment has the most dramatic effect on Johnson's popularity where the correlation shifts from strongly positive to negative.

The consumer price index is also measured in percent change since each month's index will be (almost by definition) some increment larger than the one for the previous month. Change in the consumer prices appears in the table to be a primary candidate for explaining each president's popularity.[23] During the 1950s and early 1960s the U.S. enjoyed an annual inflation rate of near 2 percent. As inflation has grown at a faster pace over the last ten years, so too has its apparent influence on the public prestige of presidents according to the large correlations for Johnson and Nixon.

Rather than being an unimportant contributor to popularity as Mueller and Stimson found, the state of the economy appears to be a hardy variable, capable of explaining a significant proportion of the variance in popularity. Finding that a six-month time perspective of the economy is most closely correlated with popularity has a certain practical significance as well. For example, the economy's impact on a president's reelection chances in November probably reflects the public's comparison of the economy at the election with conditions during the preceding spring. No wonder President Ford's economic advisers were complaining in the spring of 1976 that the Bureau of Labor Statistics' seasonal adjustments inappropriately made unemployment look as though it would worsen during the summer.[24]

Short-Term Surge. Students of public opinion have noticed over the years that during periods of international crisis the public tends to rally behind the president. Nelson Polsby summarizes the evidence: "Invariably, the popular response to a president during international crisis is favorable, regardless of the wisdom of the policies he pursues."[25] As Kennedy noted after the Bay of Pigs incident, "The worse I do, the more popular I get." If the economy affects popularity, it will probably do so gradually over

Table 3. Zero-order Correlations of Unemployment and Consumer Prices with Presidential Popularity

	Unemployment Rate	Percent Change Over 6 Months: Unemployment	Percent Change Over 6 Months: Consumer Prices
Truman*	.45	.63	–.64
Eisenhower	–.64	–.26	–.22
Kennedy	.48	–.36	–.51
Johnson	.88	–.19	–.83
Nixon	–.28	.11	–.82

*Because of missing observations, all relationships reported here and in subsequent tables for Truman are for his second term only.

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[22] This occurs when controlling for Korean War casualties, Early Term effect, and Rally, all of which are examined below. In the final regression equations in Table 5, change in unemployment is omitted for Truman since it fails to approach statistical significance.

[23] Kenski also finds inflation to be a probable cause of popularity. See "Impact of Economic Conditions," 25–27.

[24] A Bureau of Labor Statistics official recalled, "The Council of Economic Advisors felt there was too much good news earlier in the year and too much deterioration later in the year." Edward Cowan, "Jobless Rate: Elusive Statistic," *New York Times*, January 13, 1978, p. A-11.

[25] Nelson Polsby, *Congress and the Presidency* (Englewood Cliffs, N.J.: Prentice-Hall, 1964), p. 25. In the second edition he lists the "before-after" effects of international crises on the president's popularity, p. 44, Table 1. See also Mueller, "Presidential Popularity," 21.

an extended period. Rally events, on the other hand, should produce immediate bursts of support but of short duration. Explaining the significant bumps on the popularity trend should not only improve the overall explanatory power of our equations but also enhance the influence of those variables which are durable contributors to popularity.

Mueller developed a useful list of rally events which were international in origin, directly involved the United States, and were "specific, dramatic, and sharply focused."[26] He also included presidential inaugurations among the rally events for "analytic convenience." As we shall see the inclusion of what he recognized to be "arbitrarily designated" rally points has a dramatic effect on the explanatory power of this variable.[27] In order to ascertain the contemporary public salience of these events, I performed a systematic content analysis of the front pages of three metropolitan newspapers for the week in which each event received maximum coverage. Almost all of the events made the front page for at least five consecutive days, which would seem to be sufficient exposure to guarantee widespread public awareness.[28]

Mueller estimated the effects of these events for each month's approval rating as the length of time since the last rally point. By this method a high score for a given month means that there has been no recent event. While every observation after the first rally event is given some value, this still leaves the problem of what values to assign those periods before the first event occurs. Mueller found it necessary to begin *each* term as a new rally point, which worked to inflate the correlation of this variable with popularity since at the beginning of his term the president enjoys widespread public approval.[29]

In order to avoid confusing the effects of international events from the early term "halo," Mueller's coding procedure for this variable has been altered in two ways. First, we shall assume that rally events have a decreasing impact on popularity over a five-month period only. After that period their effect is assumed to be nil and the rally variable is scored zero.[30] Second, the rally point designated at the beginning of each term has been dropped and only international events are used. The new rally variable takes on a value of zero until the first event occurs. Certainly something is happening to popularity at the outset of the president's term which needs to be tapped, but to include it as part of the rally variable only prevents us from appreciating the effects of international affairs on the president's popularity.

In Table 4 we can examine the effects of each of these reforms of the rally variable on its correlation with popularity. Except for some reduction in the correlation for Presidents Kennedy and Nixon, limiting rally events to five months (column 2) has little substantive impact on the relationships. (Note that the change from a negative to positive sign from column 1 to 2 in Table 4 is wholly an artifact of the scoring procedure.) When the inauguration rally points are eliminated (see column 3), the relationships are radically altered. For

Table 4. **Zero-order Correlations of Rally and Early Term Variables with Presidential Popularity**

	Mueller's Rally	Rally Rescored	New Rally	Early Term
Truman	–.31	.34	–.01	.67
Eisenhower	–.03	.10	–.01	.36
Kennedy	–.85	.68	.60	.20
Johnson	–.09	.01	–.31	.59
Nixon	–.78	.52	.41	.38

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[26] Mueller, "Presidential Popularity," 21.

[27] Mueller, "Presidential Popularity," 22.

[28] The three newspapers examined were the Atlanta *Constitution*, the Chicago *Tribune*, and the San Francisco *Chronicle*.

[29] Mueller, "Presidential Popularity," 22. Technically, it was not necessary for Mueller to begin each term as a rally point. For the second term the count since the last rally point could have continued uninterrupted by the reelection of the president. Mueller's rally variable correlates at –.11 with popularity.

[30] Such a procedure is admittedly arbitrary. Several other scoring procedures were tested but without important changes in the relationships. It is interesting that a linear decline in the effect of an event over five months works as well as the learning curve.

Johnson, and to a lesser degree for Truman and Eisenhower, the correlations contradict the hypothesis. These negative correlations may mean that the rally events had a net negative impact on these presidents' popularity. Or it may simply be that they occurred more often during periods of relatively low popularity. Only for Kennedy and Nixon do these zero-order correlations suggest the benign effects of international events on the president's popularity. Although a final judgment must await examination of the relationships under appropriate controls, it may well be that not all "specific, dramatic, and sharply focused" international developments benefit the incumbent president.

In order to measure the early administration surge in popularity, we have created a separate Early Term variable which begins with a value of 6 and declines one unit per month, becoming 0 as the president begins his second six months in office. In Table 4 (column 4) this modestly conceived variable accounts for a prominent share of the variance in popularity.[31]

Presidents begin office as ambiguous stimulus objects. Experimental and field research in social psychology strongly suggests that ambiguity fosters positivity.[32] As the president becomes better known, citizens form judgments on the basis of more specific and contextual information. This argument fails, however, to explain why the same pattern reemerges for the second term as well. The public presumably now has ample information on which to base opinions.

Another explanation which comes via a different body of experimental research in social psychology is that the surge of approval at the outset of the term represents a "bandwagon" or, as some have preferred, a "fait accompli" effect. In a well-known study of student responses to the 1952 election, I. H. Paul concluded that "people's impressions of the personalities of presidential candidates change following the election and this change involves an increase in the attractiveness of the winner."[33] Other studies in subsequent elections employing different test instruments have corroborated Paul's early finding. Even when the winner is an incumbent, immediately following his reelection his earlier detractors become more approving.[34]

Also, for Presidents Truman and Johnson for whom these correlations are very large, there appears to have been the special effect of succeeding a president who died in office. A literature has emerged since the Kennedy assassination which agrees in finding a highly emotional public response following his death.[35] It is not unlikely that these feelings spill over into initial evaluations of the new president. Whatever the source, clearly the president begins his term with widespread public admiration.

Problems With the New Conventional Wisdom. Current research on presidential popularity is marred by several analytic shortcomings. First, there is the problem of ad hoc theorizing. Time is found to correlate closely with popularity and with some tailoring can be made to produce extraordinarily large R^2's. In order to give these relationships substantive import, theories surfaced. But on closer inspection we find that Mueller's "coalition-of-minorities" and Stimson's "attraction-repulsion" theories do not explain much about the behavior of presidential popularity beyond the information used to construct them. Neither theory explains why popularity declines (or improves) at different rates for different presidents and even for different terms.

The second problem is misplaced emphasis.

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[31] The index is crude in that it picks up whatever is occurring to the president's popularity during the first six months. It ignores the possibility that some "early term" periods will last longer than others. For example, the surge in support at the outset of the second term may be of shorter duration than for the first. The coefficient for Early Term will represent an "average" effect for these two periods, perhaps being too weak for the first and too strong for the second.

[32] David O. Sears and Richard E. Whitney, *Political Persuasion* (Morristown, N.J.: General Learning, 1973), pp. 12–17.

[33] I. H. Paul, "Impressions of Personality, Authoritarianism, and the *Fait Accompli* Effect," *Journal of Abnormal and Social Psychology,* 53 (November 1956), 341. For a summary of the prominent findings in this area of research see Sears and Whitney, *Political Persuasion,* pp. 12–17.

[34] Lynn R. Anderson and Alan R. Bass, "Some Effects of Victory or Defeat Upon Perception of Political Candidates," *Journal of Social Psychology,* 73 (October 1967), 227–40; Bertram H. Raven and Philip S. Gallo, "The Effects of Nominating Conventions, Elections, and Reference Group Identification upon the Perception of Political Figures," *Human Relations,* 18 (August 1965), 217–29.

[35] Martha Wolfenstein and Gilbert Kliman, *Children and the Death of a President* (Garden City, N.J.: Doubleday, 1965), and Harold Orlansky, "Reactions to the Death of President Roosevelt," *Journal of Social Psychology,* 26 (November 1947), 225–66.

When time is emphasized as the primary explanatory variable, other substantive variables are crudely defined to avoid the problem of multicollinearity. It is especially inappropriate to conclude from the failure of these intentionally crude operational definitions that the real-world forces are unimportant. Such a procedure is about the only way the Vietnam War, for example, could fail to be a primary determinant of Johnson's prestige.

The third problem is inadequate measurement. Although including time in the equations will reduce the strength of the estimates for such variables as the economy and war, with proper measurement significant relationships may still have resulted. We have seen that when reasonably measured, the real world does in fact shape public evaluations of the country's most prominent political leader.

Back to the Old Conventional Wisdom

> The President helps people make sense of politics. Congress is a tangle of committees, the bureaucracy is a maze of agencies. The President is one man trying to do a job—a picture much more understandable to the mass of people who find themselves in the same boat. Furthermore, he is the top man. He ought to know what is going on and set it right. So when the economy goes sour, or war drags on, or domestic violence erupts, the President is available to take the blame. Then when things go right, it seems the President must have had a hand in it.[36]

The traditional view of close observers of the presidency described in the above passage is supported by the simple bivariate relationships reported in the preceding section. Having rejected Mueller's and Stimson's novel theories of popularity for conceptual and methodological reasons, we return to a more realistic, if somewhat less glamorous, view of presidential popularity. The model we shall examine in this article contains two propositions (or hypotheses) which to a varying degree are founded on intuitive or common-sense ideas about how the president's popularity behaves.

Proposition 1. *Short-term fluctuations in the president's popularity will be largely determined by contemporary events and conditions.* Unlike the "coalition-of-minorities" theory, it is not so much presidential decision making to which the public responds as it is the results of those decisions. Outcomes in the form of current events and conditions are more visible, easier to grasp, and can be more readily appreciated by the citizen in their impact on personal welfare and values. By examining outcomes such as increased war casualties or the price of food, the citizen obtains better information at a lower cost. Increasingly expensive gasoline can be more easily discerned and evaluated than can a presidential decision to veto a price ceiling on the production of "old" domestic crude oil. The evidence presented above suggests that changing economic conditions, war, international crises, and scandal are the types of outcomes which the public examines in forming evaluations of the president's performance.

Although judging the president according to one's satisfaction with current conditions may reduce information costs, this judgment will sometimes be misdirected. The president frequently does not have sufficient control over events to produce favorable outcomes to problems for which he is likely to be held responsible. Even so, such a criterion creates an important functional benefit for the political system. The president's realization of his assigned responsibility should prompt the system's most powerful actor to engage in problem solving. The president needs to know that there is little advantage in ignoring problems even when they originate elsewhere.

Proposition 2. *The president's current popularity reflects the level of approval during the preceding month.* This proposition suggests that the president's popularity will respond sluggishly to environmental forces. During the brief intervals between observations, many citizens will maintain their assessment of the president's performance regardless of intervening events. The built-in inertia of popularity is revealed by the fact that the best information available for predicting an individual's future evaluation of the president is his or her current evaluation. This continuity will be reflected in marginal shifts in the aggregate opinion as well. Perhaps this explains why Stimson fails to find that any of the Watergate disclosures or incidents had a noticeable impact on Nixon's popularity. Where one might expect to discover a sudden bump or dip in the trend reflecting a new disclosure, such as indictment or conviction of Watergate participants, or perhaps a presidential denial of alleged involvement, Stimson finds only a continuation of a smooth and sharp downward trend.[37]

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[36] James D. Barber, *Presidential Character* (Englewood Cliffs, N.J.: Prentice-Hall, 1972), p. 5.

[37] Stimson, 30–33.

In statistical parlance such a relationship is described as autoregressive, and it can be measured by including the previous month's popularity in the regression equations as one of the independent variables among the environmental forces. If popularity does behave autoregressively, whatever forces contributed to change in popularity in the recent past will continue to be felt through the previous month's popularity. As diagrammed below, previous environmental conditions, x at t_2 and to a lesser degree at t_1, will influence current popularity, P_{t3}, through the previous month's score, P_{t2}. In this way, the lag of popularity acquires the variance produced by those exogenous (environmental) variables which influenced popularity in the past.[38]

$$\begin{tikzcd}
X_{t1} \arrow[r] \arrow[d] & X_{t2} \arrow[r] \arrow[d] & X_{t3} \arrow[d] \\
P_{t1} \arrow[r] & P_{t2} \arrow[r] & P_{t3}
\end{tikzcd}$$

where X = environmental forces
P = presidential popularity at 3 points in time

Figure 1. Autoregressive Model of Presidential Popularity

Unfortunately, there are a couple of special problems in estimating the statistical relationships for an autoregressive model. For one, the normal practice of evaluating the relative merits of alternative estimation equations by comparing their overall explanatory power (R^2) can be misleading since the lag popularity variable will pick up the effects of any variables inappropriately left out of the analysis. We can safely assume that the greater the number and importance of the environmental forces omitted from the regression equations, the greater the strength of the lag term. The general dictum to specify one's equations fully is especially important when examining autoregressive models.

A related problem common to all time-series analysis but more severe for autoregressive models is serial correlation of the error term. This can introduce several biases into the regression estimates. The standard errors for all the independent variables will be smaller than normal, which means that the reliability of the estimates will generally be lower than indicated. Also, the regression coefficients for the exogenous variables may be depressed while the value of the lagged popularity variable will be inflated since it taps much of the autocorrelation. A "shock" to one month's popularity score, for example, may continue to be felt over a series of subsequent observations, and although the cause of disturbance will remain unnoticed, the correlation of the lag and current popularity will be strengthened. Or variables in the analysis may be misspecified (such as the crudely measured Early Term, Rally, and Watergate variables), resulting in systematic disturbances of popularity remaining unmeasured. Either way serial correlation of the error term is a likely result.

The Durbin-Watson statistic which is the conventional technique for detecting serial correlation is inappropriate for autoregressive equations, and weaker tests such as the run of signs produced indeterminate results in a preliminary analysis of the relationships. Given the brief interval between observations and the likely susceptibility of our dependent variable to a large number of environmental forces, we shall take a conservative approach and assume that serial correlation is present. In order to correct for this I have followed a technique proposed by Malinvaud of creating an "instrumental variable" representation for the lagged popularity term.[39] Essentially it requires that we substitute the *estimated* lag popularity score based on the exogenous variables in the equation for the *actual* lag score. If we are to obtain reasonable estimates, it now becomes doubly important that the model be fully specified. (For a discussion of the rationale procedures, and limitations of this approach and two alternative estimation procedures see the Appendix.)

The Findings

After an examination of a number of regression equations for each president, the specifications presented in Table 5 were found to

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[38] Brody and Page view presidential popularity as autoregressive as well and incorporate and lagged variable into their analysis. In fact it is this variable and not their news indices which appears to explain most of the variance in popularity. Their rationale for treating popularity as autoregressive is somewhat different from the one employed here. To them it represents a base which in the absence of intervening events dictates the next month's approval level. Brody and Page do not consider the issue of serial correlation; "The Impact of Events. . ."

[39] E. Malinvaud, *Statistical Methods of Econometrics* (London: North-Holland, 1970).

Table 5. Testing an Autoregressive Model of Presidential Popularity
(Unstandardized [b] and Standardized [beta] Regression Coefficients; D.V. = Percent Approving the President)

	% Change Unemployment[a]	% Change Prices[a]	Rally	Early Term	Watergate (Dummy)	Killed or Casualties	N.V. Bombing	Lag of Popularity[b]	Intercept	R^2
Truman										
b	–	–99.6	1.45	1.00	–	–1.05	–	.53	17.8	.83
beta	–	– .22	.26	.10	–	– .17	–	.58		
		(65.6)[c]	(.72)	(2.3)		(1.53)		(.30)		
Eisenhower										
b	–.65	–94.5	.27	.44	–	–	–	.88	8.0	.30
beta	–.03	– .10	.07	.08	–	–	–	.44		
	(2.9)[c]	(109.5)	(.39)	(.91)				(.38)		
Kennedy										
b	–19.1	–	1.11	1.92	–	–	–	.54	28.9	.67
beta	– .18	–	.35	.35	–	–	–	.48		
	(13.8)[c]		(.44)	(.63)				(.18)		
Johnson										
b	–28.8	–427.4	.42	.35	–	–.004	–.003	.54	32.1	.93
beta	– .11	– .24	.06	.04	–	–.15	–.09	.53		
	(17.7)[c]	(207.1)	(.33)	(.50)		(.002)	(.0002)	(.17)		
Nixon										
b	–	–199.0	1.06	1.3	–16.1	–	–	.11	52.2	.86
beta	–	– .24	.14	.14	– .53	–	–	.10		
		(82.1)[c]	(.45)	(.54)	(3.6)			(.11)		

[a] Scored as decimals rather than percents.

[b] This is the "instrumental variable" representation of popularity described in the Appendix.

[c] The numbers in parenthesis are the standard error of the slope. Regression coefficients which are twice the standard error are statistically significant at .05.

perform best in producing theoretically meaningful relationships and explaining overall variance in popularity. In a preliminary analysis, each of the variables in the equations were significantly related to popularity prior to the inclusion of the lagged popularity term. On the whole the relationships given in Table 5 support the old conventional wisdom. Environmental forces do contribute to short-term fluctuations in the president's popularity, and the trends generally display a large autoregressive component. Only for Eisenhower does the equation fail to explain a substantial share of fluctuation in his public support. What appears to have happened is that Eisenhower's popularity during his eight years in office was so stable that a greater share of the month-to-month fluctuation in his popularity reflects random sampling error of the surveys. There is simply little systematic variance available to be explained. After evaluating the significance of each of the exogenous variables on presidential popularity, we shall test the adequacy of these equations in explaining the downward trends measured by other scholars, and speculate on the implication of these relationships on presidential politics.

The Economy. The zero-order correlations in Table 3 gave little cause for optimism that change in unemployment would be a major determinant of the president's popularity, and these suspicions are confirmed with the multiple regression estimates. Only for Kennedy and Johnson does unemployment seem to have been a contributing factor, and even here the coefficients are neither large nor significant. A one-percent change in the unemployment rate over a six-month period altered Johnson's popularity by only .3 of one percent. Part of the problem of unemployment is that it is negatively correlated with war. As the Korean and Vietnam wars intensified, the unemployment rate declined along with the president's popularity.[40] Change in consumer prices, which if anything increases during war, is more closely related to popularity. For four or five presidents, the coefficients are sizable with a one-percentage-point change in prices over six months affecting popularity from .9 of a percentage point for Eisenhower to 4.3 points for Johnson. The *betas* demonstrate that inflation explains a sizeable share of variance in popularity.

Although interpretation must be tentative since the relationships generally fail to satisfy conventional significance requirements, the pattern is consistent with our image of these historical periods. With the Korean war present throughout Truman's second term, it seems unlikely that unemployment would constitute a significant political issue. Similarly for Eisenhower, with the exception of the 1957–58 recession, unemployment and inflation do not appear to have been highly salient public concerns. The weak relationships here may also reflect Eisenhower's apolitical image. He may have simply been held less responsible than other presidents for conditions in the political environment. At the outset of the Kennedy administration, rising unemployment, not the modest rate of inflation, was the nation's primary concern. The 1961 tax cut was designed to stimulate consumer demand and production, not reduce it. Conversely, during Nixon's tenure, at least into the spring of 1974, the primary economic issue was rampant inflation dramatically manifested initially with the meat "boycott" in 1973 and later with the escalation in fuel prices. Not surprisingly, consumer prices emerge as the important economic variable. *While the relationships presented in Table 5 fail to establish the Iron Law of the Economy, they do inform us that changing economic conditions can have an important effect on the president's public standing.*

Rally and Early Term Effects. The new Rally variable is consistently related to approval in the predicted direction for all of the presidents. The strength of the relationships vary greatly, depending on the general overall plateau of popularity upon which they are operating and the political significance of the events themselves. Some events are simply more important than others. The effects of international events on Truman's, Kennedy's and Nixon's popularity are particularly strong. During the first month of a rally event, each president's popularity sharply improved by about 5 to 7 percentage points on the average. Perhaps Truman's surges in large part reflect his low popularity when crises occurred. There were simply more disapprovers available to rally. Much of the strong public support Kennedy enjoyed during the first two years in office appears to have been largely a product of a series of dramatic international crises. The relationship between the Rally variable and Johnson's popularity is positive although weak, whereas the zero-order correlation presented earlier was sizable but in the wrong direction (–.36). In the multivariate analysis the Vietnam War variables–U.S. Killed and N.V. Bombing–are explaining the sharp descent of Johnson's popularity leaving Rally to pick up the temporary, minor surges of approval which occurred whenever he initiated a departure from existing war policy.[41]

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[40] Unfortunately for our understanding of the Truman and Johnson trends, there are too few observations prior to the start of the wars to obtain an independent estimate of unemployment.

The Early Term variable is related to each president's popularity in the correct direction but in three instances it is not statistically significant. Controlling for the other forces present in the political environment, we find the president gifted with extra support at the beginning of his term. The serious anomaly among these relationships is the weak Early Term value for Johnson. Entering office on the death of Kennedy and being elected by a huge majority should have provided an ideal setting for a large early-term surge. An analysis of the residuals suggests that the first early term period had a genuine surge effect, but the second did not since Johnson enjoyed strong public approval going into the election. The weak estimate represents the "average" effect of the two early term periods.[42]

Chronic Problems. The Watergate dummy variable confirms what we already know. Watergate hurt Nixon; by this conservative estimate the plateau of his public support dropped 16 percentage points. And as expected, the *betas* show that Watergate was by far the most important determinant of Nixon's loss of public esteem. The Korean War (a logarithmic transformation of U.S. casualties) had a continuing negative influence upon Truman's popularity. Although the relationships are not statistically significant and any interpretation must therefore be tentative, it appears from the *betas* that increasing prices were a slightly more important cause of Truman's low public support than the monthly war casualties. The two Vietnam War indices demonstrate the importance of properly measuring one's variables. Under extreme controls (including the companion war index) each variable independently exhibits a strong negative effect on Johnson's popularity. The unstandardized regression coefficients in Table 5 may appear deceivingly small, but one must remember the unfortunate size of indices by which they are multiplied. The average monthly number of U.S. war dead was 478 and the average monthly bombings of North Vietnam was 4,692, and these figures include the year and a half of Johnson's administration before the war began, during which they had scored zero. Multiplying the coefficients by these averages gives a 3-percentage-point decline in Johnson's approval level for an "average" month. Obviously, this finding strongly disputes Mueller's conclusion that "the Vietnam War had no independent impact on President Johnson's popularity at all."[43] Even with extensive statistical controls, the war variables continue to be among the primary contributors to Johnson's downfall in the polls.

The findings differ substantially from the previously published research on presidential popularity. *Contrary to existing reports events and conditions which intuitively seem to be important determinants of a president's popularity are in fact the primary explanatory variables.* The traditional wisdom expressed in Proposition 1 has been affirmed.

The Estimated Previous Month Popularity. The second proposition posits that popularity will shift marginally from one month's observation to the next with the impact of current conditions on popularity mediated by the lingering effects of past conditions. The coefficient for the instrumental variable representation of presidential popularity confirms this prediction.

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[41] One study of public opinion toward Vietnam War policies has shown that regardless of whether the respondent identified himself as a "hawk" or "dove" any change in policy would be greeted with at least temporary approval. See Sidney Verba et al., "Public Opinion and the War in Vietnam," *American Political Science Review*, 61 (June 1967), 317–33.

[42] The arbitrary measurement of the Early Term and Rally probably reduces the explanatory power of the model and introduces serial correlation. This scoring procedure works as well as any other, but the problem lies in applying the same attrition model to qualitatively different types of events. All international events will obviously not have the same impact on public opinion. Moreover, attrition over subsequent months will vary according to the event's intensity and duration. Some, therefore, should last only a few months while others should perhaps be extended over a longer period. Unfortunately, we currently have little independent basis for weighting events to tap their differential significance. An example of its potential effect on correlations of the residual term is the Nixon Vietnamization speech in November 1969. Even with a high rally score assigned to this event, Nixon's actual popularity was some 12 percentage points greater than estimated. For the next several months the estimated popularity was less than the actual, tending to produce a correlation of the residuals.

[43] Mueller, "Presidential Popularity," 34.

For each president except Nixon, the *estimated* previous month's popularity based on the exogenous variables in the equation is the strongest determinant of the president's current status. The failure of the lag term to perform as well during the Nixon administration suggests the direct hold of Watergate and rising prices on his public support.[44]

What Happened to Time? Earlier, time was offered as a useful diagnostic for detecting whether any trend-producing variables have been inadvertently left out of the analysis. Mueller with his "coalition-of-minorities" and Stimson with his mass psychological shift explanation enshroud with theoretical significance the downward drift in approval. The appropriate question to ask here is whether the traditional view of popularity I have proposed satisfactorily explains why presidents become less popular. Does time matter? To test this I have regressed the residuals (the differences in the actual and estimated popularity) on time, and these relationships are reported in Table 6. Only for the Nixon administration does a statistically significant downward trend persist.[45] Very likely, the relationship here results from the cumulative effects of Watergate which remain untapped by the Watergate dummy variable. The failure of time suggests that the relationship found by Mueller and Stimson may have largely been a product of the autoregressive nature of popularity. It also suggests that no substantive trend-producing variables have been mistakenly excluded from the equations. Using our model of presidential popularity, time is of little substantive value. *Mutatis mutandis, tempus fugit!*

Summary and Conclusion

In this article I have developed and tested a model of presidential popularity. Although some of the relationships have large standard errors or differ from the expected, the bulk of the evidence contained in the equations in Table 5 clearly favors the two propositions describing the model. The president does not simply become less popular. Instead, fluctuations in his prestige can be located in observable events and conditions present in the political environment. Peace and prosperity, just as President Ford noted at the opening of this article, are the foundations of a popular president. Moreover, popularity responds sluggishly to environmental forces, which is to be expected, given that we are examining brief changes of an index representing the opinions of a large, heterogeneous citizenry. Like budgeting, presidential popularity is experiential and incremental.

These findings agree with our image of the modern presidency. The increased function and size of the national government, the gravity of international affairs, and the president's access to the public guarantee his prominence in national policy making. While most observers focus on the president's expanded authority and capacity for leadership, the evidence presented here reveals that with authority comes responsibility. The Full Employment Act of

Table 6. Testing Time Against the Residuals of the Regression Estimates

	Time as Independent Variable		
	b(slope)	Standard Error*	Percent of Variance in Residuals Explained
Truman	–.006	.007	2
Eisenhower	.004	.005	1
Kennedy	–.011	.008	6
Johnson	–.001	.003	.1
Nixon	–.177	.005	18

*Estimates which are twice their standard error are significant at .05.

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[44] The fact that each of the coefficients rests between 0 and +1 informs us that popularity approaches a stable equilibrium. This means that in a constant political environment popularity will gradually move to an equilibrium level probably reflecting in part the partisan division of the electorate. For an example of the interpretation of equilibrium behavior from autoregressive terms, see Duff Spafford, "A Note on the 'Equilibrium' Division of the Vote," *American Political Science Review,* 65 (March 1971), 180–83.

[45] It is curious that when time is included in the equation for the Nixon administration the coefficients for the other variables–including Watergate–improve: Pop = 1.5(Rally) – 284(Prices) – 19(Watergate) – .6(Early Term) – .04(lag of popularity) – .03(Time) + 68.6. $R^2 = .89$.

1946 may have acknowledged overall governmental responsibility for managing the economy, but public opinion makes the president personally responsible. The buck does indeed stop at the White House. These data also reveal that for brief but important periods the public suspends critical judgment and gives the president its support. At the outset of the president's term and during international crises, his prestige surges offering him in the first instance an opportunity to establish successful relationships with other elites and in the second the necessary flexibility to respond to international exigencies.

Sensing that the public is holding him responsible, the president is encouraged to engage in active problem solving. His decisions on policy do not affect his popularity so much as their results. Counter to the "coalition of minorities" explanation, the theory presented here argues that presidential inaction on occasion may be more costly in public support than action. No president wishes to have to deal with an energy crisis, but since it will not simply go away, he must deal with it. He knows that the consequences of public disapproval can be severe. A president's effectiveness in bargaining with other elites will be impaired, his capacity to lead public opinion will be reduced, and his party's fortunes at the polls will suffer.[46] The fact that these monthly job performance evaluations are rooted in real events and conditions—and not just in time—should in the long run enhance the quality of presidential performance.

Appendix. Statistical Procedures for Correcting for Serial Correlation

Instrumental variables is a procedure similar to two-stage least squares in which the "systematic" aspect of the variance in the lagged popularity variable is separated from the autocorrelation component. This is done by regressing lagged popularity on the exogenous variables in the equation.[47] The new representation of lagged popularity is then inserted in the original equations in lieu of the observed rating. The degree to which it adequately represents the systematic component will reflect the completeness of the equation and precision with which the exogenous variables have been measured. Malinvaud in simulations has determined that ordinary least squares (Table A.1) gives better results than estimation with the instrumental variables when the samples are small (around 20 observations).

Hibbs suggests a further correction of the Instrumental Variables estimates.[48] Since the I.V. estimates are consistent, they can be used to determine the correlation of the errors and less biased estimates can be developed by applying "generalized least squares." Examining the Durbin-Watson statistic for the I.V. equations indicated that the residuals were strongly correlated for the Eisenhower term ($p = .75$) and indeterminant for Kennedy and Johnson. A correlogram analysis of these three time-series suggested a first-order autoregressive process. In Table A.2 each of the equations has been reestimated, correcting for potential serial correlation.

The main difference between these estimates and those provided in Table 5 is that the lagged popularity value for Eisenhower is almost wholly eliminated and the overall explanatory power of this equation reduced to 11 percent. The estimates for the other equations suffer as well but to a lesser degree.

Tables 5, A.1, and A.2 offer three alternative estimation procedures for testing the model. At each successive stage technical corrections become more complex and the estimates less interpretable. Malinvaud's sober conclusion of these procedures is appropriate here: "This examination of the various possible methods for treating autoregressive models with serially correlated errors leads to rather a pessimistic conclusion. When the available series are short, none of the proposed methods allows precise estimation of the serial correlation of the errors. In these conditions, direct least squares fitting certainly gives the best estimates of the coefficients. But the distribution of the estimated coefficients cannot be determined very well. So the tests and confidence intervals are necessarily very approximate."[49]

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[46] See for example Neustadt, *Presidential Power* (New York: Wiley, 1960), ch. 5; George Edwards, "Presidential Influence in the House: Presidential Prestige as a Source of Presidential Power," *American Political Science Review*, 70 (March 1976), 101–13; Samuel Kernell, "The Truman Doctrine Speech: A Case Study of the Dynamics of Presidential Opinion Leadership," *Social Science History*, 1 (Fall 1975), 20–44; and Samuel Kernell, "Presidential Popularity and Negative Voting: An Alternative Explanation of the Midterm Congressional Decline of the President's Party," *American Political Science Review*, 71 (March 1977), 44–66.

[47] Hibbs, "Problems of Statistical Estimation."

[48] Hibbs.

[49] Malinvaud, p. 569.

Table A.1. Original Regression Estimates With the Untransformed Values for Lag of Popularity

	% Change Unemployment[a]	% Change Prices[a]	Rally	Early Term	Watergate (Dummy)	Killed or Casualties	N.V. Bombing	Lag of Popularity[b]	Intercept	r^2
Truman	–	−104.22 (60.07)[a]	1.14 (.55)	−.94 (1.85)	–	−.457 (1.10)	–	.64 (.17)	13.0	.89
Eisenhower	−1.34 (1.69)	−125.44 (64.56)	.25 (.27)	.62 (.45)	–	–	–	.72 (.07)	64.2	.65
Kennedy	−10.0 (9.8)	–	1.05 (.29)	.62 (.63)	–	–	–	.70 (.11)	17.7	.83
Johnson	−33.31 (14.52)	−484.89 (159.38)	.41 (.31)	.49 (.46)	–	−.004 (.001)	−.0003 (.0002)	.48 (.11)	36.2	.94
Nixon	–	−166.90 (57.88)	.78 (.31)	.59 (.44)	−4.88 (2.83)	–	–	.60 (.09)	24.3	.93

[a] The figures in parenthesis are the standard errors of the slope. Regression coefficients which are twice the standard error are statistically significant at .05.

Table A.2. Regression Estimates Correcting for Serial Correlation[a]

	% Change Unemployment	% Change Prices	Rally	Early Term	Watergate (Dummy)	Killed or Casualties	N.V. Bombing	Lag of Popularity	Intercept	r^2
Truman	–	−123.21 (89.55)	1.70 (.58)	−.24 (2.19)	–	−1.62 (1.13)	–	.17 (.135)	20.10	.58
Eisenhower	−2.61 (3.75)	−111.67 (125.26)	.56 (.32)	1.26 (.76)	–	–	–	.04 (.05)	21.79	.11
Kennedy	−3.70 (15.45)	–	1.10 (.36)	−.05 (1.46)	–	–	–	.66 (.18)	13.5	.59
Johnson	−49.10 (15.38)	−730.85 (165.93)	.48 (.32)	.49 (.54)	–	−.0059 (.0016)	−.00019 (.00024)	.27 (.10)	37.7	.88
Nixon	–	−331.99 (91.73)	1.09 (.39)	.99 (1.01)	−10.6 (3.7)	–	–	.06 (.08)	30.8	.75

[a] All values for the independent and dependent variables have been transformed for a 1st order autocorrelation process. Lag of Popularity is the instrumental variable transformation.

The Decline of Class Voting in Britain: Problems of Analysis and Interpretation

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Social class has long been recognized as being the preeminent influence upon electoral choice in Britain, but recent studies provide support for the argument that it has become a weakened force. These studies differ only in the interpretation that they place on its decline. Through the simultaneous estimation of the effects on voting choice of class measured by occupation, and other variables, this article shows how one of the recent studies implied an overestimate of the declining importance of occupational class. Further it shows that when the indirect effects of class on party choice are taken into account, by means of causal modeling techniques, its preeminent position in determining voting choice in Britain can still be seen. The article seeks to clarify the nature of class-based voting behavior during a period of dramatic decline in this phenomenon, terminating at the last point at which traditional measures can reasonably be used.

1. The Problem

Social class has long been recognized as the preeminent influence upon electoral choice in Britain.[1] It has even been argued that it is the only significant influence. One statement of this viewpoint asserts: "Class is the basis of British party politics; all else is embellishment and detail" (Pulzer, 1967, p. 98). And even those who do not subscribe to this "homogeneity" thesis do not question the electoral primacy of social class (Budge and Urwin, 1966). However, this consensus rests on the analysis of survey data collected in the 1950s and early 1960s. More recent surveys provide support for the argument that class has become a weakened force in British electoral politics. For example, Butler and Stokes (1974, pp. 203–05), Rose (1974c, pp. 47–48) and Crewe, Särlvik and Alt (1977, pp. 168–81) all point to an erosion of class-based support for Britain's two major parties in recent elections. These studies differ only in their interpretations of this decline. In particular, although this is incidental to his thesis, one of Rose's tables shows the disappearance of occupational class as a predictor of partisanship in 1970, while the other two studies show it to have continued to exert an important, although diminished, effect in this year.[2]

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[1] We are aware that social class is a highly complex phenomenon that has been measured in a number of ways, ranging from single indicators to indices of socioeconomic status. It is not our intention to become embroiled in a discussion of the relative merits and shortcomings of these measures, since we have nothing to add to it (although see below, n. 2). Instead we choose to follow the generally accepted convention in British voting studies of measuring social class by respondents's occupation (Alford, 1963; Butler and Stokes, 1974, Ch. 4). It should be borne in mind, however, that occupational class, as we conceive it, stands for a whole set of life experiences and chances, some of which occur before adulthood and entering a specific occupation. For this reason, the meaning of the variable should not be interpreted narrowly. This caveat becomes important in our later analysis of the causal relationships between various social structural variables and voting choice.

[2] It is important to recognize here that Rose himself at no point concludes that social class ceased to have an important effect on electoral behavior in 1970. In his view, occupation is only one of several dimensions of social class (Rose, 1968, pp. 143–52). He accords it analytic primacy in examining its effects together with each of a number of socioeconomic measures (Rose 1974c, pp. 33–47). Having elaborated on the conjoint effects of occupation and other indicators of class (as he views them), Rose then introduces a multivariate analysis to summarize the effects of all the variables examined. It is in this final reduction that occupational class is seen to disappear as a predictor of partisanship in 1970 (p. 48). In the context of this general approach, such findings imply no more than that 1970 saw a change in the relative importance of various socioeconomic determinants of partisanship. Our own interest in Rose's work lies not in his broad concern with social structural influences on voting, but in the behavior of occupation as a variable in his summarizing multivariate analyses. In view of Rose's more catholic operational definition, it is important for the reader to bear in mind that we use "social

This difference in interpretation is worth pursuing. Rose examines the joint effects of occupational class and other social-structural variables, and shows that by 1970 occupational class itself adds little to an explanation of voting behavior that already takes explicit account of other variables. Such a multivariate approach constitutes a more interesting perspective on the decline of class voting than the essentially bivariate one adopted in the other two studies. But, in addition to being multivariate, Rose's analysis was conducted in stages; and in a "stagewise" analysis, much depends on the order in which variables are considered. In the particular stagewise analysis employed by Rose, the order in which variables are considered is determined by a set of considerations to be outlined in the next section of this article. In 1970 these considerations led occupation to be considered only after the influence of other variables had been taken into account, and in this situation it added nothing to the amount of variance already explained. The result is a parsimonious prediction of voting choice in that it involves the smallest number of independent variables. But when our concern is to trace changes in the explanatory power of a particular variable, a stagewise approach may not be appropriate.

The purpose of this article is to take a multivariate approach to explaining party choice in Britain, and particularly to assessing the decline in the single most important variable in British voting studies—occupational class—which, following Butler and Stokes (1974, Ch. 4), we take as our indicator for social class. To this end, we shall begin by demonstrating how a stagewise approach can be misleading in its implications as to the extent of changes in the explanatory power of particular independent variables. Through the simultaneous estimation of the effects of occupation and other social-structural variables on party choice, we shall show specifically that a stagewise approach overestimates the absolute and relative decline in the importance of occupational class, and, further, that when the indirect effects of class are taken into account by means of causal modeling techniques, it retains its preeminent position in structuring voting choice in Britain in 1970.[3] These separate approaches will be shown to paint different pictures of the impact of class on British voting behavior, all of which are equally valid from a statistical point of view. However, differentiating between prediction and explanation, we shall argue that the most rewarding approach is the one that contributes to our general understanding of the social processes through which class influences the electorate.

2. The Analysis (I): Why Tree Diagrams Can Be Misleading

The particular stagewise multivariate analytical technique that concerns us here (a tree analysis performed by the Automatic Interaction Detector) has achieved an important place in political sociology in recent years.[4] The strengths of this technique are indisputable.[5] However, we feel that the weaknesses deriving from its stagewise variance decomposition procedure have not always been adequately recognized. Therefore, a brief discussion of how the technique works will be helpful to our later discussion of its weaknesses.

As its name implies, the AID computer program was originally developed to detect statistical interaction between intercorrelated predictors of some phenomenon. This phenomenon, the dependent variable, can be either an interval level or dichotomous nominal level

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class" (or "class") and "occupational class" (or "occupation") synonymously *throughout* this paper. Thus even when discussing Rose's work, we sometimes use the term "social class" to refer to the occupation variable.

[3] The data used here are the same as those analyzed elsewhere by Rose (1974a, 1974c). They consist of four large nationwide quota samples collected by the Gallup Poll in the four weeks preceding the 1970 election, and analyzed with their permission. Details of the variables we actually used (chosen to make our study comparable with Rose's) are given in a separate appendix available from the authors on request. Their individual selection in the British context has already been justified on theoretical grounds (Rose, 1974a, pp. 500–23).

[4] This technique has been used in a number of single-election studies. See, for example, Liepelt (1968), Janowitz and Segal (1967), Särlvik (1969 and 1970) and, more recently in set of comparable country studies, Rose (1974b). In his *The Problem of Party Government* (1974c), Rose uses the technique to investigate the changing importance of the same set of variables in structuring voting choice in a single country over a series of elections. The version he used was AID I, but we were able to take advantage of a more advanced version (AID III), whose particular strengths are discussed in Sonquist et al. (1971). See also n. 7 below.

[5] See Sonquist et al. (1971) and section 3 in the text.

variable. The predictors, or independent variables, by contrast, must be divided into two or more categories.[6] The program's analytical procedure starts by taking the first specified predictor and executing a conventional one-way analysis of variance on every combination of categories that splits the data set into two mutually exclusive groups. Each of these binary splits explains some proportion of the total variance in the dependent variable; and that split explaining more than any other is remembered by the program, which then repeats this search procedure for all of the remaining predictor variables.

After examining each predictor in this way, the program can decide which binary split on which independent variable explains more variance than any other possible split. This optimum split then serves as the criterion for the notional division of the total data set into two subsets. If we imagine the full data set to constitute the trunk of a spreading tree, this first split can be visualized as giving rise to two branches. In the next stage, the program examines each notional branch in turn, determining by the same search procedure which binary split on which predictor explains the largest amount of the residual variance in each subset of the data. Because the subsets are examined independently, the predictor variables splitting each of them need not be the same. When subsets at the same level in the tree are split by different predictors, interaction can be seen to be taking place. The program continues to split branches until none of the predictor variables is able to decrease the amount of unexplained variance in any of the several subsets of the data by an amount statistically significant at a prespecified level.[7]

Applying AID to party choice in the 1959, 1964, 1966 and 1970 general elections, Rose found that 1966 produced a clear change in the pattern of the relationship between social structure and voting choice.[8] In the two previous elections, the AID trees showed little in the way of interaction. In addition, they indicated that occupation was by far the best predictor of party choice in these elections. But in 1966 there was a clear break with this pattern. Not only was the tree asymmetrical in this third election, but also the full sample split first on type of tenancy rather than on occupation, and the two tenancy subsets then split differently—tenants on occupation and home owners on union membership. Dissimilar splits also characterized later branches of the tree. More important for our purposes, however, is the fact that occupational class had not only lost its 1959 and 1964 position as the most powerful predictor, but it had also dropped out of one side of the tree altogether. It now appeared to explain only 1.7 percent of the variance in party choice instead of the 12.4 and 11.5 percent that it had explained in the 1959 and 1964 elections, respectively. A similar analysis of 1970 voting choice showed occupational class not to appear in the tree at all.

However, to place these findings in perspective, it is necessary to remember that AID's analytical procedure operates stage by stage to remove from the sample all variance explained by the predictor variables upon which the data set is split. Consequently, other predictors that happen to be correlated with the "best" predictor at each stage in the analytical procedure have no opportunity to claim some portion of this variance. Thus, in the case of two powerful but highly intercorrelated predictors, one of them will have its explanatory power underestimated so that it will appear lower in the tree or even fail to appear at all. It will fail to appear when the variance left for it to explain after the first split is seized by other predictors with which it is also correlated but which have at each stage a higher bivariate correlation with the dependent variable. We shall now show how occupation came to be underestimated in 1966 and how it failed to appear in the tree in 1970.

When all predictor variables were split into two groups, type of tenancy proved to be the most powerful one in 1966, explaining 9.2 percent of the variance in partisanship as indicated in Rose's tree diagram (Rose, 1974c, p. 496). What the tree does not show, however, is that in bivariate terms, class explained as much as 9.1 percent of the variance in partisanship. To say the least, this difference is marginal and, even with a sample of more than 6,000 respondents, it is well within the range of sampling error. Thus, a different sample might well have reversed the bivariate impact of these variables with the result that the AID analysis in this year would have split first on occupation, as in 1959 and 1964. Had this been the case, the explanatory power shared by the two variables would all have been attributed to occupation. This variable would then have appeared to explain more than 9 percent of the variance in voting choice, leaving tenancy with rather less explanatory power than it had enjoyed in 1964.[9]

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[6] AID III does provide for the use of interval level covariates as predictor variables, but this facility has not been used in any of the studies that we have examined.

[7] Other features of the program that can be prespecified by the researcher employing AID III (but not the earlier version used by Rose) include the ability to overcome the automatic algorithms we have outlined to the extent of forcing any number of predictors into the analysis in the order desired by the researcher. We make use of this "starting tree" capability below.

[8] For the full presentation of these trees see Rose (1974c, pp. 493–97).

The order in which the tenancy and occupation variables were entered into the program automatically could also have been reversed in another way. It is a limitation of all versions of AID that the analysis insists on finding, at each stage, that variable which has more explanatory power than any other when all codes are grouped into two categories.[10] This gives an advantage to bimodally distributed variables at the expense of those with three or more modes. For example, had the program not imposed this binary restriction on the data, middle- and lower-middle-class voters might have been distinguished in a three-way split, and the 1966 explanatory power of occupational class boosted from 9.1 to 10.2 percent. A similar three-way split on tenancy (distinguishing council house tenants from other types) increases the explanatory power of this variable as well (from 9.2 to 10.1 percent) but tenancy is more bimodally distributed than occupational class, and so gains less of a boost from this extra discrimination. The result is to transform occupational class into marginally the best predictor of partisanship in 1966. Trichotomizing the categories in 1970 would not have had this result. With the same changes in coding, the explanatory power of tenancy would have been boosted from 7.1 to 7.8 percent, while that of class would have been boosted from 5.5 to 6.2 percent.

3. The Analysis (II): Simultaneous Estimation of the Effects of Class and Other Variables

We have shown that the stagewise nature of the analytical technique embodied in the AID procedure can have quite serious consequences when attention is focused on changes in the explanatory power of any particular variable included in a series of analyses. The decline in the predictive power of class when measured by occupation is beyond dispute, but by 1970 it had not disappeared. This is shown in the bivariate correlations between class and party choice in the elections held between 1959 and 1970. Table 1 presents these coefficients, as well as the proportion of variance in partisanship explained by class in each election (the square of the bivariate correlation) and the decline in this percentage from year to year.[11]

Table 1. Correlations and Variance Explained in Labour Vote by Occupational Class, 1959–1970*

	1959		1964		1966		1970
Correlation with Labour vote	0.35		0.34		0.30		0.23
Variance explained	12.5%		11.5%		9.1%		5.5%
Decline from year to year		0.9%		1.3%		3.6%	

*The 1959 and 1964 correlations have been calculated from the original trees found in Rose (1974c, pp. 494–95).

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[9] To economize on space, a pictorial representation of this tree is not presented in the text. The variables figuring in it, however, are presented in Table 2. It should be noted that, in contrast to Rose's (1974c, p. 496) 1966 tree in which tenancy entered first, this tree is symmetrical and contains the same variables in the same order as the 1959 and 1964 trees presented by Rose (1974c, pp. 494–95). This contrast supports our later argument that, if there was any major change in the determinants of British voting behavior, this change took place in 1970 rather than in 1966.

[10] At a later stage, the analysis may still discriminate between categories of a variable that has already been split if, at that point, the split will explain more variance than any other in its subset of the data.

[11] To maintain the comparability of our results, the independent variables in all correlation and regression analyses in this article are the same binary variables as those generated automatically by the AID program. For all variables, the category containing more Labour voters has been coded 1 and the other 0. As is customary, respondents not voting either Labour or Conservative were excluded from all analyses. But missing data on the independent variables was not excluded. In Rose's AID runs, the category to which missing data was assigned was decided by the AID

This table shows first that the correlation between occupational class and partisanship persisted in 1970. Secondly, in contrast to the implications of the AID analysis that a relatively larger decline in its importance took place between 1964 and 1966, the table shows that the decline was most pronounced between the elections of 1966 and 1970.

But bivariate coefficients are inadequate substitutes for multivariate analysis because they ignore the fact that class is not the only important predictor of partisanship. Nor is it completely independent of other social structural predictors, as we have already indicated. A more balanced perspective on the importance of occupational class from one election to the next, therefore, would be gained by ascribing to each predictor an importance commensurate with its unique explanatory power, together with a due proportion of the power that it shares with other variables. Multiway analysis of variance would provide one such perspective, but we have chosen to perform an equivalent analysis by means of multiple regression.[12]

Like AID, multiple regression is a particular application of the General Linear Model. When used in a search procedure to find an optimum set of predictor variables, AID is in effect being employed as an analog to stepwise regression analysis. But the techniques differ in two important respects. When multiple regression is used in a stepwise manner, it can find, in turn, each independent variable that explains most residual variance in the dependent variable. But, unlike AID, the data set is not split on the variables once they have been identified. Instead, an estimate of their impact is calculated over the whole data set. For this reason, the explanatory power that each shares with others can be apportioned between them according to the weight of each in the analysis.[13] While avoiding the stagewise problem of assigning all the shared explanatory power to a small set of predictors, this stepwise procedure sacrifices AID's ability to detect interaction effects through the sequential splitting of the whole data set into independent subsets.[14] But if interaction effects are known to exist (perhaps as a result of previous identification by AID) allowance can be made explicitly for them by including special terms in the regression equation (Blalock, 1972, pp. 463–64). In our case, such terms were not found to be important either in 1966 or 1970.

Another difference between the two techniques is that multiple regression, unlike AID, assumes interval measurement of all variables. But if categorical variables are dichotomized, as is done automatically in AID, multiple regression can be made to generate the same coefficients as AID where the data set is successively partitioned to simulate the workings of the tree analysis program.

When multiple regression is used to estimate simultaneously the explanatory power of the independent variables found by Rose to be consistently important predictors of party choice, the results are not the same as those generated by the AID program. The two sets of results are presented in Table 2. Only those variables are included which appeared in at least two of Rose's 1959, 1964, 1966 or 1970 trees.[15] In order to achieve comparability

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program. To maintain comparability, we have followed the same procedure, which, in the case of multiple regression, amounts to using a least squares estimate of the value to be assigned to the missing data where the resulting value is constrained to 1 or 0. The proportion of missing data never exceeded .0001.

[12] On the equivalence of multiple regression and analysis of variance, see Cohen (1968). We do not mean to imply that conventional regression analysis represents absolutely the best technique available for handling this type of problem. When the data at hand can be represented by a multiway contingency table, it may seem preferable to use a technique usually called log-linear modeling (Goodman, 1972). However, multiple regression can be exploited to provide coefficients directly comparable to those generated by AID and its analytical procedure is close enough to AID's to yield valuable comparative insights. Moreover, the use of multiple regression at this stage in our discussion serves as a bridge to our later use of causal modeling techniques. The major advantage of log-linear modeling is in avoiding biased coefficients when very large or very small proportions of the data fall within one conjunction of characteristics, but we are persuaded that this problem does not arise with our data.

[13] The formulae for the different estimation techniques embodied in stepwise and stagewise regression are presented in Draper and Smith (1966, p. 175).

[14] Some researchers may also find the resulting coefficients less easy to interpret than the tree diagram that can be derived from the AID analysis, but see the notes to Table 3 in the text. For a discussion of some potential pitfalls in the regression approach, see Macdonald (1976).

[15] Two variables appeared in Rose's 1966 tree which appeared in none of the other trees, or in our own forced tree for 1966 (see above, section 2). These were "education" and "constituency partisanship" (Rose, 1974c, pp. 39–40), explaining 1 percent and 1.4 percent of the variance in party choice, respectively (p. 48). These variables are ignored at this stage

between these results, we have used the regression weight of each predictor to calculate a coefficient which, in the context of this particular analysis, can probably safely be interpreted as the proportion of the explained variance attributable to that variable when it is given its due proportion of the explanatory power it shares with other variables, in addition to that part unique to itself.[16]

Table 2 emphasizes that the methods of assigning shared variance embodied in the two analytical techniques provide different estimates of the relative explanatory power of predictor variables both in 1966 and in 1970. Particularly noticeable on the left hand side of each half of the table is AID's extreme sensitivity to the order in which intercorrelated predictors are entered into the program. Regression's procedure of averaging shared explanatory power can be seen as being in some sense more reliable. In addition to highlighting the deficiencies of AID when attention is focused on a single predictor, the table also points to the deficiencies of bivariate analysis. We stated earlier that occupational class and tenancy explain about the same amount of variance in (and hence have very similar bivariate correlations with) Labour party voting in 1966, but Table 2 shows that, in regression terms, tenancy had more predictive power than class in that election. The different interpretations afforded by the bivariate and multivariate analyses can be explained by the fact that class is more highly intercorrelated with the other independent variables than is tenancy, so that when this shared variance is averaged over all four predictors, its explanatory power is reduced by a greater amount than that of tenancy (Gordon, 1968). But perhaps the most important single feature of this table from our point of view is that the 1970 regression analysis shows occupational class to have continued to have a place in predicting partisanship in that year, even in multivariate terms, although its predictive power did decline. Its disappearance from Rose's (1974c, p. 497) 1970 tree was an

Table 2. Comparing Predictors of Partisanship: AID and Multiple Regression Estimates, 1966 and 1970 (Percentages)

Independent Variable (Labour Characteristic)[c]	1966 AID I[a] Tenancy Automatically Entered First	1966 AID III Class Constrained[b] to Enter First	1966 Regression[d]	1970 AID I Tenancy Automatically Entered First	1970 AID III Class Constrained[b] to Enter First	1970 Regression[d]
	(Variance Explained)			(Variance Explained)		
Tenancy (tenant)	9.2	5.3	6.5	7.1	4.8	5.3
Class (working)	1.7	9.1	4.1	–	5.3	1.8
Union (member)	3.4	2.1	4.4	3.3	1.0	3.4
Telephone (none)	1.4	–	3.3	1.6	–	2.6
Total Variance Explained	15.7	16.5	18.3	12.0	11.1	13.1

[a] Two variables that appeared in Rose's 1966 AID tree are not included in this table. See n. 15.

[b] In order to derive coefficients with class entered first, the program was constrained to split first on occupational class even though in two categories this variable explained less variance than tenancy. This constraint is an available option in the AID III program; it was not available to Rose when his earlier analysis was conducted.

[c] See n. 9.

[d] See n. 16 for an explanation of this coefficient.

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of our analysis (education reappears in the next section) because we regard Rose's 1966 tree as an anomaly whose divergent pattern was not repeated in 1970 and would not have appeared in the analysis of a slightly different sample for 1966 (see above).

[16] The coefficient of the ith variable is equal to:

$$\beta_i^2 + \frac{|\beta_i|}{\Sigma|\beta|}(R^2 - \Sigma\beta^2)$$

This procedure increases the value of each variable's β^2 coefficient (the explained variance uniquely attributable to it) by an amount proportionate to that variable's weight in the analysis. The resulting coefficients are comparable to those given by AID in that they sum to the total of the variance explained. To have used beta weights would have made no difference to our argument; it would only have complicated its exposition.

artifact of AID's method of assigning shared variance.

In the following two sections of this paper, multiple regression analysis is used in two somewhat different ways. In section 4, it is used to sift through the full set of Rose's social structural variables to ascertain whether or not any more variables failed to appear in either the 1966 or 1970 trees because of their high correlation with other predictors, as was the fate of occupational class in the 1970 AID analysis. Then, in section 5, we move away from simple prediction to consider the wider question of the indirect effects of class on party choice through its influence on other social-structural variables. Such an approach is necessary to a full understanding of the electoral effects of class, because many of the other variables that occur later in the individual's life cycle, but which also serve to structure voting choice, are themselves in part determined by class. In that section, regression analysis, by means of causal modeling techniques, will be employed to determine both the direct effects of class on voting choice, and also its indirect effects through intervening variables.

4. The Results (I): The Relative Decline of Class as a Predictor of Partisanship

Our exploratory use of regression analysis affords the results summarized in Table 3. In line with our AID inclusion criterion, we have identified only those predictors that explained at least 0.5 percent of the variance in partisanship (when their shared and unique explanatory power are taken together).[17] The table's most striking feature is the presence of two predictors, one of which–car ownership–appeared in none of the AID trees, and the other of which–education–appeared only in Rose's 1966 tree, explaining one percent of the variance in Labour vote (Rose, 1974c, p. 496; and see above, n. 15). The more important of these is education.

Table 3. Combined Explanatory Power of Six Social Structural Variables on Partisanship, 1966 and 1970 (Percentages)

	1966		1970		
Independent Variable (Labour Characteristic)[a]	Effect on Percent Voting Labour[b]	Variance Explained[c]	Effect on Percent Voting Labour[b]	Variance Explained[c]	Change in Explanatory Power 66–70
Tenancy (tenant)	17.6	5.6	17.0	4.7	–0.9
Union Membership (member)	16.2	4.7	15.0	3.7	–1.0
Class (working)	11.9	2.9	7.5	1.3	–1.6
Education (less)	13.3	3.0	7.2	1.1	–1.9
Phone Ownership (none)	11.6	2.6	11.1	2.1	–0.5
Car Ownership (none)	3.7	0.7	5.5	0.9	+0.2
Totals	74.3	19.5	63.3	13.8	–5.7
Constant[d]	12.6		14.7		

[a] See n. 9.

[b] The effect on percent voting Labour of each independent variable is the partial regression coefficient ("b") controlling for all other independent variables. These coefficients may be thought of as differences in proportions. Thus the group of voters not having a telephone can be thought of as containing 11.6 percent more Labour voters when all other variables are held constant. Effects for conjunctions of categories can be added.

[c] This variance explained is the same coefficient as was presented earlier in the regression columns of Table 2, an explanation of which is given in n. 16.

[d] The constant is the intercept ("a") term in the regression equation. With all variables coded 1 if they have a Labour characteristic and 0 otherwise, this can be interpreted as the percentage of Labour voters among respondents having none of the Labour characteristics represented by the independent variables. When added to the total effect, it gives an estimate of the percentage of Labour voters among respondents having all the Labour characteristics.

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[17] The variables to be included were identified in a manner similar to the "backward elimination" procedure described in Draper and Smith (1966, pp. 167–68). The variables from which the choice was made were tenancy, occupation, phone ownership, car

The type of education a British citizen receives is an important socializing influence and has considerable implications for adult political behavior. Butler and Stokes (1974, pp. 102–06) have shown how the predominant class environment encountered in different types of schools can reinforce the class environment of the home when the two are in conformity, contributing to the transmission of parental party allegiance. Educational mobility, on the other hand, weakens the relationship between the child's and parents' party allegiance. Education would therefore be expected to exercise an influence on voting choice independent of class. Our education variable, however, distinguished between persons who left school at different ages, not between types of school. Although most of those educated in predominantly working-class surroundings will have left school at the minimum legal age, the reverse is less likely to be so true of those educated in predominantly middle-class surroundings. So education, as recorded in our data, will probably not have the impact attributed to it by Butler and Stokes. Nevertheless, Table 3 still shows it to be an important predictor of voting choice both in 1966 and 1970. In multivariate terms, it can be seen to have explained 3 percent of the variance in partisanship in 1966—as much as class. Although not shown in the table, its bivariate correlation of 0.28 with voting choice was as high as that of telephone ownership, and higher than any other save class or tenancy.

The other previously ignored variable to appear in Table 3 is car ownership. Its increased predictive power between the two elections serves to confirm a general pattern discernible in the results presented in the table, and upon which we shall now focus our attention.

Table 3 confirms Rose's findings by making it clear that the ability to predict partisanship from social structure declines by almost one third, or just under six percentage points, between 1966 and 1970. But, notably, this drop is largely attributable to a decline in just two of the six variables, class (1.6 percent) and education (1.9 percent). The predictive power of tenancy, union membership and telephone ownership each declined by one percent or less over the same period, while that of car ownership increased by 0.2 percent. An important distinguishing characteristic of class and education, in comparison to these other variables, is that they are not easily changed after an individual reaches adulthood. This is generally held to be true even when class is measured by occupation (Butler and Stokes, 1974, Ch. 4). One does not have to go back to school in order to buy a telephone or a car, or in order to enter a trade that requires one to join a union (Rose, 1974c, p. 49). And these latter variables continued to have much the same impact in 1970 as in 1966. Class and education, on the other hand, relate to early socialization experience; and the declining importance of these variables in structuring voting choice suggests that today the British electorate is less influenced by childhood socialization than in the past. Instead, influences arising from the social context in which individuals found themselves at the time of the 1970 election exercised a relatively greater determining influence.

5. The Results (II): The Relative Decline of Class as a Cause of Partisanship

If we are correct in distinguishing class and education as being in some sense prior to the other influences on party choice (but see n. 19 below), any discussion of the decline of class voting in Britain must take at least one more step, which is to consider whether or not class has an additional influence on partisanship by way of its effects on the "lifestyle" variables that we have taken to define an individual's social context (see also Rose, 1974a, p. 531). Type of tenancy and union membership, in particular, have traditionally been thought of largely as consequences of social class. If this is the case, the full impact of class will be underestimated when such temporally subsequent influences are directly controlled for in a multiple regression equation. In taking this step, we move from a simple consideration of how well social structure predicts partisanship to the larger question of what causes partisanship. Any reasonably sophisticated notion of causation must take account of indirect as well as direct influences on the phenomenon in question.

Two causal modeling traditions are particularly appropriate for dealing with the indirect effects of independent variables. The first concentrates on the proportion of variance in a dependent variable whose explanation can be attributed to different causal paths (see, for example, Stokes, 1974). The second tradition focuses its attention on the amount of change

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ownership, union membership, religion, age, education, sex, constituency partisanship, urban/rural residence, region and nation. These are described in more detail in a separate appendix (see n. 3 above).

in the dependent variable that can be thought of as resulting from possible manipulation of the variables on the different causal paths. This latter tradition finds its most recent exponent in James A. Davis (1975), who has shown how the technique can be linked to the use of flowgraphs advocated by Stinchcombe (1968) in a manner particularly appropriate when the variables are measured at a nominal level, so that their effects can be viewed in terms of differences in proportions. Table 3 has already introduced the notion of an effect of differences in the value of an independent variable on percent voting Labour. Since our variables are measured at a nominal level, we propose to use estimates consistent with this second tradition, though without adopting flowgraph notation.[18]

Figure 1 represents a structure in which class and education are considered to cause, albeit imperfectly, voting choice, both directly and also indirectly through the "lifestyle" variables (type of tenancy, union membership and telephone ownership). Car ownership is not included in the model because of the negligible indirect effects it transmitted. The path between education and union membership has been omitted for the same reason. The coefficients on the right-hand side of the model are equivalent to the effect coefficients for 1966 presented in Table 3, although they are of a slightly greater magnitude because of the elimination of car ownership from the model. They indicate the proportionate change in percent voting Labour that might be expected as a consequence of change in any one of the prior variables, holding the others constant. The line with arrowheads at either end indicates that the intercorrelation between two variables (in this case class and education) has been taken into account when estimating the other coefficients, although no causal priority has been

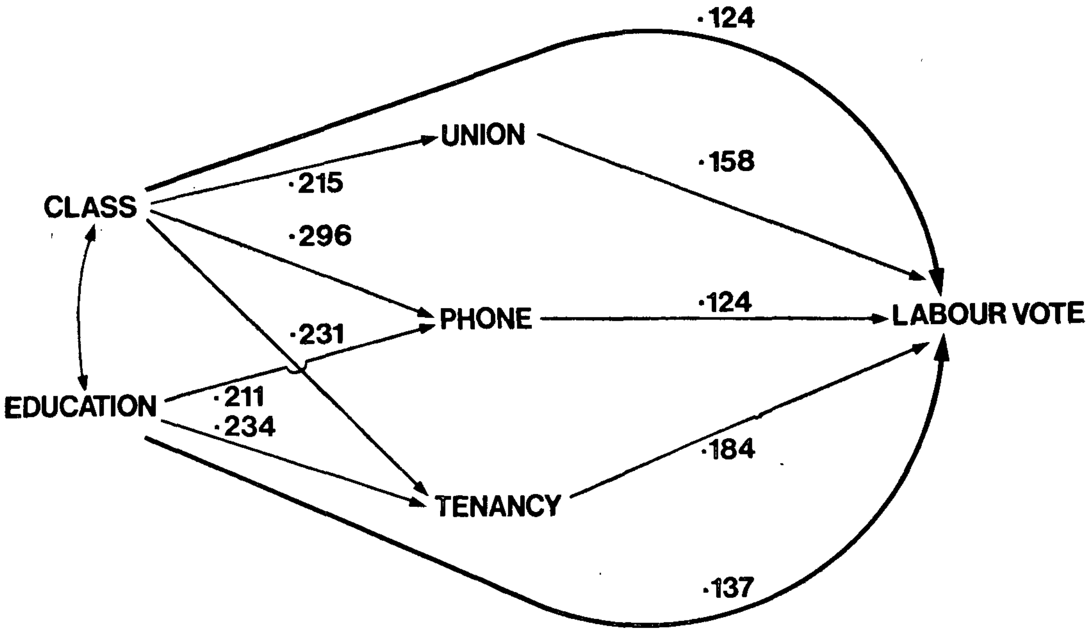

Figure 1. A Causal Model Illustrating the Effects of Class and Education on Labour Vote, 1966

assigned as between the two.[19] This is the standard way of depicting unanalyzed correlations in causal analysis. Transmission rules such as those set out by Davis allow us to calculate the indirect effects of class and education on voting choice by way of each intervening variable in turn. Such calculations involve no more than multiplying the coefficients on a path. Thus, the indirect effects of class on Labour voting by way of tenancy is (.231) (.184) = .043.

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[18] We feel that flowgraph notation is an important step forward in providing us with a means of illustrating not only causal flow, but also the actual values taken on by variables in a model. However, where the model contains more than four variables, it very soon becomes cluttered by the arrows and coefficients for base values. It then becomes hard to disentangle the causal from the structural implications of the graph. Since we are primarily interested in the causal implications of our model in terms of the power of certain variables to generate change in the value of others, the base from which this change takes place is not of central interest and indications of base values have not been included. Flowgraph notation would also require the complication of a block or typology for our intervening variables (Davis, 1975, p. 132).

Table 4 summarizes the direct and indirect effects of class and education on voting choice for 1966 and 1970. In the first of these years, it can be seen that less than half of the effects of class were exercised indirectly via its influence on tenancy, telephone ownership and union membership. The total effects can be interpreted as the additional proportion of Labour voters due to the direct and indirect effects of class. That is, if it were possible to manipulate a person's class so that a number of middle-class Conservative voters were given a new working-class identity, 24 percent of them could be expected to switch their vote to the Labour party. In 1970, however, the total effects of class had been reduced by more than 5 percent, so that only 19 percent of such a group could be expected to switch their vote. Nevertheless, when indirect effects are thus taken into account, the extent of the decline in class voting in Britain can be seen to have been much less than even multiple regression analysis would have led us to believe. The table shows that over one-third of this variable's direct effects on Labour voting (12.4 to 8.0) were eroded between the 1966 and 1970 elections. But, when viewed as a cause rather than as a predictor of partisanship, because its indirect effects declined relatively little over this period, its total influence can be seen to have diminished by only about one-fifth of its 1966 value (23.8 to 18.5). Indeed, when we take account of its indirect effects, we can see that class, with an effect coefficient of 18.5, remained at least as important a cause of voting choice as tenancy, the single most powerful direct predictor in 1970, with an effect coefficient of 18.0.

6. Conclusion

In this article we have confirmed that there has been an indisputable decline in the ability of class, measured by occupation, to structure voting choice in Britain. This is especially true when the magnitude of this decline is viewed in absolute terms. However, our estimate of the extent of the decline depends on how it is viewed and the corresponding statistical technique adopted to analyze the phenomenon. At one extreme, if we are looking for a prediction of voting choice based on the smallest possible

Table 4. Direct and Indirect Effects of Class, Education and Intervening Variables on Voting Choice, 1966 and 1970

	Independent Variables									
	Class		Education		Tenancy		Union		Phone	
Effects	1966	1970	1966	1970	1966	1970	1966	1970	1966	1970
Indirect Effects										
Via Tenancy	.043	.039	.043	.035						
Via Union	.034	.032	.009[a]	.006						
Via Phone	.037	.034	.026	.023						
Total Indirect	.114	.105	.078	.064						
Direct Effects[b]	.124	.080	.137	.078	.184	.180	.158	.146	.124	.122
Total Effects	.238	.185	.214	.142	.184	.180	.158	.146	.124	.122

[a] The indirect effect of education via union membership, which was excluded from Figure 1 in the interests of parsimony, is included in this table for the sake of completeness.

[b] These direct effects are generally greater in magnitude than those given in Table 3 because, as in Figure 1, car ownership is not included in the equation from which these figures are derived.

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[19] The causal nature of the relationship between class and education is not explored in this model because of ambiguities arising from measuring social class by occupation. Logically, education precedes occupation in the individual's life cycle. But, as was noted in n. 1, class, as measured by occupation, taps life chances and experiences that precede adulthood. Thus, the causal ordering between the two variables is not as unambiguous as might first appear.

number of social structural variables, it is clear that by 1970 occupational class added nothing to the power of a prediction based on tenancy, union membership and telephone ownership. At the other extreme, a model that treats class as causally prior to these three variables, as well as to voting choice, finds it to have remained preeminent in structuring partisanship, even in 1970. Falling between these two extremes is a multiple regression model in which indirect effects are not calculated. Each of these approaches gives a somewhat different view of changes in the effect of class on partisanship, and, from a purely statistical standpoint, all are equally valid.[20] Which view we take must therefore depend on which is most helpful to an understanding of the social processes in which class in involved.

Evidently, the fundamental choice is one between prediction and explanation. If our concern is the extent to which voting behavior can be predicted, then what is needed is some form of prediction equation. Whether or not class enters into such an equation is a matter of trading off parsimony against comparability. An equation not containing occupational class had become the most parsimonious by 1970, but its absence meant that its effects could not be compared to those in earlier elections. A stagewise variance decomposition procedure is likely to provide the most parsimonious prediction equation because of its imperialistic method of assigning variance shared by two or more predictors. It is also easier to describe because each effect is treated separately. A particular stagewise technique, AID, has the added advantage of enabling us to take interaction effects into account in our analysis. However, other things being equal, a stagewise analysis will explain less variance in the dependent variable than if the effects of all predictors are estimated simultaneously by ordinary least squares. The estimate of the effects of each independent variable will also be very different in the two techniques (see Table 2).

If, on the other hand, we are interested in the effects of class on voting choice, then not only can we ill afford to drop our indicator—occupation—from the analysis, but also we must ask ourselves whether class has indirect effects on voting by way of its influence on other variables. In doing this, we impose a particular view of the nature of social processes on our data. To specify the "lifestyle" variables that define an individual's social context as intervening between class and voting choice in a causal model ensures that indirect effects will be found. For this reason, the effects that we observe do not serve to validate the model. But failure to treat these variables as intervening involves no less of an imposition. To assume that class has no indirect effects (or to argue that occupation is not a surrogate for class) will simply lower the importance attributed to this variable by our model. This assumption underpins the use of the multiple regression model in preference to its causal counterpart. But again, the results from the former model derive strictly from the rules of algebra and do nothing to validate the assumption that there are in reality no intervening variables.

Having decided that "lifestyle" variables were best treated as intervening, we had to decide on the place of education in the model. Other research had indicated that education is also a cause of these variables, although its causal relationship to class is much more ambiguous. Therefore, we opted for a model which estimated the effects of class while controlling for the influence of education. This model revealed a decline of more than 5 percentage points (from 23.8 to 18.5) in the ability of class, as measured by occupation, to structure partisanship between 1966 and 1970. But it also showed that by far the greater proportion of this decline lies in the direct effects of class on voting choice. The indirect effects of class via intervening variables also declined over the period, but by no more than 1 percentage point, or less than one-fifth of the total decline. The same general pattern holds true for the education variable. Thus, we see a relative (though not an absolute) increase in the power of social context to structure partisanship between these two elections. We tend to attribute these changes to a diminution in the long-term influence of early socialization experiences in the British electorate, and it seems probable that the decline in class voting that we have traced up to 1970 laid the foundations for a quite dramatic abandonment of the two major parties by the British electorate in the 1974 elections.

We have concentrated our attention on the 1966 and 1970 elections partly because the move away from the Labour and Conservative parties was so extensive in the next election as to question the whole concept of a class-based, two-party system. But any reassessment of traditional measures of class voting will put comparability with earlier studies into question. No doubt this must be done, but elsewhere. In this article we have tried only to clarify the nature of class voting, where class is measured by occupation, during a period of decline in this phenomenon, terminating our analysis at the last point at which traditional measures can reasonably be applied.

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[20] Equally, the same deficiencies apply to all of them. See n. 12 above.

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Collective vs. Dyadic Representation in Congress

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Previous studies of legislative-constituency representation have focused almost exclusively on pairs of Congress members and their constituencies. It is possible, however, to think of representation collectively, i.e., to consider the extent to which Congress as an institution represents the American people. Our analysis delineates this concept of representation, analyzes its existence by use of probability theory and the Miller-Stokes data, and then considers the relationship between collective representation and electoral control. We conclude that citizens probably get better representation than is suggested by the Miller-Stokes analysis, that the amount of representation may be more a function of institutional arrangements than of electoral control, and that citizen indifference towards many aspects of legislative politics is quite reasonable, given the existence of collective representation.

Legislative representation has long been a basic concern in political analysis. Particularly in the last 20 years a wide variety of theories, data, and methodologies have been employed to examine the question of whether legislators, in some sense, follow district opinion.[1] The results of these studies are by no means conclusive and controversies abound on how one should analyze representation and what certain types of data actually indicate.[2] Nevertheless, despite the sheer variety of analyses and debate, previous studies almost all share one fundamental perspective: they view representation in terms of a particular legislator and the constituency that elected that legislator. This dyadic perspective (i.e., one legislator and one constituency) is surely important, but it is not the only way of approaching representation. Specifically, a long and equally valid tradition exists that views representation in terms of *institutions* collectively representing a people.[3] Within this tradition the central question would be whether Congress as an institution represented the American people, not whether each member of Congress represented his or her particular district.

This paper will explore both theoretically and empirically this notion of collective representation.[4] In so doing we shall not only elaborate on an important (but frequently neglected) concept in the study of legislatures, but within the limits of our data we will attempt to show the following: (1) legislative representation of citizen opinion when viewed collectively is not nearly as poor as is indicated in most studies of legislator-constituency relationships; (2) at least some features of American politics typically associated with poor representation may in fact contribute to accurate representation; and (3) much of the public ignorance of legislative politics and opposition to disciplined parties is reasonable, given the operation of collective representation.

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[1] See, for example, Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," *American Political Science Review,* 57 (March 1963), 45–56; Lewis Anthony Dexter, "The Representative and His District," *Human Organization,* 16 (Spring 1957), 2–13; Wilder W. Crane, Jr., "Do Representatives Represent?" *Journal of Politics,* 22 (May 1960), 295–99; and Robert S. Erikson, "The Electoral Impact of Congressional Roll Call Voting," *American Political Science Review,* 65 (December 1971), 1018–32; John W. Kingdon, *Congressman's Voting Decisions* (New York: Harper and Row, 1973), esp. Ch. 2; and Aage R. Clausen, *How Congressmen Decide: A Policy Focus* (New York: St. Martin's, 1973), pp. 126–50.

[2] Several of these controversies are presented in Morris P. Fiorina, *Representatives, Roll Calls, and Constituencies* (Lexington, Mass.: D. C. Heath, 1974), Ch. 1.

[3] Several political scientists have considered the theory of collective representation. See, for example, Samuel C. Patterson, "Introduction," in *American Legislative Behavior* (Princeton: D. Van Nostrand, 1968), p. 4 and Alfred De Grazia, *Public and Republic* (New York: Knopf, 1951), pp. 5–8. Analyses of the mechanics of electoral representation also, in a sense, are concerned with collective representation. At a more theoretical level, Hanna F. Pitkin, *The Concept of Representation* (Berkeley: University of California Press, 1967), pp. 216–25.

[4] Though definitions of "representation" abound, our analysis defines representation as agreement between legislative voting and citizen opinion. The closer the agreement, the "better" the representation. This definition is consistent with notions of representation employed in most empirical analyses of representation. It is identical to the conception of representation guiding the Miller-Stokes analysis.

We shall first explicate the dyadic approach to the study of legislative-constituency opinion representation. This approach will then be contrasted with collective and virtual conceptions of representation. Second, nondyadic aspects of representation will be discussed and in part illustrated by data collected by Warren Miller and Donald Stokes. We shall see, for example, that a particular legislator's misrepresentation of constituency opinion can, under certain conditions, increase the overall level of opinion representation. We shall also show that partisan legislative voting discipline can likewise decrease opinion representation, despite claims to the contrary. Third, we will consider nondyadic representation in the context of democratic control of leaders through elections. We shall argue that there is no one-to-one relationship between accurate representation of citizen preferences and citizen electoral control over legislators. Indeed, random selection of legislators would maximize representation at the expense of citizen control. Finally, we shall examine certain citizen attitudes and behaviors in the light of different conceptions of representation and misrepresentation. We hope to show, for example, that ignorance of one's particular Congress member's voting record, political apathy, or even voting for unresponsive legislators are much more reasonable than previously claimed, given collective representation.

Before proceeding we should note two limitations. First, while we speak of legislators in general, our analysis will be of members of the U.S. Congress in particular. No doubt there are legislatures whose rules and functions would make our analysis irrelevant. Second, our data are the well-known Miller and Stokes data, a data set somewhat limited by small sample size in several districts. Nevertheless, both these problems are not serious since our effort is largely theoretical and illustrative. For such a purpose the Miller-Stokes data are perfectly adequate and restricting ourselves to the U.S. likewise poses no serious problem.

Conceptions of Citizen-Legislative Representation

Perhaps due to our underlying democratic values, recent American research on legislative responsiveness has usually viewed representation in an electoral context. That is, citizens are (or are not) represented by an elected official they could have voted for or against. In this sense, a citizen could only be represented by one member of Congress, one governor, two senators, and one president, but not—say—a Supreme Court justice. Scholars have therefore asked: do elected officials represent those who elected them (or who live in their districts)? Regardless of whether constituency opinion is measured by interviews or inferred from socioeconomic or demographic data, the units of analysis have always been the legislator and the district, paired. Representation means a high correlation between constituency opinion and roll-call voting on a pairwise basis.

It should be obvious, however, that the representation of an opinion (or interest) is theoretically independent of an electoral connection between the person with a preference and the person doing the representing. Much in the same way that individuals' economic interests can be advanced by organizations to which they do not belong (these are called "free riders"), their interests can be served in Congress among any one of 435 representatives and 100 senators. Moreover, it is quite likely that one's best representative could change over time and across issue areas. In fact, it is extremely unlikely that out of a pool of 435 legislators one's own legislator will represent one's particular opinion on an issue. An excellent illustration of such representation independent of an electoral relationship no doubt occurred when northern black and liberal white Congress members articulated the preferences of disenfranchised southern blacks during the 1950s and 1960s.

Though it is not customary to separate representation from a direct electoral relationship, any review of the various meanings of representation will show that representative institutions were never automatically equated with electoral institutions. Moreover, even when elections and representation were linked, dyadic representation was never the dominant historical model. In her analysis of the various meanings of representation, Pitkin describes an important school of thought that viewed legislatures as a group of individuals collectively representing the people *as a whole.*[5] The purpose of the legislature is to create an accurate reflection of the community; misrepresentation occurs when the diverse interests and opinions of the political community are excluded from debate. A particular legislator, according to this view, was not a delegate for those particular people who chose him or her, but all the legislators taken collectively would act as if all the people themselves were acting since they were a reflection of the whole. This principle is quite similar to the principle of random sampling: a particular individual in a sample of 1500 from a universe of 210 million does not personally represent 140,000 people, but the sample collectively is a close approximation of the 210,000,000 people.

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[5] Pitkin, p. 61.

The concept of virtual representation, particularly as employed by Edmund Burke, also separates the electoral association between two individuals from their representational relationship. As Burke put it, virtual representation occurs where "there is a communion of interest and sympathy in feelings and desires between those who act in the name of any descriptions of people and the people in whose name they act, though the trustees are not actually chosen by them."[6] Hence, at least for Burke, the city of Birmingham is not automatically unrepresented merely because it has no delegate in Parliament. Because Bristol, which has the same commercial interests as Birmingham, does send members to Parliament, Birmingham is virtually represented. Indeed, Burke asserts that in many instances virtual representation is superior to actual representation (i.e., the representation of electors' interests by their elected officials) since virtual representation is based on common sentiment, not fallible attempts to advance interests one might not share.[7] To be sure, Burke is not talking about constituency *opinions* when he speaks of "interest," but the basic thrust of his argument—that legislators "look out" for nonconstituency interests and thus represent them—is equally applicable to the analysis of opinion representation.

Moreover, many recent studies of legislative decision making have examined nondyadic representational relationships, though rarely has this type of relationship been deemed to be of special theoretical importance. For example, Wahlke, Eulau, Buchanan, and Ferguson in their study of four state legislatures find a significant number of legislators viewing themselves as representing state, as opposed to district, interests.[8] Studies of interest group/legislative interaction have often noted that interest groups must of necessity cut across purely geographical division so that, say, a New York industry wanting tariff protection may be "represented" by a member of Congress from California who happens to be on the relevant committee.[9] Equally familiar are instances in which legislators will "broaden" their constituencies by becoming spokespersons for non-geographically based interests, e.g., automobile safety, withdrawal from Vietnam, or the fate of Soviet Jews. It is clear, then, that many researchers have dealt with nondyadic representation but this behavior has rarely been conceptually distinguished from purely district-oriented activity.

Finally, the concept of collective representation is even embedded in the idea of responsible party government despite this doctrine's emphasis on individual citizen policy voting. That is, citizens choose a party and the victorious party represents a national majority; members of the victorious party do not represent their district majority except insofar as district majorities are congruent with a national majority. Thus it is proper to speak only of national majorities being represented, not the relationship between particular voters and the legislators they actually selected. Indeed, one of the major purposes of party discipline is to eliminate purely dyadic representation.

In short, even if we require an electoral connection between citizens and their representatives, there is no historical or theoretical reason to limit analysis to dyadic representational relationships. To focus exclusively on such questions as "does Representative X follow the constutency's preferences on policies A, B, and C?" thus ignores several equally plausible ways of asking whether elected representatives represent.

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[6] Cited in Pitkin, p. 173. Excellent discussions of virtual representation are also found in Samuel H. Beer, *British Politics in the Collectivist Age* (New York: Knopf, 1967), pp. 15–20 and De Grazia, pp. 36–45.

[7] Pitkin, p. 175.

[8] John C. Wahlke, Heinz Eulau, William Buchanan, and LeRoy C. Ferguson, *The Legislative System* (New York: Wiley, 1962), pp. 290–91, 270. Also see Roger H. Davidson, *The Role of the Congressman* (New York: Pegasus, 1969), pp. 121–26, and Randall B. Ripley, *Congress: Process and Policy* (New York: Norton, 1975), p. 18.

[9] We suspect that the non-dyadic nature of organized interest representation in Congress is so "obvious" that it is seldom explicitly mentioned. That is, it is self-evident that a legislator "looking out" for, say, his own peanut farmers will also represent peanut farmers not in his own district. Also see John E. Schwarz and L. Earl Shaw, *The United States Congress in Comparative Perspective* (Hindsdale, Ill.: Dryden Press, 1976), Ch. 10, for an excellent analysis of how groups such as blacks are represented by non-group members.

Representation Without Control or Agreement

According to the dyadic model, the maximum possible degree of representation would occur if legislators followed the preferences of their constituents (somehow defined). It would therefore seem to follow that citizens would not be represented if (a) all members of Congress voted randomly or (b) all members of Congress—for any number of reasons—violated constituency preferences. We shall argue here that misrepresentation need not occur if Congress members voted randomly or otherwise chose to ignore constituency opinion. What makes such an argument possible is an acceptance of a collective perspective on representation which focuses on the representation of an opinion within an institution independent of an electoral relationship between opinion holder and legislator. Let us begin by considering random legislative voting and collective representation. Subsequently we shall examine how misrepresentation by some legislators can result in better overall responsiveness to citizen preferences.

Random Voting and Representation. As we have stated, analyses of representation based on the dyadic model measure representation in terms of nonrandom association between pairs of constituencies and legislators (i.e., a high correlation indicates close representation). Are we then to equate a zero correlation (i.e., perfect randomness) with zero representation? Our answer is that even with perfectly random legislative voting it is unlikely that national opinion would be violated about more than half the time. In a sense, the use of measures of association with random baselines, together with the dyadic approach, can readily lead to an underestimation of how often majority preferences are heeded.[10] To assess the impact of perfect randomness, let us assume a legislature of 20 members and constituencies. (We shall assume an N of 20 since probability calculations of the type we are going to perform for an N of 435 would be impossible with existing accessible computational facilities. Changing the N to 435 would not alter our conclusion, however.) Furthermore, let each representative adhere to the following behavioral rules:

(1) on any given issue a legislator decides to vote "yes" or "no" by flipping a coin (i.e., votes randomly);

(2) legislators vote simultaneously and independently of each other (i.e., there is no party discipline, logrolling, or presidential pressure);

(3) each constituency (assumed to be singular) decides its preferences by likewise flipping a coin for "yes" and "no" issues.

In view of the "majority wins" rule in legislative decision making, let us add a further stipulation: accurate representation occurs if the majority on a given issue receives 50 percent + 1 in a vote. In other words, if 75 percent of the constituencies favor policy Y and policy Y is passed even by a bare majority, we will conclude that supporters of Y are represented.[11] We should also add that minority opinion within constituencies is excluded.

In this hypothetical legislature the probability that a particular legislator will be in agreement with his or her own constituency is .5 (.25 for the "yes-yes" pair and .25 for the "no-no" pair, for a total of .5). In a 20-member legislature, how many pairwise agreements between legislators and constituency majorities should one expect? Table 1 presents the probabilities (calculated from the binomial) of various representative-constituency matches. As one might expect, the likelihood of *perfect* dyadic agreement on a given issue is extremely remote—$p = .0000095$ (for 435 relationship, this number is virtually zero). It is equally unlikely, however, that all legislators will be on the opposite side from all their constituencies ($p = .0000095$). To determine the probability that a *majority* of constituencies and their own representatives will be in agreement, one may sum the probabilities for the bottom rows in Table 2. The sum if .412, which means that assuming random voting by legislators, a majority of constituency-representative dyads will be in agreement 41.2 percent of the time in a series of votes. If we combine this probability with the probability that exactly half of the constituency-legislative pairs will be in agreement ($p = .176$), the probability is approximately .588 that 50 percent or more constituencies will have their representatives vote "right" even if the representatives vote randomly.

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[10] We should emphasize that our theoretical concern is the degree of representation, not the predictability of this representation. Merely because some representation can, *in a purely statistical sense,* be predicted on the basis of a random model does not mean that this representation does not exist. If my member of Congress "votes right" on a key issue it may be irrelevant to me whether this results from random factors or careful attention to my desires.

[11] Since our conception of collective representation refers to policy agreement between a majority of all citizens and a majority of legislators, our analysis here further requires the assumption that all legislative districts are of equal population. Unless this were true it would be theoretically possible for a majority of constituencies and a population majority to be in opposition. This assumption is reasonable for the House and unless district size were assumed to be constant, our hypothetical example here would become hopelessly confused. Unequal district size could be included in this analysis, but it would not add anything theoretically important.

It is also possible, by using the joint probability of independent events form of the binomial distribution, to calculate the likelihood of legislative majorities being on the same side as a majority of constituencies, regardless of the degree of pairwise agreement.[12] The odds of concurrent legislative-constituency majorities occurring in this formulation are .5 (excluding ties). It should be noted that a representation "success" (majority-majority agreement) can occur even when pairwise agreement is extremely low. Moreover, the chances of legislative distribution of votes being, say, 10 to 15 percentage points within the distribution of constituency majorities are quite good (in the range of 60 to 70 percent likelihood). Hence, even where a majority of constituencies are misrepresented, the outcome given random voting will more than likely be "close." It can further be shown, moreover, that even a slight improvement over randomness in legislative adherence to constituency opinion will result in a majority of constituencies being represented a majority of the time and many more "closer" losses. In short, we cannot equate legislative ignorance of constituency opinion, a lack of desire to be a delegate and other such impairments to dyadic representation as automatically leading to complete, systematic misrepresentation.

Table 1. Probabilities* of Successful Pairwise Legislative Matches Given Random Legislative Voting, Random Constituency Opinions (N=20)

Number of Successful Pairwise Matches	P
0	.0000095
1	.00001907
2	.00018120
3	.00108719
4	.00462006
5	.01478577
6	.03696442
7	.07392883
8	.12013435
9	.16017914
10	.176198001
11	.16017914
12	.12013435
13	.07392883
14	.03696442
15	.01478577
16	.00462006
17	.00108719
18	.00018120
19	.00001907
20	.0000095

*The calculations are from the binomial. The formula is:

$$P(r) = \frac{N!}{r!\,(N-r)}\cdot p^{r}q(N-r)$$

where

N = total number of pairs (20 in this example)
r = number of successes out of 20
$(N-r)$ = number of failures out of 20
P = probability of success (here, .5)
q = probability of failure (here, .5)

Two additional comments are relevant to the issue of representation by random voting. First, this random model seems most applicable to situations where legislators have no knowledge of constituency opinion, no clues from district interest groups (i.e., the "worst" conditions for representation). Where information on preferences is clear, or can be accurately guessed, representation is likely to be much better. Certainly few southern Congress members during the 1950s and 60s could support pro-civil rights legislation in the face of strong, unambiguous constituency opinion. Hence, if a member of Congress toes the line on these sorts of issues and votes randomly on all others, the proportion of votes on which legislative and constituency majorities agree will be somewhere between .5 and 1.0, depending on the mix of unambiguous and ambiguous issues.

A second point concerns individual judgments about how well one's own preferences are being represented in the legislature. If we

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[12] The formula of the binomial distribution for the joint probability of independent events is:

$$\frac{N_1!}{N_1!(N_1-r_1)!}\cdot\frac{N_2!}{N_2!(N_2-r_2)!}\cdot p_1^{r_1}q_1^{N_1-r_1}\cdot p_2^{r_2}q_2^{N_2-r_2}$$

Subscripts refer to samples (i.e., 1 = members of Congress, 2 = constituents); the notation is described in Table 1.

were to say that a "preference is represented" when the preference finds at least one legislative advocate, then it is clear that on a particular issue both sides are extremely likely to have at least 40 percent of the legislative votes most of the time (about 73.6 percent of the time in a 20-seat legislature), given purely random voting. This likelihood of a "good showing" even if one's preference was a majority but lost in the legislature seems particularly important where one's knowledge of the true opinion distribution is unknown. That is, a citizen who favors policy X might guess that fellow supporters of this policy amounted to, say, between 40 to 70 percent of the population. A legislative outcome of 40 percent against 60 percent in favor might thus appear reasonable to a supporter of X even if the true (but unknown) proportion in favor were greater than 50 percent. We are not suggesting that losing is, therefore, cheerfully accepted; rather, misrepresentation would usually be obscured by public ignorance of the true distribution and by random legislative voting. Compare this to what might happen if all legislators voted together on what they (falsely) believed to be majority opinion—a majority would be misrepresented by a 20 (or 435) to zero vote. In a sense, random voting may only guarantee representation half the time, but it also makes gross misrepresentation (i.e., a popular majority being countered by an extremely lopsided legislative vote) a remote possibility.

Individual Misrepresentation and Collective Representation. A different and somewhat broader way of viewing the amount of representation is to think of both constituency and legislative preferences (or votes) in terms of a continuum (or scale) as opposed to a yes-no voting dichotomy. This was Miller's and Stokes' approach and it allows us to consider more refined degrees of representation. In addition, with a sufficient distribution on a continuum we can examine the extent to which relatively small policy minorities are represented. In making this examination we make the same basic contention as in our previous discussion of representation; namely, that a certain degree of representation is likely though legislators may ignore or be oblivious to constituency opinion. Let us begin by considering the hypothetical data in Tables 2A–2B which depict a legislature of three constituencies.

Table 2A shows what might be considered perfect representation. Each district is represented by a legislator with the precise scale position as his or her constituency. Moroever, if we consider national opinion to be the sum total of individual legislator opinion (or behavior), the last column of Table 2A shows perfect representation. Table 2B, on the other hand, shows a situation in which two of the three districts are substantially misrepresented (districts 1 and 3). Does this show that citizens in these districts go without adequate representation? In this particular example, it is evident that citizens in district 1 are represented by the legislator from district 3 and vice versa. Equally important, observe that the mean scale positions for both constituents and legislators in Table 2B are identical. Thus, on the whole, constituents are perfectly represented by legislators, though not necessarily on a dyadic basis. In the Burkean sense, representation is virtual, not actual.

Table 2A. Perfect District-by-District Agreement, Perfect Aggregate Agreement

Opinion/Roll Call Scores for	District 1	District 2	District 3	$\bar{X}$ All Districts
Constituency Preferences ($\bar{X}$)	1	2	3	2
Legislators (scale scores)	1	2	3	2
Differences in Preferences	0	0	0	

Table 2B. Poor District-by-District Agreement, Perfect Aggregate Agreement

Opinions/Roll Call Scores for	District 1	District 2	District 3	$\bar{X}$ All Districts
Constituency Preferences ($\bar{X}$)	1	2	3	2
Legislators (scale scores)	3	2	1	2
Differences in Preferences	–2	0	2	

What we have illustrated in Tables 2A–2B is a general phenomenon. Specifically, by summing across both districts and legislators separately, then subtracting the grant totals, and dividing by the number of districts, we allow positive and negative district deviations to cancel each other out. Thus, as we see in Table 2B, the scores for district 1 and 3, which show deviations of 2 and –2 scale positions from constituency means, cancel each other out when added together. Hence, citizens of district 1 are represented because legislators in both district 3 and district 1 misrepresent their constituents in precisely opposite ways. Statistically, this principle is:

$$\frac{\Sigma|d-R|}{N} \geq \frac{\Sigma d - \Sigma R}{N}$$

where

d = the mean district scale score
R = the representatives scale score
N = the number of pairs

In words, this formula can be stated thus: the average legislator-constituency difference on a dyadic basis is equal to or greater than the average difference between all legislators and all constituents. Moreover, for reasons that will be more fully discussed below, it will usually be the case that $\frac{\Sigma|d-R|}{N} > \frac{\Sigma d - \Sigma R}{N}$, so citizens as a whole are better represented by Congress than are citizens in each district by their particular legislators.

This principle can be illustrated with the Miller-Stokes data. Though an ideal illustration would require that both legislators and constituents be measured on the same scale, we shall make do with scales composed of different items and of different ranges.[13] Table 3 shows both dyadic and aggregate (or virtual) representation on the issues relating to social welfare, civil rights, and foreign affairs. Observe that the mean district by district legislator-constituency difference (here measured by the mean of all those identifying with the legislator's party) ranges from 4.34 for foreign policy to 2.92 for civil rights.[14] However, as the values in the second column indicate, this discrepancy is significantly reduced if viewed in terms of aggregate differences. For social welfare the overall legislator-constituency difference is reduced by about 25 percent of the average of dyadic differences, while on foreign affairs—which shows the largest amount of dyadic misrepresentation—representation is improved 40 percent in the aggregate. The latter finding is particularly important since the near-zero constituency—Congress members' correlation in foreign affairs reported by Miller and Stokes would lead us to expect extensive misrepresentation, when in fact Congress as an

Table 3. Dyadic Versus Collective Representation, by Issue Domain, 1958 (District Majorities)

Issue Domain	$\bar{X}$ Difference between Member of Congress and Partisan Constituents on Dyadic Basis	Difference between All Legislators and All Districts in the Aggregate	"Improvement" of Collective over Dyadic*
Social Welfare	3.26	2.45	24.8%
Civil Rights	2.92	2.41	17.5%
Foreign Affairs	4.34	2.62	39.6%

Source: Survey Research Center, University of Michigan.

*Improvement is calculated by subtracting the aggregate from the dyadic scores and dividing the result by the dyadic score.

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[13] That congressional scores may range 0 to 9 while constituency scores range from 0 to 3 is no problem with a correlation coefficient that "standardizes" these scores. We cannot, however, use standard scores since these would (by definition) yield means of zero in column 2 of Table 3, thus rendering our analysis statistically nonsensical. We should also acknowledge that the mean or any other measure of central tendency can be a poor indicator of what is to be represented, given the wide variety of opinion distribution in a district. It certainly seems politically important whether or not the distribution is bell-shaped or U-shaped. These and several other questions dealing with the measurement of constituency are considered in greater detail in Robert Weissberg, "Some Issues in the Analysis of Legislator Constituency Policy Agreement," unpublished mimeographed paper.

[14] Our use of legislators' party identifiers as "constituency opinion" (as opposed to, say, the entire district) derives from two considerations. First, of all the groups we could have used, those belonging to a Congress member's party seem the most relevant in terms of representational relationship. Certainly a member of Congress could not be expected to represent everyone in the district or partisans of the opposition party. In any case, the use of the score for the entire district would not change the thrust of our analysis. Second, subsequent analysis considers the theoretically important questions of district minority representation and this requires that we disaggregate district opinions.

institution is doing a better job than suggested by the very low correlation. This is not to say that dyadic representation is poor (see recent analyses by Fiorina and Kingdon, for example);[15] rather, whatever the degree of dyadic representation, collective representation is equal or better.

More generally, several politically important principles can be deduced from $\frac{\Sigma|d - R|}{N} \geq \frac{\Sigma d - \Sigma R}{N}$. First, as we have seen, extreme cases of dyadic misrepresentation (i.e., district mavericks) need not result in misrepresentation of any or all constituents. The quality of representation is in part dependent on the *distribution* of misrepresentation; the absolute level of dyadic misrepresentation is less important than how it is distributed. Imagine, for example, an extremely liberal district that happens to be represented by an extremely conservative member of Congress. If this were the only district in a political system these liberal citizens would indeed be misrepresented. But, if there also existed an extremely conservative district having an extremely liberal member of Congress, misrepresentation in the second district results in overall representation. It follows, then, that aggregate (or virtual) representation will be better than dyadic representation to the extent that dyadic misrepresentation is equally distributed on both sides of the overall constituency mean. Only if there is misrepresentation on both sides of the constituency mean can misrepresenting actions cancel each other out so if all legislators vote the same way or all violate constituency preferences in the same direction, dyadic and aggregate representation will be identical (this would be unlikely, of course, if legislators voted randomly).

This last point has important implications for the doctrine of responsible party government or, for that matter, any doctrine calling for greater legislative party cohesion. Specifically, in a legislature with two (or more) highly cohesive parties only two (or more) points in the distribution of citizen opinion will be *precisely* represented. To be sure, the point represented by the majority party may be the modal constituency issue position or the mean of all constituency issue position, but many issue positions are not literally represented in the legislature. On the other hand, let us imagine a situation consisting of two parties with different ideological centers of gravity but where some legislators can diverge substantially from the official "party line." If party deviates (i.e., those at odds with the official party position) are equally dispersed on the "right" and the "left," the mean legislative party positions may be the same as in the case of a cohesive party system. The obvious difference, however, between the two situations is that many more constituency opinions are *precisely* represented in the instance of decentralized parties. Thus, even if the legislative outcomes are identical in both instances, the less cohesive situation possesses the likely advantage of offering a greater variety of legislative spokespersons for various citizen positions.

Our arguments here are also relevant to the problem of representing minority interests in a system of single-member, first-past-the-post districts. On the long debate over the merits of proportional representation (PR) *vs.* single member districts (SMD), the representation of sizable interests without a majority in any particular district has usually been considered one of the advantages of PR and a democratic weakness of the SMD arrangement. This weakness can disappear, however, if some legislators (1) violate preferences of their own and all other constituency majorities and (2) if these violations are equally dispersed around the mean of all constituency majorities. Hence, so long as extreme misrepresentation in one direction is balanced by equal misrepresentation in the opposite direction, extreme preferences not dominating any particular electoral district (e.g., the Klu Klux Klan race position) can be represented without overall constituency preferences being violated. We are *not* claiming that such balanced misrepresentation need result in PR-like equal representation of all public opinion in exact proportion to its popular strength; rather, in the aggregate, the violation of constituency preferences can produce a spokesperson for minority preferences while not resulting in the legislature in general misrepresenting constituency majorities.

To assess the representation of district minorities via virtual representation, using the Miller-Stokes data we have computed the mean district-by-district difference between Congress members and members of the minority party and the overall difference between all district minorities and all members of Congress in the aggregate. These differences for the issues of social welfare, civil rights, and foreign policy are presented in Table 4. Not surprisingly, the mean pairwise gap between the Congress member and the mean of minority-party identifiers is larger than the gap for majority-party identifiers depicted in Table 3. This larger gap does not indicate, however, that district minorities receive poorer congressional representation than constituency majorities. Indeed, comparison of the aggregate legislator-minority constituency differences in Table 4 with the comparable figures in Table 3 shows that in civil rights and social welfare policy, the constituency minorities receive *better* overall representation than do majorities. For example, on civil rights issues and the mean aggregate difference for the majority is 2.41, while for the minority it is 2.20. In short, "minority representation" in a single-member district appears to be a problem only if viewed on a district-by-district basis. Viewing the legislature as an institution representing a collectivity does not mean, of course, that all minority preferences are, therefore, proportionately represented; rather, our analysis suggests that in the aggregate a district minority may receive no better representation than constituency majorities.

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[15] Morris P. Fiorina, "Constituency Influence: A Generalized Model and Its Implications for Statistical Studies of Roll-Call Behavior," *Political Methodology*, 2 (1975), pp. 249–66; and John W. Kingdon, *Congressmen's Voting Decisions* (New York: Harper and Row), Ch. 2.

The final point in our analysis of dyadic versus collective representation concerns the number of legislative districts. If all legislators perfectly followed district opinion the number of districts would, obviously, be irrelevant for the improvement of dyadic representation, i.e., $\frac{\Sigma|d-R|}{N}$ would always equal $\frac{\Sigma d - \Sigma R}{N}$. On the other hand, as legislators diverge from constituency preferences it seems clear that, assuming nonsystematic divergences, the larger the number of districts, the greater the likelihood that the distribution of extreme misrepresentation will be symmetrical, so the improvement due to the aggregating of opposing misrepresentation will likewise increase. In other words, the likelihood of, say, extreme liberal misrepresentation being canceled out by extreme conservative misrepresentation is greater in a large assembly than in one of, say, 5 or 10 members. To illustrate this principle we have computed the ratios of dyadic to collective representation for random samples of the districts in the Miller-Stokes study (Table 5). We should add that since Miller and Stokes do not collect data on all Congress members, even the 1.00 sample is not the population of the House of Representatives; quite likely the full 435 cases as opposed to the 146 here would show even greater improvement of representation due to aggregation.

As expected, the greater the number of legislators, the more representative the institution as a whole. For the 1 of 10 sample (i.e., about 15 cases), dyadic and aggregate representations are nearly identical in social welfare and civil rights. Aggregate representation shows a sizable gain in the .5 sample (about 75 districts) where the figures are quite close to those for the entire set of districts. Though we cannot say with any certainty what the dyadic to aggregate ratio would be for all 435 cases, our figures here suggest that any improvement would likely be quite modest (though a very large assembly would likely give voice to many more points of view).

Political Control and Representative Government

Representation, at least as considered in the context of elections, is usually viewed as a consequence of political control (i.e., the ability of citizens to remove undesirable officials).[16] By means of popular, direct elec-

Table 4. Dyadic Versus Collective Representatives, by Issue Domain, 1958 (District Minorities)

Issue Domain	$\bar{X}$ Difference between Member of Congress and Opposing Partisan on Dyadic Basis	Difference between All Legislators and All District Minorities in the Aggregate	"Improvement" of Collective over Dyadic*
Social Welfare	3.41	2.38	30.2%
Civil Rights	3.21	2.20	31.5%
Foreign Affairs	4.79	2.70	43.6%

Source: SRC, University of Michigan.

*See Table 3 for computation of "Improvement."

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[16] Since concepts like "electoral control" are always troublesome, it might be useful to reiterate that

Table 5. Legislatures Size and Improvement of Collective Representation by Issue by Issue Area, 1958

	Sample		
Issue Domain	.10	.50	1.00
Social Welfare	.96*	.77	.75
Civil Rights	.98	.86	.82
Foreign Affairs	.86	.65	.60

Source: SRC, University of Michigan.

*Entries are the ratios of collective to dyadic legislator-district differences; the lower the number, the greater the improvement due to collective representation.

tions, the argument goes, citizens select officials, and this selection process will lead to or is necessary, though not sufficient for, the representation of citizen preferences (or interests) in policy making. Conversely, it is assumed that leaders free from citizen control (whether by being "safe" electorally or being appointed) will feel freer to ignore constituency demands. No doubt this logic of political control encouraging representation accounts for the often-sought relationship between electoral competition (a supposed measure of control) and various policy responses that are viewed as responses to citizen preferences. There is, however, no *logical* inter-connection between control of an official via the electoral capacity to terminate tenure and representatives. Perhaps the major reason why electoral control and representation appear so intimately linked is the plausibility of the connecting supposition, namely, that leaders will seek to please (i.e., represent) those who control their fate, and the clearer the control, the greater the effort to please.

We have seen, however, that representation of opinion can occur apart from an electoral control relationship. Indeed, it is *likely* that citizens will be best represented by someone with whom they have no electoral relationship, and even under random voting constituency majorities would frequently be followed. What we shall do here is explore the relationship between the representation of citizen opinion by legislators collectively and citizen electoral control of legislators. We shall show that if one accepts the notion of collective, as opposed to dyadic, representation, high levels of citizen control are not only unnecessary but may be in fact a hindrance to accurate representation.

If one's constitutional goal were simply the best institutional representation of mass opinions, the optimal solution is clearly a random sample of about 1500 citizens who would thus accurately "represent" the population.[17] Alternatively, the population could be subdivided into relatively homogeneous groups and citizens would be selected randomly from within each of these groups (as in quota samples). Neither of these random-sample solutions would, of course, allow citizen intervention through voting or geographically defined units such as states. Nevertheless, the model of a legislature by random sample need not be abandoned if we introduce the requirements of citizen choice and election districts defined by criteria other than homogeneity of opinion. It is possible to approximate a random sample while still maintaining existing constitutional requirements for the electoral system. The most obvious mechanism of approximation would be to increase the number of legislators. If we had a legislature of 42,000 members (i.e., each legislator represented 5000 citizens) we would expect the legislature by virtue of its sheer size to be a much better microcosm of the population than where the ratio were, say, 1 to 500,000. One only has to compare the racial, ethnic, and sexual composition of the U.S. House of Representatives with that of the Senate to see how a large institution is a "better" sample than a small institution. Of course, each citizen has an electoral relationship, and thus a possibility of electoral control, with only 1 of 435 Congress members compared to 2 of 100 senators.

What if huge assemblies of citizens were unacceptable or impractical? On the basis of the random sample model we would suggest that collective representation would be increased if election districts were as homo-

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all we mean by "electoral control" is the opportunity available to citizens to determine who shall govern them. The maximum amount of control conceivable would be a political system of one official and one voter. Adding a second voter reduces the first voter's amount of control. Electoral control over one's government would be even further reduced if a second official were added or one were not allowed to participate in the selection process.

[17] Though our main purpose in raising the idea of random selection is to have a baseline model of perfect representation, lotteries for office-holding have a long theoretical and practical tradition. The random-selection principle has appeared in ancient Greece, medieval Spain, and modern jury selection. See Dennis C. Mueller, Robert D. Tollison, and Thomas D. Willett, "Representative Democracy Via Random Selection," *Public Choice*, 12 (Spring 1972), 57–68.

geneous as possible in their preferences, despite geographical constraints. In actual practice this districting would have to be done not on the basis of opinion data, but on more apparent criteria such as economic base, income levels, racial and ethnic composition, or other characteristics associated with sets of predictable political preferences. Under such circumstances, the odds that each legislator is typical of the district would increase and, if such district homogeneity were general, the legislature would approximate a quota sample of the population. Needless to say, such criteria could very well lead to oddly shaped "gerrymandered" districts that would likely violate current reapportionment standards established by the federal courts.

A legislature composed of members chosen on the basis of quota-sample criteria for districting would provide representation on both a collective and a dyadic basis. That is, while citizens as a whole are more accurately represented by this legislature collectively than they would be if districts were diverse, it is also true that each citizen within the dominant district group probably receives the most accurate representation from his or her "own" legislator. Hence, at least at first glance, the relationship between the representation of one's opinions and the voting decision is restored. Nevertheless, it is also likely that where homogeneity of opinion prevails, the amount of electoral competition will be very low. It is difficult to imagine a well-developed two-party system and close elections where a strong consensus exists on policy preferences. Even if the legislator in such a homogeneous district did not share the district's opinion on the salient issues, it would seem unlikely that this legislator would openly advocate a contrary view in order to give voters a real choice.[18] In short, if electoral control and representation are linked, the control is likely to be more nominal than threatening, given safe electoral margins and the lack of an organized opposition.

In making the argument that the dyadic electoral control relationship between citizen and legislator may not be all that important for policy representation, we are not claiming that such a relationship is inconsequential. To describe the nature of collective representation is not to devalue dyadic representation. Obviously, regardless of collective actions, a legislator is always electorally accountable to a constituency. This accountability may be imperfect, but its existence is not trivial politically. To appreciate this, one should consider the possibility of nonelected leaders who could not be removed regardless of popular objections. Clearly, such accountability is central to notions of democratic control of leaders by citizens as well as some degree of policy control. Moreover, apart from overall policy representation, purely dyadic representation may very well provide psychological benefits to citizens who may feel represented by their particular legislators regardless of the source of representation (or at least enjoy all the attention at election time).

Implications and Speculations

Collective, as opposed to dyadic, representation is obviously both historically justifiable and politically possible. We have also suggested that citizen preferences can indeed be represented collectively even if particular legislators ignore their constituencies. What we do not know is whether citizens relate to legislators dyadically or as mere components of a collective body. Let us momentarily assume that citizens (unlike most political scientists) are less interested in dyadic legislature relationships than collective representation. If this were true, we would make the following predictions about citizen behavior.

First, given that voting for or against 1 of 435 legislators is unlikely to affect greatly the representation one receives, a lack of citizen concern and involvement in legislative elections is probably understandable (though citizens do not necessarily engage in the requisite calculus). After all, it could be argued that it makes little sense to get involved in improving one's representation when one cannot even vote on one's best representative and when one's vote, even if absolutely crucial in one's district, affects only a small fraction of the representative institution. Under such conditions, high involvement would be worthwhile only if legislative benefits could be gained from one's particular legislator (e.g., a private bill). If we assume that people are interested in what they can affect, we would also predict that citizen involvement would covary with the size of representative institutions. Specifically, the fewer the legislators, the greater the impact of each legislator on representation, so turnout should be greater in elections for small legislatures. The irony of this relationship is, of course, that low-involvement-generating large institutions are more likely to be representative of citizen preferences than the high-interest-generating smaller institutions.

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[18] Some interesting data on district safeness and accurate representation are presented in Warren E. Miller, "Majority Rule and the Representative System of Government," in *Cleavages, Ideologies and the Party Systems*, ed. Erik Allardt and Yrjö Littunen (Helsinki: Academic Bookstore, 1964), 343–76; also see Morris P. Fiorina, *Representatives, Roll Calls, and Constituencies*, pp. 90–100.

A second set of predictions based on collective representation concerns citizen satisfaction with the performance of individual legislators. It has been commonplace to observe that, barring extreme misrepresentation on an especially salient issue (e.g., a southern member of Congress voting pro-civil rights during the 1950s and 60s), most legislators can get away with ignoring district opinion without electoral retribution.[19] This absence of retribution might appear to be an odd situation until one realizes that legislator inattention does not necessarily mean that either the district majority or a particular individual's opinion goes unrepresented by the legislature as a whole. Hence, we would guess that citizen satisfaction with legislators could very well be independent of satisfaction with policy outcomes in general. Overall legislative performance (even if the legislature itself were not highly evaluated) would thus "explain" the survival of legislators who do not perform particularly well in representing their districts. On the other hand, the same reasoning would account for why legislators doing a good job of representing constituency opinions sometimes meet defeat if things as a whole go badly.[20]

Our analysis of collective representation would also predict a less obvious relationship between citizen satisfaction and powerlessness. Discussions of political power usually treat power as an instrumental value. We have argued—both in our discussion of random voting and the representation electoral control nexus—that citizens can receive benefits without holding much or any electoral power over legislators. Indeed, the absence of some coercive mechanisms such as a strong two-party system, close elections, may be associated with accurate representation. The fact that citizens still receive benefits (i.e., representation) under poor control situations, and even when they can affect only a very small portion of a legislature's composition, may make citizen toleration (if not satisfaction) with these "poor" conditions quite reasonable. This logic would seem particularly relevant for district and national minorities who would get "shut out" completely if legislators slavishly followed district majorities. No doubt notorious House iconoclasts like H. R. Gross or Vito Marcantonio represented significant numbers of otherwise unrepresented citizens while probably misrepresenting their district majority.

The benefits of collective representation might also make more plausible widespread public indifference towards the idea of responsible party government or other schemes involving greater legislative cohesion.[21] The present system with its complement of mavericks and party deviates does give expression, no doubt, to preferences that might very well be excluded in a system of disciplined, coherent legislative parties. To a certain extent the existing system gives citizens the best of both worlds: reasonably accurate representation (even for district minorities) and legislators who are willing to perform narrow constituency services. Hence, to ask citizens to give up the such particularistic benefits as, for example, legislative intervention in the bureaucracy, for the promise of better representation when existing representation is probably substantial (though not perfect), is to make an easily refusable offer. Such an offer of greater party discipline might be more attractive if (1) citizens knew the true distributions of preferences in the population; (2) if popular majorities were systematically violated in lawmaking by these undisciplined lawmakers, and (3) the misrepresented citizens believed that responsible parties, as opposed to "better" Congress members, would rectify the misrepresentation. Obviously, these requirements are quite poorly met. In short, given the reasonable amount of legislative success under present conditions, a drastic change lacks appeal.

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[19] See, for example, Dexter, "The Representative and His District." However, also see n. 13.

[20] Corroborating evidence on the vulnerability of individual Congress members to national economic events is offered in Gerald H. Kramer, "Short-Term Fluctuations in U.S. Voting Behavior, 1896–1964," *American Political Science Review*, 65 (March 1971), 131–43. Data on how particular members of Congress can be "innocent victims" of economic conditions and presidential popularity are presented in Edward Tufte, "Determinants of the Outcomes of Midterm Congressional Elections," *American Political Science Review*, 69 (September 1975), 812–26. Actually, the relationship between evaluations of one's own Congress member and the institution of Congress is more complicated than suggested here. Fenno's recent observations on citizens liking their own Congress members while holding a low opinion of Congress as an institution supports our contention that such evaluations can be independent of each other. What is troublesome is why citizens should prefer members of Congress to Congress as a whole when the latter is likely to be more representative than the former. Clearly, as Fenno points out, much more is involved in these evaluations than policy satisfaction. See Richard F. Fenno, Jr., "If, as Ralph Nader Says, Congress Is 'the Broken Branch,' How Come We Love Our Congressmen So Much?" in *Congress in Change*, ed. Norman J. Ornstein (New York: Praeger, 1975), pp. 277–87.

[21] Jack Dennis, "Support for the Party System by the Mass Public," *American Political Science Review*, 60 (September 1966), 600–15.

Summary and Conclusions

The major points of our discussion can be summarized as follows:

(1) If we define the worst possible conditions of representation as random legislative voting, under such conditions a majority of constituencies will still be represented about half the time, and even when this majority is violated, the legislative vote will usually be fairly close. Given citizens' ignorance of true majorities in the population and the use of broad estimates, citizens will probably perceive less blatant misrepresentation than is suggested by low correlations of the Miller-Stokes type.

(2) Collective representation will never be worse than dyadic representation. If individual legislators are "free" to deviate from district opinion it is likely that deviations will approach normality and the institution as a whole will be more representative of national opinion than the average legislator is representative of district opinion.

(3) Collective representation also appears to solve the troublesome theoretical problem of how minorities are to be represented in a system of single-member districts with first-past-the-post elections. As long as extreme deviations from district majorities are "canceled out" by opposing deviations, both the public generally and district minorities can be given representation.

(4) Electoral control is not a logical prerequisite to accurate representation. Indeed, it appears unlikely that one's "best" representation will come from the individual one votes for (or against). It also appears likely that by reducing the proportion of a legislature a citizen can choose by increasing the number of legislators, and by reducing electoral competition by creating homogeneous districts, the accuracy of representation will be improved.

(5) Finally, if we accept collective representation as meaningful for citizens, several somewhat puzzling attitudes and kinds of behavior become more understandable. Among these are citizen apathy towards legislative elections, willingness to tolerate unresponsive legislators from their districts, and a distaste for greater legislative party cohesion.

Lest our arguments be misunderstood, we should also add:

(1) We have *not* claimed that dyadic representation is unimportant. Obviously, as Mayhew and many others acknowledge, what Congress members do for their constituents is highly relevant for both their own careers and constituent voting.[22] We are not trying to replace the study of dyadic representation with the study of collective representation; collective representation exists in addition to dyadic representation.

(2) We have *not* argued that collective representation is accurate representation. Our argument is that collective representation is likely to be *more* accurate, not perfectly accurate. Unfortunately, the Miller and Stokes data as we employ them do not allow statements about the absolute degree of representation.

(3) We do *not* state that elections lead to misrepresentation. We claim that it is possible, even quite likely, that representation of citizen preferences will occur independently of an electoral connection between member of Congress and a constituent. Elections are not irrelevant to the faithfulness of representation, but they are not the only determining factor.

Some final observations on the study of representation are appropriate. As we have previously indicated, the model of dyadic representation has completely dominated contemporary research. This is true whether the data are opinions or constituents or socioeconomic characteristics of the district. Given both our constitutional order, which was never designed to make legislators into perfect mirrors of popular opinion, and the absence of unbiased communication channels between citizens and leaders, the search for extensive dyadic agreement may be the search for the impossible. It may be impossible for one legislator to represent 400,000 people with any degree of accuracy; it may, however, be possible for 435 legislators to represent more accurately the opinions of 220,000,000 citizens. To be sure, whether or not a particular legislator follows his or her constituency is an important question, but this question is not necessarily the most appropriate one if we ask, "Do representatives represent?"

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[22] David R. Mayhew, *Congress: The Electoral Connection* (New Haven: Yale University Press, 1974).

Policy Making in Congressional Committees: The Impact of "Environmental" Factors *

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It is well established that various characteristics of a congressional committee may shape and, to an extent, standardize its handling of a range of policy areas. This article, however, examines the substantial variations *that most committees continue to display as they move from issue to issue. Why is it that the House Commerce Committee, for example, approaches issues like major-disease research, health care delivery, cable television development, and power plant siting in such radically different ways? What determines the incentives of legislators to invest time and effort in a policy area, and to orient themselves toward broad "public" interests, once involved? Focusing on the House and Senate Commerce Committees, the article takes as its data base patterns of legislative action and inaction in major areas of the committees' jurisdiction during 1969–1974. Particular attention is given to how perceived levels of group conflict and of public salience affect a legislator's orientation toward a policy area, and to the level and direction of executive involvement as an intervening variable of major significance. High-salience, low-conflict areas (e.g., health research) generally offer the strongest incentives to congressional involvement, and low-salience, high-conflict areas (e.g., communications regulation) the weakest, while mixed cases (e.g., a high-salience, high-conflict area like health care delivery) display considerable variability. Issues may change considerably with time in their perceived levels of salience and conflict, and legislators can influence, as well as respond to, such changes. Congressional initiatives often respond to perceived "neglect" on the part of the executive, or disagreement with the content of its moves. But particularly in high-conflict areas, congressional activists will often find that their success requires executive involvement.*

Recent students of the United States Congress have premised a great deal of their work on Woodrow Wilson's 1885 observation that "Congress in its committee-rooms is Congress at work."[1] Single-committee and comparative studies have reconfirmed the *centrality* of committees to the life and work of both chambers, have delineated some of the dimensions along which committees significantly and systematically *differ*, and have begun to elucidate the *linkages* between various characteristics of committees and the *policy* Congress produces (or fails to produce) in the areas under their care.[2] The volume and often the content of congressional output in one or another policy area is greatly affected by the goals and orientations members and aides of relevant committees bring to their jobs, the skills and leadership styles of chairmen, and the leverage and resources that various subcommittee arrangements afford individual members. But if there is no escaping the centrality of committee-related variables to any account of congressional policy making, it is also obvious that the committees of Congress, like the institution as a whole, are increasingly buffeted by powerful forces in their policy-making environment, organized interests and governmental agencies chief among them. Thus any explanation of a committee's policy role not only must include an examination of its "internal" goals, norms, structures, and processes, but also must take account of the environment in which the committee subsists and of the forces impinging upon its operation.

This article will examine the impact of environmental forces on the policy-making efforts and capacities of the House and Senate Commerce Committees. It is frequently striking to note how certain characteristics of a congressional committee shape and, to an extent, standardize its behavior over a range of policy areas.[3] Our purpose here, however, is to con-

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*The author wishes to thank Terry Brooks and Susan Skillen for their help in preparing and circulating successive drafts of this manuscript, and Steve Grant and Steve Haeberle for research assistance. For their valuable comments on an earlier version, thanks are due Jerry Hough, David Mayhew, Harris Miller, William Mishler, Brad Pigott, Lester Salamon, and Philip Williams.

[1] *Congressional Government* (New York: Houghton Mifflin, 1913), p. 79.

[2] See especially John F. Manley, *The Politics of Finance: The House Committee on Ways and Means* (Boston: Little, Brown, 1970); David E. Price, *Who Makes the Laws?* (Cambridge: Schenkman, 1972); and Richard F. Fenno, Jr., *Congressmen in Committees* (Boston: Little, Brown, 1973).

[3] See Price, *Who Makes the Laws?*, pp. 9–10, 311–22, and *passim*.

centrate on *variations* in the behavior of single committees from area to area and on the extrinsic factors related to these variations. Examining the work of only one committee in each house enables us to control for committee-related variables which become confounded with these environmental factors in shaping policy outcomes.

It is not self-evident how best to construe the policy-making "environment." What sort of conceptualization and analysis is most likely to lead to a plausible account of the range of observed behavior? Theodore Lowi poses one possibility: a decentralized, bargaining assembly such as the U.S. Congress will be able (and inclined) to assume policy leadership on some *types* of issues more than others. Thus, one might expect to discern differing degrees of congressional initiative and capability with respect to policies whose "impact on society" promises to be "distributive," "regulatory," or "redistributive," respectively. But to link the differentiation of policy type to the explanation of congressional behavior and of the executive-congressional division of labor as Lowi proposes, one must make several further assumptions: that legislators find conflict or deadlock "costly," for example, and that there is an implicit consensus in each branch to grant some degree of deference to the other in the sorts of policy leadership that it does "best." This being the case, one might best look at variables such as environmental conflict and degree of executive involvement directly, particularly since Lowi's policy types prove a blunt instrument for distinguishing among policy areas which congressional committees handle quite differently.[4]

Richard Fenno offers a second possibility, focusing not on "policy characteristics" but on the *specific* "outsiders that take an interest in [policy] and, hence, in the committee." He thus compares six House committees according to which "outsiders"—members of the parent House, the executive branch, clientele groups, or the party leadership—have the most intense interest in committee affairs and the greatest capacity for pressure and influence. He is able to account for a range of congressional decisions in terms of the influence exerted by these "policy coalitions," with the "goals" which members bring to one committee or another as his second independent variable. But his capacity to explain differing levels of congressional interest and assertiveness is reduced by his conceptualization of environmental forces mainly in terms of the "constraints" they place on the pursuit of these independently existing "member goals." Fenno thinks of the "environment" primarily as groups of interested outsiders who must be *adapted to.* But to understand the impact of environmental factors on policy makers, one must also think of their settings as fields of incentives, opportunities, and constraints that shape their priorities and strategies. Exactly *which* groups of outsiders are able to command their attention may be less important than certain general characteristics of these groups and their policy terrain which determine how "profitable" policy makers are likely to regard various modes of prospective involvement.[5]

This essay will assume that legislators are rational-economic persons who maximize their political "profits," and that committee outputs are a function of individual initiatives.[6] Neither assumption, as will be noted, is an entirely reliable guide to congressional policy making. But for the slice of congressional life we wish to examine—the remarkable differences in the level and "quality" of legislative activity across a single committee's jurisdiction—individualistic, maximizing assumptions about motivation are of considerable help in making sense of the data. We assume that legislators ultimately wish to maximize their electoral strength and, intermediately, to gain the favor of those proximate "outsiders" with whom their committees must deal. Our method, however, will not be to deduce probable behavior from these premises, but to look concretely at how Commerce Committee members handle (and talk about) a range of policy areas and to abstract certain environmental factors which help explain the patterns observed. We will, in particular, focus on how perceived levels of group *conflict* and of public *salience* affect a legislator's orientation toward a policy area—and on the level and direction of executive involvement as an intervening variable of major significance.

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[4] See Theodore J. Lowi, "American Business, Public Policy, Case Studies, and Political Theory," *World Politics,* 16 (July 1964), 677–715; and Price, *Who Makes the Laws?,* pp. 323–27.

[5] See Fenno, *Congressmen in Committees,* Ch. 2; and the review of this book by David E. Price, *American Political Science Review,* 711 (June 1977), 701–04.

[6] For a characterization of the "economic" approach to the study of politics, see Brian Barry, *Sociologists, Economists and Democracy* (London: Collier-Macmillan, 1970), Ch. 1. For a suggestive application to congressional behavior, see David R. Mayhew, *Congress: The Electoral Connection* (New Haven: Yale University Press, 1974), pp. 1–9 and *passim.* Seymour Scher takes a similar approach in his explication of "the conditions under which committees become involved in agency oversight": "Conditions for Legislative Control," *Journal of Politics,* 25 (August 1963), 526–51.

Policy Making on the Commerce Committees: "Clientele-Centered" Areas

A preliminary view of the Commerce Committees' levels of activity during the period covered by this study (1969–1974) is provided in Table 1. Their jurisdictions do not coincide perfectly: House Commerce's responsibilities in the energy and environmental fields are broader, and it shares railway labor, securities and exchanges, and public health with committees other than Senate Commerce; the Senate but not the House committee handles merchant marine, fisheries, and oceanography.[7] The indicators of activity chosen do not measure precisely the same thing. Bills restricted in scope or impact (or initiated and developed elsewhere) may be reported by a committee but require little hearing time. On the other hand, the desire of legislators to publicize an issue, to advertise their own leadership, or to convince the groups pushing a bill of their industry and good will can produce extensive hearing activity even when the prospects (and sometimes the desire) for ultimate passage are remote.

Our primary interest is in how *differences* among the environmental forces operating in each of these policy areas are reflected in the level and content of committee output. But certain general characteristics distinguish the Commerce Committees' environment from that of many other committees. In the first place, the committees do not deal extensively with "peak associations" seeking "redistributive" policies.[8] The "distributive" and "regulatory" measures with which the committees characteristically work involve organized labor only peripherally and seldom raise the spectre of a massive reallocation of national resources. Business interests generally seek industry-specific benefits or exemptions, although this pattern is changing as consumer and environmental initiatives become more ambitious and more general in effect. The result is an environment in which group conflict is, while sometimes intense, generally limited in scope; one need only to look at the setting of the Education and Labor or Finance Committee to appreciate the difference.

Table 1. Policy Areas Ranked According to Days of Hearings Held, Public Bills Reported, and Public Laws Approved, 91st-93rd Congresses, 1969–1974 (Nominations excluded)

Senate Commerce Committee				House Commerce Committee			
Area	Days of Hearings	Bills Reported	Public Laws	Area	Days of Hearings	Bills Reported	Public Laws
Consumer	159	33	17	Health	206	60	47
Surface transportation	121	32	18	Consumer	139	15	15
Environment	103	11	4	Surface transportation	120	20	18
Aviation	77	18	10	Communications	89	17	12
Communications	70	21	12	Securities and exchanges	86	7	6
Energy/power	55	6	4	Aviation	67	12	10
Oceanography	51	12	12	Energy/power	61	9	7
Merchant marine	47	40	37	Environment	59	11	9
Foreign commerce/ tourism	28	11	4	Railway labor	36	10	10
Commercial fisheries	22	21	17	Foreign commerce/ tourism	6	3	3
Fish and wildlife conservation	22	26	23				
Coast Guard	7	14	14				

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[7] The House committee's jurisdiction was appreciably altered by the Committee Reform Amendments of 1974, which took effect in the 94th Congress. Civil aviation and surface transportation (except railroads) were transferred to the new Public Works and Transportation Committee. Commerce's health jurisdiction was expanded, but Ways and Means retained its claim to health-care programs financed by payroll taxes. The Senate's Committee System Reorganization Amendments of 1977 gave Commerce substantial new science and technology jurisdiction but further reduced its claims on environmental policy.

[8] See Lowi, "American Business."

A second and related characteristic of the Commerce environment is the relative absence of high-intensity executive branch and party involvement. As we shall see, the executive role is hardly a passive one, but Commerce Committee matters are rarely at the top of the agenda of the administration or of the parties' congressional leaders. Committee members' initiatives in consumer affairs, oceanography, environmental policy, health, merchant marine, and aviation often represent an attempt to counter declared administration policies and priorities, but they just as often—and this applies to the Johnson as well as the Nixon-Ford years—take place under conditions of apparent administration indecisiveness and neglect. This means that the pressures and positions of party and presidency have a less marked (and less homogenizing) impact on the policy-making efforts of the Commerce Committees than they do on many others. Consequently, other environmental factors—and variations among them from area to area—probably are more important in shaping Commerce Committee behavior than they are on committees where the "policy coalitions" facing the group are more monolithic.[9] We turn first to policy areas where the main forces with which legislators must contend are organized and relatively narrowly based "clientele" groups.

Communications. The clientele groups confronting the Commerce Committees differ considerably in their degree of intramural conflict and in the executive and agency support they enjoy. Communications is an area notable for both its high potential for conflict and its relatively high degree of executive involvement. The conflict potential is only partially attributable to divergencies among the groups trying to influence broadcast policy, for the preeminence of the commercial broadcasters renders communications less effectively pluralistic than most of Commerce's clientele-centered areas. But the National Association of Broadcasters and its local affiliates are poised to make trouble for legislators who would challenge the comfortable regulatory situation they enjoy. They have a few things they want from Congress—an extended license period, for example, and scuttling the "fairness doctrine"—but their main interest is in governmental *inaction* on such fronts as license-renewal criteria and cable and public television development.

The dominant agencies in the area have been the Federal Communications Commission and, from 1970 until most of its functions were transferred to the Commerce Department in 1977, the White House Office of Telecommunications Policy. The OTP was mainly identified in its early years with the Nixon administration's criticisms of public television and network news and the White House's intervention in FCC deliberations regarding cable television. But it also developed proposals for the long-range financing of public television and the eventual deregulation of CATV. On these and other matters, FCC, OTP, and broadcasting industry perspectives have sometimes diverged, but the agencies have generally been close to the broadcasters in their working relationships and policy preferences, and they no more than the industry have desired to stimulate major congressional policy initiatives.

Legislators thus find themselves frequently preempted in the communications field and perceive new policy departures and critical oversight as distinctly unwelcome. Nor does the environment provide sufficient positive incentives to override these constraints for most legislators. The cable operators generally cannot match the resources of the commercial broadcasters and neither, of course, can the scattered groups interested in such matters as public television, children's programming, and facilitating challenges to license-holders. Many broadcasting issues are arcane and dimly perceived by the public. The incentives to undertake reform efforts are thus rather low and the possible penalties for involvement quite high. "I suppose public sentiment would generally be in favor of loosening restrictions on cable," reflects one veteran member of the House Communications Subcommittee, "but you're just not going to see many congressmen stepping out in this area."[10] This is not to say that a skillful legislator could not manipulate the situation to advantage—dramatizing issues, appealing "over the heads" of the broadcasters to an incipient "public," mobilizing reform-minded groups to underwrite such efforts. But it would be an uphill fight, and not without risks.

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[9] Compare Richard Fenno's description of the settings of the House Appropriations, Ways and Means, Foreign Relations, and Education and Labor Committees: *Congressmen in Committees*, Ch. 2.

[10] Quotations without citations are taken from transcripts, as nearly verbatim as possible, drawn up from memory immediately following interviews with a range of members, aides, lobbyists, and agency and commission personnel. The basic set of interviews was conducted in 1972 by the team studying the Commerce Committees for the Congress Project (see David E. Price, et al., *The Commerce Committees* [New York: Grossman, 1975], p. 5). These were supplemented by some two dozen in-depth interviews, more specifically focused on the topic of the present study, conducted by the author in 1973 and 1975.

In general, the Commerce Committees have followed the course of inaction and deference which this configuration of environmental forces would lead one to predict. Of the twelve communications bills reported by the committees which became law during the 91st–93rd Congresses, six dealt with such matters as COMSAT board election procedures and the conditions under which amateur radio licenses could be granted to aliens; they were quite limited in scope and, in most cases, were requested by the FCC. One public law (plus one vetoed bill and two Senate bills that died in the House) concerned the regulation of campaign broadcasting and thus involved party and presidential forces external to the environmental matrix we have delineated. One bill dealt with a matter that many legislators saw as potentially of high public appeal: ending the local blackout of sold-out professional sports events. The four remaining public laws and a second vetoed bill were public broadcasting authorizations. All of these were quite modest measures; none authorized funds for a period of more than two years or substantially altered the statute under which public broadcasting operates.[11]

Conspicuously absent from this list are the long-debated proposals which would give the regulation of cable television a statutory base. What former FCC Commissioner Nicholas Johnson once called "the never-ending saga of cable television" illustrates nicely the disincentives and constraints facing legislators in the communications area. Senate Communications Subcommittee hearings in 1971 helped smoke out the FCC's plans to loosen the restrictions which had "frozen" cable development since 1966. But it was OTP that orchestrated the ensuing "consensus agreement" between the commission and the broadcast and copyright interests that objected to its proposals; legislators had almost nothing to do with the policy outcome.[12] This reluctance to come to cable television's defense can largely be attributed to the broadcasters' political potency. At the same time, the crosscurrents were strong enough to make an outright endorsement of NAB objectives seem perilous as well. As a House Commerce aide points out, the FCC-OTP episode was one of a sequence of "three shots Congress has had at cable, each one aborted.[13] . . . A mutual veto power has developed between the broadcasters and cable. Congressmen just aren't going to stick their necks out." The OTP's 1974 cable deregulation initiative helped persuade House Subcommittee Chairman Torbert MacDonald to "seize a nettle most members of Congress [had] sought to avoid"; he ordered a staff study, which came out close to the OTP position. But by the same token, when in 1976 it was announced that the White House would not be sending up a deregulation bill as anticipated, it could confidently be predicted that congressional action was unlikely.[14]

Members of the communications subcommittees have undertaken a few publicizing and investigative ventures—Pastore's 1972 hearings on television violence, for example, and a series of House hearings on "films and broadcasts demeaning ethnic, racial, or religious groups" —which no doubt reflect calculations of broader political appeal. Similarly, while some members have sought industry favor by seeking to repeal equal-time requirements, to eliminate pay television, to loosen standards for license renewals, and so forth, others have been deterred from supporting such measures by the prospect of appearing uncritically sympathetic.[15] But such symbolic gestures and defensive scruples hardly add up to a "public" orientation, much less to independent policy entrepreneurship.[16] The basic pattern we have delineated still holds: a regulatory situation highly favorable to the broadcasters, with a matrix of incentives and constraints that places a low premium on congressional interference.

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[11] The White House in 1974 allowed OTP to send forward a long-range funding proposal long sought by CPB advocates. But various intra-industry and intracongressional conflicts delayed enactment until the 94th Congress—and then only as a five-year authorization shorn of the unusual concurrent appropriations provisions which proponents had sought (P.L. 94–192).

[12] See John Paris, "Communications," in Price, et al., *Commerce Committees*, pp. 240–51; and the wry, sometimes bitter, comments by FCC Commissioners Lee, Johnson, and Wiley on the commission's OTP-induced reversal: *Final Cable Television Decision* (Washington: Television Digest, 1972), pp. 142–52.

[13] The first episode came in 1960 when the cable forces, led by Oklahoma's Senator Robert Kerr, turned back an attempt led by Pastore and supported by commercial broadcasters to put cable under FCC regulation. The second confrontation occurred in the House in 1966 as the Commerce Committee reported but the Rules Committee killed a bill "confirming" the FCC in the jurisdiction over cable it has assumed. The commission's claims were ultimately upheld in the *Southwestern Cable* case, 329 U.S. 157 (1968).

[14] *Broadcasting*, February 2, 1976, p. 19; April 12, 1976, p. 26.

[15] On Senate Subcommittee Chairman John Pastore's caution in the license-renewal area, see Paris, "Communications," pp. 219–23. The House and Senate committees in 1974 reported bills which

Aviation. Comparable results are produced by a slightly different array of forces in aviation and surface transportation. The organized interests are more fragmented, and no one industry segment occupies a dominant position comparable to that of the commercial broadcasters. As in communications, legislators sometimes enter the policy-making fray only to cancel out one another's efforts, but the fact that the environment is more complex and the strengths of the contending interests more nearly equivalent provides strong disincentives to venture into controversial areas in the first place. There is, however, a countervailing consideration: in aviation and surface transportation, what is at stake is not merely a regulatory *modus vivendi* but also massive governmental "promotional" programs. As a House transportation aide puts it, the various transportation modes "all want champagne; they usually just disagree on the brand." This is important in creating an incentive for action as well as inaction. The fact that public knowledge and interest are perceived to be minimal makes for reduced scruples about supporting one sort of promotional package or another—though, of course, it also reduces the incentives for broader sorts of lawmaking and oversight.

Important conflicts within the aviation industry include those between commercial and general (private) aviation on airport and airways development priorities and on the distribution of the burden of "user" taxes, and between the various "levels" of scheduled carriers ("trunk," "local service" or "feeder," and air-taxis or "commuter" airlines) concerned with federal regulatory policy and the distribution of subsidies. Congressional advocates of one or another of these interests must weigh the rewards of initiative against the likelihood of opposition and conflict. Moreover, the presence of the Civil Aeronautics Board (which certifies carriers and approves rates, routes, and mergers) and the Federal Aviation Administration (which administers safety, traffic control, and airport development programs), often makes congressional initiatives appear either unpromising or superfluous. The FAA is strongly promotional in its orientation ("I'm an aviation man," declares one administrator. "My first objective is the promotion and expansion of civil aviation. If it weren't, I shouldn't have the job"), although its proposals and preferences are sometimes tempered by budgetary and other considerations as they make their way through the executive branch. The CAB has also often proved deferential to the industry—allowing, for example, the local service airlines to achieve trunkline status through its approval of routes and large-aircraft purchases[17]—although aiding one segment of the industry often means offending another and creating pressures for executive or congressional intervention.

The congressional agenda in aviation matters reveals a willingness, ordinarily, to let latent conflicts lie and to live with the regulatory status quo. Only ten aviation bills processed by the Commerce Committees became law between 1969 and 1974, and of these four were restricted in scope and relatively noncontroversial. A fifth bill aimed to alleviate the financial plight of U.S. international passenger airlines in marginal ways, but avoided what Senate Commerce Chairman Magnuson termed the "more difficult problem" of direct governmental assistance.[18] Two public laws were directed at highjacking; perceived public concern was sufficient to instigate Senate passage of two additional anti-hijacking measures and two aimed at the elimination of sonic booms,

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strengthened the presumptions in favor of incumbent license holders but which provided for only four- and three-year license terms respectively. In both cases, the bills were amended by overwhelming floor votes to include the five-year term sought by the broadcasters. The rebuffed House committee leaders subsequently dragged their feet, however, and a House-Senate conference was never convened.

[16] A considerably bolder effort was initiated by Lionel Van Deerlin, who inherited the House subcommittee from MacDonald in 1976. A former broadcaster long interested in communications policy, Van Deerlin began hearings in the 95th Congress aimed at a "basement to attic" revamping of the Communications Act. Early indications, however, were that the project had attracted little public interest, that the Senate committee was lukewarm, and that the broadcasters' attitudes ranged from suspicion to hostility; the rewrite, Van Deerlin acknowledged, would be a "longer range proposition than I first thought." See *Broadcasting*, December 12, 1977, pp. 21–22; December 19, 1977, p. 21.

[17] See George C. Eads, *The Local Service Airline Experiment* (Washington: Brookings, 1972).

[18] *Congressional Record* (daily), October 10, 1974, p. 18899.

but not to compel House agreement.

The incentives and constraints facing congressional promoters of civil aviation are clearer in the remaining three cases, bills authorizing (and amending) a major new airport and airways development program.[19] Carriers, localities, and airport operators had been pressing for more funds and better facilities for a decade, but the crosscurrents of industry and executive conflict had frustrated aviation's promoters in Congress. Action came only when the deterioration of airline service and safety reached crisis proportions and was given heightened public salience by the work slowdowns of the Professional Air Traffic Controllers Organization, protesting the inadequacy of equipment and working conditions. Familiar environmental obstacles reappeared, however.[20] The Nixon administration insisted that user taxes must be sufficient to cover costs, and opposed the financing of such frills as terminal facilities. In 1972 the president vetoed a bill which raised the airport aid authorization and increased the federal share of the matching formula. General aviation, while alarmed by overcrowded facilities and restrictive local practices designed to discourage small-plane traffic, nonetheless was wary of a program that would raise their fuel taxes while benefiting mainly the larger municipal airports. "The airlines have ordered steak and lobster," said a general aviation spokesman; "general aviation has ordered spaghetti and is getting stuck with the check." Several congressional compromises, such as the establishment of a separate development program for general aviation airports, served to conciliate the private users, if not totally to win them over.

Much less movement is visible in other areas of aviation policy. Proposals to charter and regulate commuter airlines as third-level scheduled carriers, for example, have not gotten past the hearing stage. As a Senate counsel explains:

> Senator Pearson put the bill [S 796, 92nd Congress] in as a favor, but he has never gone down the line. There are simply too many diverse interests. Some second-level [local-service] carriers are on each side.... The Pearson bill represents the position of about seven commuter lines, but there are over 100 of them; generally it's the larger ones that are behind this.... There is no "administration position"; it's impossible to formulate one.... The likely result for the near future is no action.

Restrictive CAB rulings led to Senate efforts in 1973 to loosen restrictions on charter airlines. But this too was an effort that legislators did not relish, and for which they could not have high hopes. An aide discussed the difficulties:

> The charters don't compare with the skeds [schedule airlines] in their lobbying force, the money they can spend.... Yes, [Aviation Subcommittee Chairman] Cannon and especially [full committee Chairman] Magnuson are reluctant to oppose the skeds.... Ordinarily we would rather these problems be handled by the CAB, but there has been a shift [in chairmen] there which means the charters wouldn't have a chance.

Senate Commerce Republicans opposed the bill, citing the CAB and the administration in their support of the regulatory status quo.[21] Their position prevailed in the House Committee, where key leaders were, as one observer put it, "close to the skeds."

Such "locked-in" policy areas sometimes become destabilized in a manner conducive to congressional policy initiatives, but that too is largely a matter of shifts in external environmental conditions. For example, the administration, concerned with the inflationary impact of regulatory policy, shifted its position on charter deregulation in 1975. At the same time, legislators perceived a "consumer uprising" in reaction to rising air fares and the CAB's elimination of family, youth, and other special fares. Thus Cannon and his collaborators could for the first time be bullish about their bill's prospects: "We're going to succeed. [Because of the administration shift?] It's not just that. It's the broader public concern. It's become an *issue*, for the first time I can remember, not just a fight among different classes of carriers." As it turned out, a series of CAB reversals largely mooted the issue. But the episode demonstrated clearly the relation of environmental conditions to the viability of consumer-oriented initiatives in the aviation area.

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[19] See the account in Andrew Weiner, "Aviation," in Price, et al., *Commerce Committees*, pp. 200–11.

[20] On the obstacles posed by the Eisenhower and Johnson administrations to legislators wishing to broaden and expand the Federal Airport Act of 1946, see Randall B. Ripley, "Congress Champions Aid to Airports, 1958–59," in Frederic N. Cleaveland, et al., *Congress and Urban Problems* (Washington: Brookings, 1969), pp. 20–71; and Congressional Quarterly *Almanac*, 1968, pp. 621–23. On the general-commercial aviation and city-state conflicts involved in the 1946 legislation, see Ripley, "Congress Champions Aid," pp. 24–25.

[21] The bill (S 1739, 93rd Congress) was never brought to a vote in the Senate. For an account of the crossfire among lobbyists and the desire of senators to avoid the issue ("Either way you vote you're in trouble"), see Congressional Quarterly *Weekly Report*, September 29, 1973, pp. 2593–94.

Surface Transportation. If members of the aviation subcommittees must tiptoe among contending industry factions, their promotional efforts nonetheless reflect a conviction that aviation is a transportation mode uniquely important to the nation and to their districts. As a former chief counsel testifies, most Senate Commerce members regard aviation as the "super-mode": their "philosophy is, hell, build all the airports you can." Legislators concerned with surface transportation, by contrast, must take much more explicit account of intermodal conflict, responsible as they are for motor carriers, railroads, and domestic water carriers. Cutting across these intermodal conflicts are carrier-shipper and labor-management cleavages. Legislators are thus quite content to let the Interstate Commerce Commission handle most regulatory questions—all the more since the ICC, like the FCC and CAB, is valued by the carriers for the "stability" and protection from competition it provides the industry.

The prospect of conflict does not always discourage initiatives—in fact surface transportation ranks high in the number of bills reported and the days of hearings held—but it often dooms what initiatives are taken to failure. During the 91st-93rd Congresses the landscape was littered with bills that had enough industry and congressional support to be reported by a committee or passed by one house, but ran afoul of crosscurrents of opposition short of final enactment—a proposal permitting railroads to charge freight forwarders lower rates than other shippers, a requirement that states spend 5 percent of their federal highway funds on rail-highway crossing improvement, a bill to provide loan guarantees for "certain common carriers of express" (i.e., the Railway Express Agency), and bills designed to assure western and rural shippers an adequate supply of freight cars.[22]

Similar rivalries inhibit the initiation of promotional legislation, although efforts to develop a positive-sum package advanced considerably in the 92nd Congress.[23] In this instance, the impetus was provided by the Department of Transportation's development of deregulation proposals designed to increase the railroads' ability to provide competitive service. The American Trucking Associations (ATA) earlier had denounced the deregulation and subsidy proposals sought by the railroads—"aimed at destroying federal regulation of transportation," "a raid on the Treasury."[24] But the DOT initiative alarmed them, and, fearful that the railroads might ally with the administration, they sought a united front. The railroads and water carriers, stymied in their efforts to secure enactment of their own proposals, decided to cooperate. The result was the Surface Transportation Act (S 2362), an extravagant package authorizing $5 billion in loans to transport companies, increased investment tax credits, and liberalized procedures for the approval of rate increases and rail line abandonments—but omitting provisions which would facilitate rate reductions and thus excite competition within or between transportation modes. Senate Surface Transportation Subcommittee Chairman Vance Hartke, freed from the perils of cross fire among the carriers, had little hesitancy in heralding their joint request:

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[22] The exception that proves the rule is a bill (H.R. 8298) approved in 1970 which blocked an ICC decision unfavorable to regulated water carriers. The complexities of intermodal rivalry were outlined by a Senate aide:

> The best example [of one mode effectively outmaneuvering its rivals] is a very complicated one: the water carrier mixing rule. . . . The railroads were successful in holding off action for a long time, but the water carriers finally got smart. They [the regulated carriers] joined with the railroads in supporting a bill that would screw the *unregulated* water carriers. That got the bill through the House. Then they came over to the Senate, where the railroads didn't do as well. The final bill pleased [both classes of] water carriers; the railroads were finessed. But the whole business comes up again this year, and it may very well come unstuck.

In fact, the agreement did not come unstuck but was given permanent status by P.L. 93-201. As the same aide reflected in 1975: "I thought the whole thing would blow up, but it went through very easily. . . . I guess the railroads just decided, what the hell, they could live with it. They probably didn't want to upset the water carriers at a time when they were trying to get the Surface Transportation Act through [see below]. And they must have thought that they weren't really giving up much; the bill just ratified a *de facto* situation they'd been living with for a long time."

[23] See the account by Robert Fellmeth and Jonathan Low, "Surface Transportation," in Price et al., *Commerce Committees*, pp. 160–83.

[24] CQ *Weekly Report*, October 2, 1971, p. 2022.

> The transportation industry is its own worst enemy. There are so many differences among the railroads, the truckers, and the water carriers that we cannot make a start on solving the industry's problems.... I have urged [them] to lay aside their differences.... Now the Association of American Railroads, the Water Transport Association, and the American Trucking Associations have done what I asked them to do.[25]

In the end, however, this alliance did not fare as well as the one the promoters of aviation had forged in 1970. The proposal provoked vulnerable shippers,[26] farm organizations, and local communities, so that its champions ran up against environmental fragmentation and conflict after all.[27] The Surface Transportation Act was brought to a vote in neither chamber in the 92nd Congress. The House Committee brought a far more limited bill to the floor in the 93rd Congress, but too late for a conference to be called ("It took us a long time," Brock Adams apologized to his colleagues, "because we had to get the various modes to agree").[28] It was only in 1976, after the administration had made an issue of regulatory reform and the difficulties of the railroads had been given heightened public salience by the failure of the Penn Central and five other lines, that a deregulation and financial assistance package (P.L. 94–210) was enacted. But the bill's deregulation provisions were limited and applied only to the railroads, while the authorizations were cut back in the face of a veto threat.

"Public" orientations and appeals, in fact, rarely enter into surface transportation deliberations. A partial exception is AMTRAK, which legislators perceive to have considerable political appeal not only to affected regions and communities but also to the general public. While this reduces the impact of confirmed opponents of subsidized rail passenger service (e.g., the National Association of Motor Bus Owners), it does not prevent acquiescence to the carriers on low-visibility items—and, no more than in the case of public broadcasting, does it lead legislators to more than a sporadic defiance of the administration's budgetary and programmatic constraints. Railroad safety has also occasionally become a public issue; as a Senate Commerce aide recalls,

> You remember all that business about transporting nerve gas across the country in freight cars. We geared up hearings on that [S 1933, 91st Congress] and actually got pretty good press coverage—unheard of in the surface transportation area. And we produced a safety bill that for the first time wasn't just a labor matter, aimed at employees....

A few additional congressional ventures—the underwriting of research on high-speed ground transportation, for example, and an abortive proposal to set up regional planning and funding entities aimed at achieving "balanced transportation"[29]—also bore marks of a broad public orientation and appeal.[30] But the policymaking environment does not, by and large, encourage such ventures.[31] "The main problem with surface transportation" observed one counsel, "is that nobody seems to care about it. . . . The only people listening are those who are going to object [to departures from the status quo]." Echoed Brock Adams, who before his appointment to President Carter's cabinet was the most active legislator on the House Subcommittee: "Transportation is such an unrewarding thing."

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[25] *Congressional Record* (bound), July 28, 1971, p. 27633.

[26] On the role of shippers in this and subsequent confrontations, see Michael J. Malbin, "Rail Reform Issue Divides Carriers, Major Shippers," *National Journal Reports,* February 2, 1974, pp. 171–80.

[27] Less important was the fact that some legislators grew increasingly wary of the *electoral* impact of appearing too subservient to the industry. "The loan guarantee giveaway was the worst part of the bill," one counsel recalls. "But we couldn't get anybody interested in focusing on *that.* That's what Jack Anderson stressed. But the senators were mainly concerned with other provisions, those that drew fire from opposing *interests.*"

Anderson portrayed (*Washington Post,* June 6, 1972, p. B11) "Hartke and other friends of the railroads and big truckers" as "sneak[ing] through a bill that could cost the taxpayers more than $5 billion.... Ralph Nader's transportation experts call it the worst such bill they have ever seen." This had some effect on Hartke, whom an aide described as then "at his nadir politically in Indiana." "He pulled back.... He stuck by the bill but just didn't go out of his way." But full committee chairman Magnuson and other members responded to the carriers' blandishments to keep the bill alive, and backed off only when the crossfire of opposing interests became intense.

[28] *Congressional Record* (Daily), December 10, 1974, p. H11501.

[29] The "balanced transportation" proposal (S. 2279, 92nd Congress) "never got anywhere," recalls one aide. "The carriers were suspicious, and the administration was dead-set against it." As for farther-reaching proposals to use highway trust fund monies to develop other modes of transportation: "Magnuson has equivocated on that one.... Part of the problem has been a reluctance to offend another chairman [the Public Works Committee is responsible for highway policy]; also, Magnuson fought for a separate aviation trust fund and doesn't want to do anything to jeopardize that. Still, if Magnuson's staff had been able to convince him that 'balanced transportation' was a good PR issue, he might have pushed it more strongly."

[30] More particularistic proposals were often *justified* in these terms as well. "This is everybody's problem," explained astronaut Wally Schirra in an industry advertisement on behalf of the Surface Transportation Act. "If America can't deliver the

Merchant Marine. Somewhat more rewarding in legislators' perceptions, and more impressive (at least quantitatively) in congressional output, are merchant marine and commercial fisheries policy. Neither, Table 1 suggests, inspires extensive publicizing, information-gathering, or oversight efforts, but the 91st-93rd Congresses nevertheless saw the passage of 37 public laws in the merchant marine area and 17 dealing primarily with the commercial fisheries. Most of these were restricted in scope, but they included a major reformulation of the construction-differential and operating-differential subsidy program for the merchant marine and frequent increases in the president's authorization requests for the program.[32]

At first glance, the maritime industry appears fully as fragmented and conflict-ridden as any of the other transportation modes:

> The industry's non-subsidized companies have fought bitterly with the subsidized lines; conference operators have scrapped with independents; tramp operators have tried to enjoin grain shipments via tankers; the NMU has opposed the SIU; and small shippers have sought added protection from abuses of the conference system and the big business organizations supporting it. In the shipbuilding industry, bitter battles have been waged between East and West coast yards and between the industry's public and private sectors.[33]

Such conflicts generally imperil attempts to deal with maritime regulation.[34] But the last major promotional effort, the Merchant Marine

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goods, we'll all pay the price, higher *prices*, for fewer goods. . . . Who needs the Surface Transportation Act? We all do." Fellmeth and Low, "Surface Transportation," p. 179.

[31] Eighteen surface transportation measures became law during the 91st-93rd Congresses: three railroad safety and hazardous materials control bills, two extensions of the experimental high-speed ground transportation program, four AMTRAK authorizations, three bills responding to the Penn Central and other bankruptcies and providing for continuing rail service in the northeast, a bill providing loans to cover rail damage done by summer floods in 1972, the two formulations of the water carrier mixing rule, and three minor administrative measures. This excludes ten public laws in the area of railway labor, most of them minor in scope and relatively noncontroversial but of particular interest to House Commerce Chairman Harley Staggers because of the character of his West Virginia constituency.

[32] Included among the 37 merchant marine bills were 8 regular and supplemental authorizations; a 1969 bill continuing the subsidy programs and the Merchant Marine Act of 1970, which revamped them extensively; five bills pertaining to boat and waterway safety; nine industry-backed bills which liberalized the conditions for transferring cargo among barges carried by foreign vessels, permitted the sale of subsidized (but laid-up) passenger vessels, allowed terminal operators and stevedores to acquire a lien on vessels they supplied or serviced, loosened restrictions on ship-construction assistance, liberalized the mortgage guarantee program to promote investment in shipbuilding, made cruise and mail business more accessible to U.S. flag passenger vessels, and raised load limits for ships on international voyages; and thirteen additional minor measures.

[33] Samuel A. Lawrence, *United States Merchant Shipping Policies and Politics* (Washington: Brookings, 1966), pp. 295–96.

[34] The House Merchant Marine and Fisheries Committee, for example, sought in 1971 (H.R. 155) to ensure flexible operating conditions for U.S. barge-carrying ships sailing between foreign ports by modifying a restrictive U.S. statute which invited foreign retaliation. House Committee Chairman Garmatz presented the bill as "strongly supported by all shipping management, labor, and the Government agencies" (*Congressional Record* [bound], May 3, 1971, p. 12998), but protests from Sealand and other container-ship operators, which in certain situations competed with the barge carriers, threatened the bill and led to Senate amendments restricting its applicability. This sort of conflict, a former Senate Commerce aide recalls, is quite common:

> It was really a 'nothing' bill, but it ended up taking lots of time because Sealand didn't like it. Some senators began to choose sides. . . . My main job was to keep bills [like this] from exploding in the laps of members, to keep Magnuson out of intra-industry conflict. . . . [Even when a bill was passed] we would always avoid these conflicts–pass them along to the administering agency, though they should have been decided as a matter of policy. . . . I can show you language in a dozen reports where we avoided coming down on one side or the other.

For another example, this time involving labor-management conflicts, see the account of the delay of a bill permitting the sale of five laid-up passenger vessels: Price et al., *Commerce Committees*, pp. 282–84.

Act of 1970, was also threatened by intra-industry conflicts—between shipyards and their suppliers regarding the continuation of the requirement that subsidized vessels must be built with materials of U.S. origin, between newly-subsidized carriers and their competitors regarding the rate at which the former must shed their foreign holdings in order to be eligible for subsidies, between these same carriers regarding the status of the "indirect" subsidies the newly subsidized lines had previously received via cargo preference, and so forth.[35] The bill's passage was assured only when participants agreed to set aside a dispute over whether a line's enjoyment of indirect subsidies should affect its operating subsidy entitlement; the question was the subject of a pending administrative proceeding, and the Senate *Report* took pains not to prejudge the outcome.[36]

These and other conflicts were, however, contained; the less-advantaged segments of the industry realized that, rather than questioning the benefits enjoyed by the subsidized lines, it was more fruitful to seek comparable benefits for themselves.[37] And there was no doubt that the launching of this "new" maritime program was important to its congressional champions. In maritime, as in aviation policy, substantial rewards await the legislator who, having traversed the shoals of intra-industry conflict, can put together a positive-sum promotional package. The industry has the organization and the resources to reward its friends and channel their efforts, and industry objectives can normally be accommodated without exciting the opposition of other transportation modes or of the public. Moreover, the merchant marine, even more than aviation, is perceived as a localized interest, essential to certain constituencies. Its promotion thus comports especially well with the perceived responsibilities of maritime-district legislators—who naturally flock to the Senate Commerce and House Merchant Marine and Fisheries Committees—and with the patterns of accommodation and reciprocity typical of distributive and constituency-related policy areas.

Congressional initiatives in maritime policy have been both encouraged and constrained by the posture of the executive. Congress has been largely content to leave the regulatory status quo—as represented by the Federal Maritime Commission's enforcement of the various shipping and merchant marine acts—undisturbed. But they have been far from content with the executive's promotional efforts. Part of the problem from the standpoint of the industry and its champions is the degree to which the Commerce Department's Maritime Administration—which administers the subsidy and construction loan guarantee programs and is the starting point for the executive's promotional recommendations—is ignored or constrained within the executive branch. "No president since Franklin Roosevelt has taken a sustained interest in merchant shipping, considered the industry's problems comprehensively, had frequent personal contacts with the Maritime Administrator, or used personal leadership to achieve desired changes in the field."[38] Moreover, promotional proposals—of which "cargo preference" bills requiring a certain percentage of military cargo, agricultural exports, oil imports, or the like to be transported aboard U.S. bottoms are the most notorious examples—often excite the opposition of other governmental agencies.[39] And merchant marine advocates have been suspicious of the executive's consolidative tendencies, as reflected in attempts to integrate the Maritime Administration into the Department of Transportation; maritime congressmen have rather favored increased agency independence and freedom from budgetary trade-offs and control. But the Maritime Administration itself has been perceived as bureaucratically hidebound, an ineffectual advocate and sometimes—especially during the term of Administrator Nicholas Johnson (1964–1966)—a skeptical or hostile one.

The difference in executive and congressional approaches to maritime policy became quite pronounced during the 90th Congress. The Johnson administration's program, which indeed could claim to be "new" in invoking such universalistic criteria as economic efficiency, competitiveness, and national defense requirements, envisioned the termination or scaling-down of most subsidy and cargo-preference programs. Predictably, it got nowhere in Congress. The gap closed considerably during the Nixon administration; the Merchant Marine Act of 1970 was in fact an administration bill, and Congress passed it with relatively few emendations. It was patterned, however, on a measure Magnuson had introduced in opposition to the Johnson administration's recommendations. White House officials conceded, moreover, that the bill, which exceeded the recommendations of President Nixon's economic and military advisers, was sent up in anticipation of what Congress was likely to do in any case: "If we came in with a small program, we knew we were going to get rolled in Congress. . . . Some portion of this program is perhaps not required, but that is a very small factor compared with the price we would have had to pay on issues like the ABM and Cambodia."[40]

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[35] For a somewhat veiled discussion of the accommodations reached on these questions see Committee on Commerce, U.S. Senate, 91st Congress, *Report* to accompany H.R. 15424, August 10, 1970, pp. 19, 30–31, 55–59.

[36] Ibid., pp. 34, 63; and Senate Committee on Commerce, Subcommittee on Merchant Marine, *Hearings* on S. 3287, 91st Congress, March 2, 1970, p. 126.

[37] See Lawrence, *U.S. Merchant Shipping,* p. 307.

[38] Lawrence, *U.S. Merchant Shipping,* p. 315.

[39] On the factors making for the defeat of recent cargo-preference proposals, see Price et al., *Commerce Committees,* pp. 284–87; Richard Cohen, "Cargo Preference Faces Rough Waters in Congress," *National Journal,* September 24, 1977, pp. 1494–96; and CQ *Weekly Report,* October 22, 1977, pp. 2223 ff.

Commercial Fisheries. Congress also tends to be preeminent, and for similar reasons, in the promotion of the commercial fisheries. The fishing industry, like the merchant marine, is in a state of decline, increasingly dependent on governmental assistance, and close to the pocketbooks and emotions of maritime constituencies in ways that their representatives can hardly ignore. The National Marine Fisheries Service, located in the Commerce Department's National Oceanic and Atmospheric Administration (NOAA) since 1970, has conducted extensive research on resource management and from time to time has rendered valuable assistance to fishery advocates in Congress. But industry and other critics of the bureau have found its work overly "academic," lacking in "dividends" and "pragmatic remedies" relevant to the industry's decline; as an advocate, moreover, its effectiveness has been compromised by "internal handicaps, frowns from the Budget Bureau, and the merely casual interest of [the] Secretary."[41] Congressional partisans have thus found it profitable to develop their own proposals. If their incentives resemble those of the merchant marine's champions, the context in which they work differs considerably, for the fishing industry is much more loosely organized, much less dependent on and committed to going programs. Policy entrepreneurship, "one step ahead of the industry and two steps ahead of the executive branch," thus has been an attractive course for many members.[42]

Policy entrepreneurship, however, has on the whole been rather limited. Both fisheries and shipping policy, in fact, disabuse one of any notion that congressional preeminence necessarily spells creativity or rationality in policy making. Whereas Congress' maritime "initiatives" have largely consisted of bolstering old programs and industry-sanctioned approaches, fisheries initiatives have a scattershot and largely symbolic quality. During the 91st–93rd Congresses, they generally involved retreading or extending existing measures–providing construction and reconditioning subsidies for fishing vessels, increasing penalties for illegal foreign fishing in U.S. waters, reimbursing U.S. vessel owners whose boats are seized by nations claiming extravagant limits to their territorial sea (two bills), authorizing additional monies for state-federal fishery development research, and twice renewing a development program for salmon and other anadromous fish resources.[43] Meanwhile, longer-range proposals for federal management of the resource, the development of competitive ships and equipment, and the promotion of cooperative and more efficient business practices in the industry have gone begging.[44]

There has been less overt conflict within the fishing industry over promotional measures than has been the case with shipping; as a Senate fisheries aide observes:

> You basically have three industry groups–tuna, shrimp, and coastal fishermen–and there's not all that much fighting between them. The tuna fishermen already have their boats, so they don't benefit from the subsidy program, but they're basically doing pretty well. It's the shrimp fishermen that are mainly eligible for

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[40] See Joseph Albright's three-part coverage in *Newsday*, July 20–22, 1970.

[41] Edward Wenk, Jr., *The Politics of the Ocean* (Seattle: University of Washington Press, 1972), pp. 308–11.

[42] Price, *Who Makes the Laws?*, pp. 67–68.

[43] The ten remaining measures included four implementing international fisheries conventions, one authorizing the president to ban the importation of fish products from nations violating international conservation programs (aimed mainly at Danish salmon operations), one defining lobster as a protected continental shelf resource, a bill authorizing a U.S. contribution (and hence continued claim to the resource under treaty) for the restoration of salmon stocks in British Columbia, and three additional minor measures.

[44] See the list of objectives developed in 1969 by the National Council on Marine Resources and Engineering Development: Wenk, *Politics of the Ocean*, p. 310.

> the big loans under the program—but they don't care if some of it goes to the coastal operators.... I wouldn't say it's a "low" conflict area though. We hear grumbling from the coastal fishermen on the Fishermen's Protective Act [the reimbursement program, benefitting mainly shrimp and tuna operators whose vessels are seized by Latin American nations]. Why, they say, should we ransom the tuna people?

But if conflict is lower than it is in shipping, so is industry interest and pressure ("They don't really seem to care all that much . . . still living in the world of Adam Smith"); nor is administration hostility to distributive boondoggles any less evident.

The balance of legislative incentives becomes less favorable when one turns from promotion to regulation. The industry's deepening woes—overfished and depleted stocks, contradictory and overlapping state and local regulations—plainly call for a unified national policy. But intra-industry fragmentation and conflict, overlaid with an individualistic tradition and anti-governmental animus, have militated against any sort of comprehensive management program. A Senate staff member elucidates the situation:

> Fishing is basically a dying industry, but it's very sad, because the industry will shoot down any proposal that would bring meaningful change. They're all very individualistic; they just want to go out there in their own little boats, and they don't want the government interfering. And they have all kinds of conflict among themselves—anybody who puts anything forward is sure to offend somebody, so most politicians simply shy away. Alaska fishermen want to keep Washington fishermen out of *their* waters. And then there are the different *kinds* of fishermen—the gear they use, for example, gill netters and purse seiners and long liners.... There's no way to develop a management program that's going to be satisfactory to all of them.

Congressional passage of the Fishery Conservation and Management Act of 1976 (H.R. 200) signaled that change was afoot even in this most intractable of policy areas. Strengthened executive efforts, facilitated by the reorganization of NOAA, had some effect, although the industry, and Congress, balked at NOAA's 1973 proposal (S. 1069) to establish a federal management regime between the 3- and 12-mile limits of the territorial sea. More decisive in breaking the legislative logjam, ironically, was the coastal fishermen's desire for an extended zone closed to foreign fishing. As a Senate aide predicted in 1975: "It looks like the 200-mile limit is coming worldwide . . . but we can't simply ban *all* foreign fishing within that limit. It's going to be a matter of managing the resource, and you can't do that with respect to foreign operations without applying it to domestic operations as well." But congressional action was still far from unconstrained. The Senate's 200-mile limit bill of 1974 was shorn of management provisions, while the opposition of shrimp and tuna operators fearful of foreign retaliation helped forestall committee action in the House. The plan enacted in 1976 excluded tuna from coverage, gave the states the initiative in formulating regional management plans, and contained numerous qualifications designed to conciliate the Department of State.

Environmental conflict thus remains, and with it the temptation for legislators to revert to familiar approaches: the expansion of existing subsidy programs—which pour money into the industry without remedying its inefficient and fragmented operations—and the imposition of more stringent limits and sanctions on foreign fishing operations—which are directed at a tangible threat but, in view of the industry's larger problems of resource management, take on a symbolic, scapegoating quality. All this assumes, of course, that legislators concerned with merchant marine and fisheries policy take their cues almost exclusively from industry groups. Thus, while these policy areas differ from communications and surface transportation in offering marginally greater incentives for congressional involvement, they are, if anything, even *more* isolated from public understanding and interest. What congressional initiatives amount to, then, are generally quite particularistic promotional and protective measures; environmental conditions deflect attention from regulatory problems and agency oversight.

"Publicly Salient" Policy Areas

The "clientele-centered" areas of the Commerce Committees' domain differ from one another in their levels of intra-industry fragmentation and conflict, in the degree to which the relevant interests are localized or "distributive" in character, and in the degree of executive support and solicitude they enjoy. These differences have an important bearing on the incentives and constraints facing congressional policy makers. But these policy areas also have important features in common. Governmental "intervention," in the form of long-standing regulatory and promotional programs, is ubiquitous, but it has taken place largely on the industry's terms; present policy is valued for the protection from "destructive" conflict and competition, and often for the direct financial assistance, it provides. The industries, with the partial exception of the commercial fishermen, are well-organized and seasoned politically, and, seeking to repress more often than to promote major policy changes, are prepared to make their influence felt. Most importantly, these policy areas share a certain arcane and obscure quality in the eyes of more broadly-based groups and the general public. Insofar as legislators involve themselves in these areas at all, they generally perceive environmental forces which overwhelmingly dictate a "service" orientation toward the dominant interests and offer few rewards and little hope of success to one who deviates.

There are, however, areas of Commerce Committee jurisdiction where different sorts of environmental constraints are in evidence—areas, in short, where actual or potential levels of public knowledge and interest are higher. Congressional behavior in these "publicly salient" areas often deviates considerably from that characteristic of clientele-centered areas. Few areas, to be sure, can be precisely categorized. We have seen how anticipated public reactions can *restrain* unabashed clientele support in communications and surface transportation and, conversely, how the developers of clientele-oriented promotional legislation may be *stimulated* by (or frame their justifications in terms of) widely-perceived crises or needs. Similar circumstances can lead to a *modification* of the agenda and a shift in member priorities in traditionally clientele-oriented areas—witness the recent graft of hijacking, AMTRAK, boat safety, and tanker construction-standard measures onto the Commerce Committees' transportation agenda.[45] But there are nonetheless striking overall differences in the nature of environmental conditions and in the shape of Commerce Committee activity between the policy areas just examined and those to which we now turn.

Consumer Affairs. The prototypical publicly salient policy area is consumer affairs. The most striking feature of the Senate Commerce Committee's behavior in recent years has been its emerging leadership in this area, and the House Committee, while often lagging, has generally followed suit. Consumer policy occupied more days of hearings (Table 1) than any of Senate Commerce's other policy areas during the 91st-93rd Congresses, and ranked second only to health on the House committee. During this period the Senate committee reported 33 consumer bills, of which 28 were passed by the Senate and 17 finally became law. A number of the public laws dealt with matters of considerable scope—the regulation of cigarette advertising (2 bills), toy safety, child-resistant packaging of dangerous products, automobile construction standards, the repair of safety-related auto defects, warranty standards, and the creation of an independent Consumer Product Safety Commission.[46] In addition, bills permitting class-action consumer suits, establishing a federal program of no-fault automobile insurance, and providing for increased food plant inspection were prominent on the Senate Commerce agenda, though they fared less well on the Senate floor and/or in the House Committee.

The capacities of consumer-oriented agencies have increased in recent years, but not in such a way as to convince Congress members of the superfluity of their own efforts. It was President Johnson who first appointed a special assistant for consumer affairs; under Nixon the consumer office staff grew to 49 but was, in effect, downgraded by its transfer from the White House to HEW. This office and the consumer advisory staffs that have proliferated throughout the executive establishment have generally played public-relations and ombudsman, rather than substantive policy-making, roles.[47] The president's consumer office has sometimes been able to give helpful public testimony and behind-the-scenes assistance to congressional consumer advocates, and on occasion has influenced the administration's public stance. But legislators generally regard the consumer offices as impotent in the face of pressures from the White House staff, OMB, Commerce, Agriculture or other departments. As one House leader put it in discussing legislative strategy: "Sometimes we might call in the 'toothless tiger' [Special Assistant to the President for Consumer Affairs] Virginia Knauer, but without much effect."

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[45] See Price, et al., *Commerce Committees*, pp. 33, 69–72, 280.

[46] The remaining public laws included five bills extending and amending earlier consumer-protection measures (flammable fabrics, traffic safety, and fire research and safety) and four additional minor bills.

[47] See Mark V. Nadel, *The Politics of Consumer Protection* (New York: Bobbs-Merrill, 1971), pp. 46–59; Judy Gardner, "Spread of Departmental Consumer Offices Fails to Abate Pressure for Separate Agency," *National Journal Reports*, November 3, 1973, pp. 1627–35; and Price, *Who Makes the Laws?*, pp. 26–31, 96–99.

The two agencies traditionally responsible for consumer protection have not, despite some recent stirrings, filled the breach. The independent Federal Trade Commission and HEW's Food and Drug Administration, both of which came under severe attack with the rise of consumer activism in the sixties, have altered their procedures and stepped up their activity in various ways. But ties of industry-agency support, while loosened, have continued to inhibit the advocacy of new departures in policy, and both agencies have been "surprisingly reticent" in cultivating alternative, supportive constituencies.[48] The Commission's "independent" status subjects it to role constraints and political isolation, while FDA has been circumscribed both by its subordinate place in the executive hierarchy and by its lack of the kind of organizational machinery and leadership that would underwrite policy advocacy. Some hoped that the independent Consumer Product Safety Commission, established in 1972, might fill this organizational gap, playing a role "precisely analogous" to that of the Environmental Protection Agency.[49] Congressional observers tended to blame the CPSC's early shortcomings on the caliber of Nixon-Ford appointees, but their expectations for an agency of this size and stature were perhaps unrealistic in the first place.

The interest-group environment in consumer affairs has become more complex and conflict-ridden. Consumer-oriented groups have grown in prominence and effectiveness; this has often rendered industry opposition less debilitating than it was on such early efforts as cigarette labeling and fair packaging. A new breed of "public-interest" advocates and organizers following in the wake of Ralph Nader have made their voices heard, and many of the traditional consumer groups—united since 1967 in the Consumer Federation of America—have learned important political lessons. At the same time, however, opposition groups, alarmed by the increasing boldness and scope of the consumer-affairs agenda, have effectively marshaled their forces to fight such measures as no-fault insurance and the establishment of a Consumer Protection Agency to represent consumer interests within the federal establishment. There is some evidence that the prospect of such conflict has begun to offset the appeal of identifying oneself with consumer issues for some legislators. A House staff member commented on the apparent decline in popularity of the subcommittee handling consumer affairs:

> It may be that they see no-fault as the main thing coming up and just don't want to face the kind of pressure they're going to get from the trial lawyers.... Also, there's toxic substances, which is also a high-pressure situation, with a very powerful chemical industry.... Then, too, they might figure that some of the most significant things have already gone through that subcommittee....

Consumer affairs, however, remains an area in which a great many legislators find it profitable to invest time and energy. The size and relative seniority of Senate Commerce's Consumer Subcommittee during the 91st-93rd Congresses placed it behind only Aviation and Communications in general "desirability," and it clearly outranked both of those in the degree to which member interest got translated into legislative activity. Relatively little of this could be attributed to incentives provided or pressures exerted by consumer groups. Much more important was the conviction of members that consumer affairs was sufficiently appealing to the public to make policy-making efforts—and perhaps even the defiance of organized "interests"—a rewarding prospect. This explanation is borne out by the assessment of Ralph Nader's effectiveness given by his congressional collaborators: much more important than his contribution to *group* formation—what David Truman would term the actualization of a "potential" group[50]—has been his ability to increase an issue's *public salience* via the media. It suddenly seems politically profitable to champion an issue like radiation protection, and helping the automobile manufacturers secure a deferred compliance deadline becomes decidedly less attractive when such an "inconspicuous" amendment is likely to be reported in Jack Anderson's column the next day.[51]

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[48] Nadel, *Politics of Consumer Protection*, p. 81; cf. Judith A. Turner, "FDA Pursues Historic Role amid Public, Industry Pressures," *National Journal*, February 15, 1975, pp. 250–59. For an assessment of FDA, FTC, and CPSC operations (and those of the FCC, ICC and EPA as well), see "Federal Regulation and Regulatory Reform," *Report* of the Subcommittee on Oversight and Investigations, House Committee on Interstate and Foreign Commerce, 94th Congress, October, 1976.

[49] See the exchange between NCPSC Chairman Elkind and Senator Moss, *Hearings* on S. 983 and other bills, Committee on Commerce, U.S. Senate, 92nd Congress, July 19, 1971, p. 143.

[50] David B. Truman, *The Governmental Process* (New York: Knopf, 1964), pp. 510–15. More apropos is E. E. Schattschneider's discussion of broadening the "scope of conflict" as a political tactic: *The Semi-Soveign People* (New York: Holt, Rinehart, and Winston, 1960), Ch. 1 and *passim*.

Environmental Policy. Environmental affairs, as Figure 1 indicates, became a leading item on the Commerce committees' agenda some four years later than consumer affairs, as public concern for environmental quality mounted.[52] It was joined in turn—and to some extent counterbalanced—by energy, after the Arab oil boycott of 1973. It was also an area (like energy) where the Commerce Committees had to compete for jurisdiction with Public Works, Agriculture, Interior, and other committees. Yet environmental policy, especially in the Senate, received a major share of committee hearing time during the 1969–1974 period. At the same time, there was a striking disparity between the time spent on publicizing and information gathering and the number of bills actually reported and passed (see Table 1). A closer look at this pattern reveals differences among the incentives and constraints faced by advocates in environmental and consumer affairs.

First, conflict and contention in the environmental protection area, as compared to consumer affairs, is more publicly visible, more closely approximates the zero-sum formula, is broader in scope, and involves more complex issues. The environmental lobby, like the consumer lobby, has proliferated in recent years and has increased markedly in political sophistication; older groups such as the Sierra Club and the Audubon Society have broadened their membership and have increasingly focused on policy questions, while their efforts have been supplemented (and often challenged) by activist newcomers such as Environmental Action and the Environmental Policy Center. But the potency of industry's counter-efforts has been convincingly demonstrated on such matters as the Alaska pipeline and the Clean Air rollbacks; the opposition, moreover, has appealed effectively to broader public fears of increased taxes, unemployment, and reduced energy supplies.

Related to this pattern of salience and conflict is a second important difference between the environmental and consumer areas: the relatively greater prominence of the executive in environmental policy making. The Environmental Protection Agency (EPA), organized in 1970, has had the advantages of high public visibility and relatively adroit leadership. Its position within the executive has been subject to considerable erosion; Presidents Nixon and Ford both were erratic in their support, and the opposition engendered by enforcement efforts and the energy crisis subjected EPA to debilitating agency crossfire.[53] Still, the seriousness with which EPA's position on such questions as Clean Air revisions is taken in Congress and the bureaucracy provides a striking contrast to the neglect often accorded the Office of Consumer Affairs in its attempt to play an independent advocacy role on matters such as the formation of a Consumer Protection Agency. A perusal of the major legislative initiatives in the environmental area since 1969 reveals a quite substantial executive role—the EPA on drinking water, noise, and air pollution, the Council on Environmental Quality on ocean

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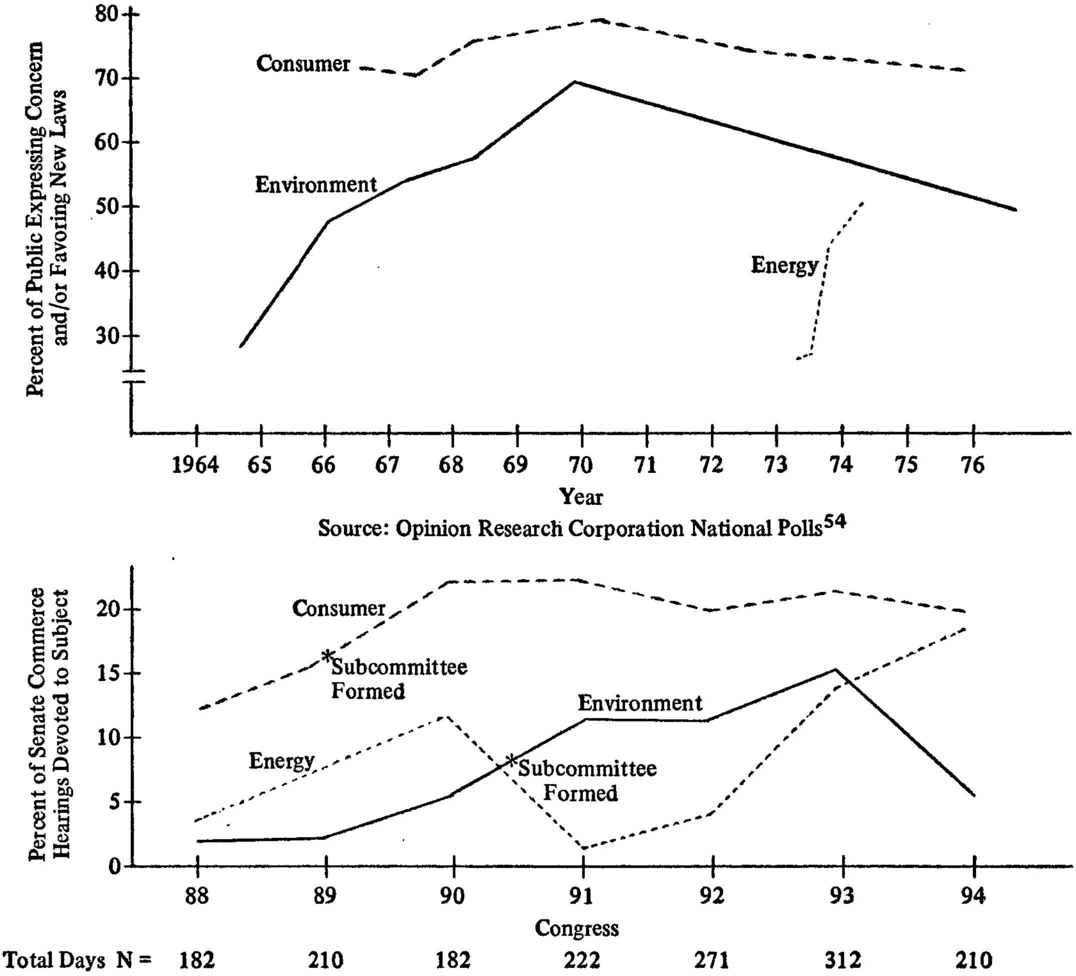
[51] This interpretation bears out Nadel's treatment of the press and consumer advocates as information brokers, facilitating the transfer of consumer politics to a wider public arena. But this is not primarily accomplished by lowering the information and organizational costs that inhibit collective action on a broad front; those costs remain formidable, and the "potential group" largely unorganized. What happens, rather, is that consumer affairs comes to be perceived as *politically* relevant by legislators; citizens with low incentives to consumer activism may nonetheless deem such issues relevant to their *vote*. See Nadel, *Politics of Consumer Protection,* Chs. 5, 7; Price, *Who Makes the Laws?,* pp. 35–36, 59–60.

[52] On the rise of the public salience of environmental policy see Anthony Downs, "Up and Down with Ecology—the 'Issue-Attention Cycle'," *The Public Interest,* Summer, 1972, pp. 38–50; and Charles O. Jones, *Clean Air: The Policies and Politics of Pollution Control* (Pittsburgh: University of Pittsburgh Press, 1975), Ch. 5.

While the ORC data does not permit a precise *comparison* of the salience of environmental and consumer affairs, Figure 1 indicates that levels of public support for federal activity in the latter area are remarkably high and stable. Environmental affairs has been subject to more variation, sharper increases and decreases in salience. While consumer matters have never made the lists in the Gallup Poll's open-ended "most important problem" surveys, pollution and ecology hovered in the 5 percent range in 1971–1973. Energy has received a similar share of mentions since 1973, but with a temporary jump to 46 percent in early 1974. See *The Gallup Poll: Public Opinion 1935–1971* (New York: Random House, 1972) and relevant numbers of the *Gallup Opinion Index.*

[53] See the *National Journal Reports* pieces of December 22, 1973, pp. 1911–15; March 23, 1974, pp. 431–38; April 13, 1974, pp. 533–43; and March 6, 1976, pp. 306–09.



Source: Opinion Research Corporation National Polls[54]

Source: Committee Calendars

Figure 1. Patterns of Public Salience and Congressional Activity for Selected Issues, 1963–1976

dumping, the Office of Science and Technology on the siting of nuclear power plants.

This combination of high salience, high conflict, and relatively high executive involvement has a marked effect on how legislators relate to the environmental protection policy area. At certain times, as Charles Jones argues in his study of the Clean Air Act of 1970, the perceived level of public salience has made "policy escalation" seem imperative even to cautious legislators such as Paul Rogers, chairman of House Commerce's Subcommittee on Public Health and Environment.[55] The normal congressional response, however, has been a certain wariness of becoming identified as a pro-environmentalist "extremist." Often the safest course seems to be to identify oneself with the administration's environmental proposals but not to attempt major alterations of those bills' timetables, standards, or enforcement machinery, however open to criticism they may be after having run the White House gauntlet. The House Commerce Committee, for example, was content to remain close to the administration draft of most provisions of the Clean Air Act and the two other environmental measures it processed in the 91st Congress. Senator Edmund Muskie and his Public Works Subcommittee introduced more substantial modifications, however, and Rogers himself sought a more independent–though still basically consensual–course in 1973 as the White House broke with the EPA and advocated extensive clean air rollbacks.

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[54] The ORC questions, not precisely comparable, tapped the extent to which respondents favored new federal laws to protect the health and safety of consumers, were inclined to view air pollution as a "serious" problem, and believed the energy shortage would last "a long time."

[55] Jones, *Clean Air*, pp. 175–91.

Despite its narrower jurisdictional claims–not including, for example, the air pollution and solid waste measures that accounted for six of the eleven environmental bills reported by House Commerce in 1969–1974–Senate Commerce for a time developed a more assertive role in environmental affairs. The Senate committee developed legislation dealing with automobile emissions, toxic contaminants in consumer products, drinking water standards, and the manufacture and use of harmful pesticides. Commerce's Environment Subcommittee, formed in 1969 under Philip Hart's chairmanship, put forward innovative proposals to encourage recycling and materials conservation and to facilitate citizen suits in the environmental area. And the committee's agenda in maritime affairs, oceanography, and other areas assumed an increasingly prominent environmentalist cast.

These efforts, and analogous ones undertaken by the Muskie subcommittee and John Dingell's House Subcommittee on Fisheries and Wildlife Conservation and the Environment, suggest that contextual conditions induce legislators to handle environmental matters more like consumer affairs than like, say, transportation or communications. Yet a closer look at Commerce Committee involvement reveals some of the constraints and obstacles which environmental initiatives face.[56] Both Commerce Committees balked at giving EPA the authority to regulate aircraft noise in 1972. Both have experienced great difficulty in formulating power plant siting legislation. Neither has taken up the citizen suit bill at the full-committee level. Several of the bills which the Senate committee has approved–toxic substances, drinking water, pesticides, emergency chlorine allocation, sonic boom control–have suffered dilution, delay, and/or defeat at the hands of House Commerce and other committees. Within the House committee, Rogers and his subcommittee have become more assertive only to find themselves at loggerheads with auto industry spokespersons and others wishing to proceed cautiously in the air quality area. And Figure 1 suggests that Senate Commerce members have had limited staying power in the environmental policy area; while consumer affairs continued to occupy approximately the same share (18–21 percent) of the committee's hearing time in the 94th Congress that it had since the 90th, environmental affairs dropped to 5 percent from its 1969–1974 level of 11–15 percent. As environmental matters became more politically controversial and less publicly salient, Senate Committee interest waned. No one took up the slack left by Hart's failing health, and few objected strongly in 1977 when Senate committee reorganization reduced Commerce's already limited environmental jurisdiction still further.

Oceanography. A policy area notable for recent increases in both public salience and congressional involvement is that complex of research, development, environmental, and governmental reorganization measures generally termed "oceanography" policy. Although oceanography resembles merchant marine and fisheries policy in the interests and geographical regions to which it is most relevant and in the legislative champions it has attracted, its group and agency setting differs considerably from the clientele-centered model of which maritime affairs is, in many ways, a prototype. Pressures for increased federal attention to oceanographic matters initially came from governmental bureaucracies and the scientific community. Legislators from the maritime committees fastened on the issue, hoping that oceanographic policy ventures might catch the imagination and approval of the public (and also seeking to protect their committees' jurisdictional turf).[57] But the interest of industry and of state and local governmental units in the policy area has been limited and their inputs diffuse; after extensive efforts to heighten the support of such groups, Edward Wenk, former Executive Secretary of the National Council on Marine Resources and Engineering Development, concluded in 1972 that oceanography's constituency "remained unmobilized."[58]

Legislators interested in oceanography are thus not generally faced with persistent demands or pressures. Nor has the executive added a great deal of coherence to the policy-making environment. CEQ took the lead in developing ocean-dumping legislation, and a measure of bureaucratic coordination was achieved by the formation of the National Oceanic and Atmospheric Administration in the Department of Commerce in 1970. Yet, as one NOAA administrator explained, the agency was subject to severe budgetary and program constraints: "This [Nixon] administration is just not marine-oriented, just like the last one wasn't." NOAA's leaders have lacked the resources to counter balkanization within—Administrator Robert White said he was "the first to admit NOAA is more like a collection of the groups that came into it than a coherent agency"[59]—or to prevail in policy conflicts within the executive. On questions such as deep-water port construction and coastal zone management, for example, the Nixon-Ford administration favored Interior Department approaches and authority over the claims of NOAA.

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[56] See the discussion in Price, et al., *Commerce Committees*, pp. 270–78.

[57] Price, *Who Makes the Laws?*, pp. 61–75; Wenk, *Politics of the Ocean*, Ch. 2.

[58] Wenk, p. 149.

Environmental conditions thus help explain how oceanography, like maritime affairs generally, has been dominated by Congress, but with far freer play given to the initiatives of members and the recommendations of "experts" than is true of merchant marine and fisheries matters. Congress has often turned to panels of scientific and bureaucratic practitioners in its search for promotional measures and has taken an increasingly active role in *stimulating* agenda development—as with the formation in 1966 of the Commission on Marine Science, Engineering and Resources, and the launching in 1974 of an extensive "Ocean Policy Study" by the Senate Commerce Committee.[60] The Senate committee held more days of hearings on oceanography matters in the 91st-93rd Congresses than it did on merchant marine policy (Table 1), and it reported twelve bills in the area, all of which became law. Four of these dealt with the sort of organizational questions prominent on the oceanography agenda during the sixties, but six bills dealt with problems of ocean pollution and coastal zone management, setting up major new programs in both areas.[61] This focus on environmental protection promises to move "marine-oriented" legislators into more competitive and embattled policy-making arenas; the opposition of the dredgers and port authorities to EPA administration of the ocean dumping program, and of the oil companies and the Interior Department to a NOAA-based coastal-zone program, were no doubt prophetic. But by the same token, oceanography seems likely to become less the exclusive province of maritime-district legislators, and increasingly to resemble other publicly salient policy areas.

Health. Health policy probably ranks first among those we have considered in its attractiveness to publicity-minded legislators. Yet the environment in which it is formulated is also uniquely complicated and "crowded." The witness list for almost any congressional hearing on health policy contains dozens of professional and other interest groups: the American Medical, Nurses, Dental, and Public Health Associations; the Lasker Foundation, heart and cancer societies, and others promoting medical research; the national associations of hospitals, medical schools, and insurance companies; labor and senior citizens' groups, and so forth. The legislator's basic dilemma is how, in exploiting health's public and political salience, to deal with these organized groups, particularly those within the medical profession. It is not merely a matter, as the long battle for medicare accustomed many to think, of dealing with the *opposition* of the AMA and other groups to anything that seems to threaten the individual fee-for-services system. It is more often a question of *how* the massive governmental involvement in health-care delivery and research is to be directed and institutionalized. Many early proponents of what became the "regional medical programs," for example, envisioned a primary focus on the *diagnosis* and *treatment* of major diseases. But the bill that emerged from Congress—the Heart Disease, Cancer, and Stroke Amendments of 1965—was primarily directed toward the promotion of *research* and the "strengthening" of the National Institutes of Health (NIH) and of *existing* hospitals and medical schools.[62] Similarly, the main question that arose in 1974 regarding federal aid to medical students was whether the funds were to be granted to institutions to use at their discretion, as the Association of American Medical Colleges insisted, or whether grants would be made directly to students or otherwise tied to their willingness to practice in medically under-serviced areas.[63]

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[59] Wenk, p. 365.

[60] On these initiatives and the 1972 struggle to establish a coastal zone program, see Price, et al., *Commerce Committees*, pp. 288–300.

[61] The two remaining bills were an administration measure authorizing the development of deepwater ports and a renewal of the National Sea Grant College and Program Act; Commerce shared jurisdiction with the Interior and Public Works committees on the first, and with Labor and Public Welfare on the second.

[62] Price, *Who Makes the Laws?*, pp. 216–27.

If legislative efforts are frequently directed toward well-worn institutional channels by the AAMC, American Hospital Association, and other professional organizations, so does the executive exert a decisive influence on the moves legislators make in the health field. The White House, OMB, HEW and various other arms of the executive are deeply involved in the formulation and implementation of health policy; the kind of information legislators working in the area need, the scale of the bureaucratic operations they must presuppose or project, and the kind of publicizing and mobilization they need to bring controversial initiatives to fruition often make congressional activists dependent on executive agencies and place a high premium on the development of cooperative relationships. Of particular importance has been Congress' tie to HEW's research bureau, the National Institutes of Health. NIH has stood out for its independence within the executive hierarchy and the vigor of its lobby support, centered in the remarkable Lasker Foundation. This institutional strength, together with the relative absence of oppositional crossfire in the research area, has encouraged legislators seeking a reputation in health affairs to choose the boosting of NIH programs and budgets as the most efficacious way of implementing their objective.[64] The Nixon-Ford years saw some erosion of NIH's favored position, with OMB and HEW successfully asserting administrative and budgetary control and long-standing conflicts over "basic" versus clinical research becoming more intense. By 1971, in fact, the research lobby and their Senate collaborators felt that the new "Conquest of Cancer" effort must be placed outside of NIH if it was to escape trade-offs with other research efforts and receive the priority it deserved. But the House Commerce Committee, in returning the program to NIH's National Cancer Institute, demonstrated the kind of support NIH could still muster among legislators and in the research and professional communities.[65]

More than one-third of the bills reported by the House Commerce Committee during the 91st-93rd Congresses were in the health area. The 59 House-passed bills included 18 extensions of existing programs of construction, planning, fellowship, and other assistance to medical schools, hospitals, libraries and public health facilities. The Committee's usual pattern was to resist administration attempts to cut back, consolidate, or otherwise drastically change these programs on which various communities and groups had come to depend, but at the same time to temper the Senate's program expansions and innovations.[66] Five bills dealt with drug abuse and displayed an extensive executive role, reflecting the appeal of this issue to both Congress and the president. Congress also responded to executive initiatives, albeit in ways that deviated from the administration's preferred approaches, with bills providing family-planning assistance, consolidating certain public health programs, and setting up local health planning agencies. Thirteen bills dealt with research and treatment of major diseases: the 1965 regional medical centers program was expanded to include kidney disease, and the 1971 "Conquest of Cancer" Act was followed by a spate of bills focusing effort on heart and lung diseases, digestive diseases, sickle cell anemia, Cooley's anemia, multiple sclerosis, arthritis, diabetes, the sudden infant death syndrome, and disorders associated with aging.

Major-disease research was the area where congressional priorities most obviously violated the budgetary and organizational preferences of the executive, but the same was true of assistance to communities in diagnosing and treating communicable diseases (two bills) and in developing emergency medical services (three bills), research and treatment programs on alcoholism (three bills), and efforts to train more doctors for general practice and to deploy them to medically underserviced areas (four bills).[67] But in most of these cases the House

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[63] J. K. Iglehart, "Maldistribution of Physicians is Focal Point of Manpower Debate," *National Journal Reports*, June 8, 1974, pp. 837–46; September 7, 1974, pp. 1357–58.

[64] See Price, *Who Makes the Laws?*, pp. 216–21; and Stephen P. Strickland, *Politics, Science and Dread Disease* (Cambridge: Harvard University Press, 1972), Chs. 5–8.

[65] See Richard A. Rettig, *Cancer Crusade* (Princeton: Princeton University Press, 1977); Strickland, *Politics, Science and Dread Disease*, Chs. 9–10, 12; Price, et al., *Commerce Committees*, Ch. 6; Harold Schmeck, "Cutbacks and Low Morale Trouble NIH," *New York Times*, February 22, 1974, p. 36.

[66] Such intergovernmental conflicts gave House health bills a relatively high mortality rate: five successfully vetoed in 1969–1974, three deadlocked in conference, four never acted on in the Senate.

[67] The 59 health bills included, in addition, a diluted HMO measure (see below), a bill altering

Subcommittee was ratifying or tempering Senate initiatives, and on several additional Senate bills—dealing with such matters as health services for migrant workers, the safety of medical devices, and the subsidizing of health maintenance organizations (HMO's)—the House committee failed to act.[68] What persists, then, is a congressional tendency to gravitate toward the areas of health policy which are relatively high in their public saliency and relatively low in the conflict they engender, e.g., medical research and aid to communities for the development of facilities. At the same time, the groups and agencies that "crowd" the field impose certain constraints, more noticeably on the House than on the Senate subcommittee, both on what is taken up and on how the issues that are taken up are finally resolved.

The Committees and Their Environment: Key Variables

Our survey of patterns of Commerce Committee action and inaction suggests two overriding determinants of the incentives and constraints legislators perceive as they decide where to invest their policy-making time and effort. Table 2 sorts out a number of policy areas handled by one or both Commerce Committees according to their degree of public salience and the amount of conflict they entail.

"Public salience" refers not to any precise measure but to a rough comparative standard based on the expressed preceptions of congressional members and aides. "Salience" of course denotes the degree of interest a policy excites within a given sector, but more crucial is the "domain" within which salience is present, the range of persons "whose policies are affected."[69] On almost any issue there are several intensely interested groups to contend with, but in some policy areas more than others politicians also perceive a more broadly based "public" awareness and concern that must be taken into account (or might profitably be awakened). Shipbuilding and airport subsidies, for example, are seen as problems that excite intense interest but only in very small and specialized sectors; rail passenger service and the promotion of tourism and trade perhaps generate less intensity but have a wider appeal; and health research and consumer protection are felt to possess high salience for a broad segment of the public.

The "conflict" variable is also imprecise but is real enough to policy makers. The reference may be to conflict among the particularistic groups with a stake in a given policy area. Proposals to regulate (or deregulate) surface transportation, for example, excite intense conflict both within and between transportation model. "Promotional" packages are sometimes more amenable to positive-sum formulations, although policy areas vary considerably in this respect (from fisheries promotion near the "low conflict" pole to surface transportation toward the high) and scarcely resemble the tranquil world of mutual back-scratching described by some analysts of "distributive" politics. Rele-

Table 2. Policy Areas Handled by the Commerce Committees, Categorized According to the Degree of Public Salience and Conflict They Are Perceived to Entail

Public Salience	Conflict High	Conflict Medium	Conflict Low
High	Health care delivery Environmental protection	Most consumer protection measures	Medical research
Medium	"Balanced transportation"	AMTRAK, coastal zone development	Tourism
Low	Communications and transportation regulation	Merchant marine subsidies; aviation promotion	Oceanographic and fisheries research

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federal assistance programs for mental retardation (which died in conference), and six relatively minor food and drug and public health amendments.

[68] The HMO bill fell victim to House committee delays and to the opposition of the administration and organized medicine in the 92nd Congress. In 1973 the House passed a bill that was of pilot-project dimensions compared with the Senate version. See Price et al., *Commerce Committees,* pp. 261–69. Legislation authorizing FDA to regulate medical devices, pending in Congress for more than a decade, was enacted in 1976.

[69] See Harold D. Lasswell and Abraham Kaplan, *Power and Society* (New Haven: Yale University Press, 1950), p. 73.

vant conflicts also arise between the wishes of organized groups and the course of action the politician calculates to have the greatest public appeal. In medical research, for example, politicians have found the securing of alliances with NIH and the research lobby to be compatible with (indeed, instrumental to) building a public reputation as a leader in the area, but those concerned to promote the establishment of HMO's or to meet the needs of medically underserviced areas have had to calculate the costs of defying skilled and seasoned professional organizations. Here, of course, a relationship to the saliency variable becomes apparent: the presence of conflict, after all, presupposes saliency in *some* quarter, and the conflict is more likely to be perceived in universalistic-particularistic terms in those areas that possess high public salience. But while the level of saliency may determine the form or arena of conflict, both high- and low-saliency policy areas range across the spectrum in the degree of conflict they entail.[70]

Perceived Salience and Conflict: Their Impact. Our evidence suggests that issues tending toward the upper right-hand cell of Table 2 will offer the highest incentives to legislators calculating the likely consequences of initiative and involvement, while those tending toward the lower left-hand cell will be least attractive. The congressional tendency to promote medical research efforts and the reluctance to tamper with the regulatory *modus vivendi* in communications and surface transportation are readily understood in these terms. More intriguing, because the incentives for involvement are mixed, are those issues tending toward the upper left-hand and lower right-hand cells. The lure of public salience may overcome the perils of conflict for senators such as Edward Kennedy and Philip Hart as they put forward relatively bold initiatives in health care delivery and environmental protection respectively—but it is hardly surprising to find Paul Rogers tempering his subcommittee's output in both areas (even as he comes out with both guns blazing in areas of less conflict such as health research and HEW reorganization). By the same token, legislators are likely to endorse—and, given the prospects of constituency and group support, actively to push—airport and shipbuilding subsidies if intra-industry conflict has been ironed out and the likelihood of public "exposure" is remote. But they are unlikely to see such involvements as the stuff of which an adequate public or in-house reputation can be made.

More elusive but equally important is the impact of these conflict and saliency variables on the *content* of congressional initiatives. It has become common to explain congressional policy making (or its absence) in terms of the cozy relationships that obtain among particularistic groups, committees, and agencies—"unholy trinities," "triangular trading patterns," "whirlpools" of influence, "subgovernments," and the like.[71] Our analysis suggests that policy areas are not equally closed in this respect, and that the inclination of legislators to take their bearings from broader public interests or extrinsic standards of good public policy will be dependent in large part on an issue's perceived public salience. In areas such as consumer and environmental protection, the presence of (or attempt to generate) public salience is likely to produce "stronger" legislation—an increased range of activities under regulation, "more certain" and more stringent enforcement mechanisms.[72] But the "strength" of an airline subsidy bill can neither be defined in these terms nor related in this way to the level of public awareness. The crucial dependent variable is thus not "strength" in some abstract sense but the degree to which legislators respond to "those interests which people have in common *qua* members of the public."[73]

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[70] It may also be true that under certain conditions conflict helps *produce* public salience. "The logic of audience maintenance," writes Edward Jay Epstein in a study of television news, assumes "that scenes of potential conflict are more interesting to the audiences than scenes of placidity. Virtually all executives and producers share this view," *News from Nowhere* (New York: Vintage, 1974), p. 262. Certainly legislators often try to heighten apparent conflict as a means of calling attention to their efforts; recall Ribicoff's and Hartke's handling of the auto manufacturers during the 1966 hearings on traffic safety (Price, *Who Makes the Laws?*, pp. 50, 54–56). But conflict per se is surely no guarantee of salience, and even in high-salience areas, most legislators, most of the time, have high incentives—in light of both electoral and institutional-maintenance considerations—to avoid or to mute conflict.

[71] See John Gardner's testimony before the Select Committee on Committees, U.S. House of Representatives, 93rd Congress, *Hearings* on Committee Organization in the House, October 4, 1973, p. 242; Theodore J. Lowi, *The End of Liberalism* (New York: Norton, 1969), pp. 111–12; Ernest S. Griffith, *Congress: Its Contemporary Role*, 3rd ed. (New York: New York University Press, 1961), pp. 50–51, 158–59; and Douglass Cater, *Power in Washington* (New York: Vintage, 1964), pp. 16–22.

[72] See Nadel, *Politics of Consumer Protection*, p. 152. Charles Jones' examination of Congress' handling of environmental policy leads him to hypothesize that perceptions of public and media salience make non-incremental policy decisions, the overriding of group opposition and agency reluctance, and "leaps" beyond approaches of proven feasibility more likely. *Clean Air*, Ch. 7.

Heightened public salience thus might lead to a "strengthened" auto safety measure or a "weakened" package of maritime subsidies; in fact, certain kinds of industry-oriented policy initiatives (e.g., increasing the length of broadcast license terms, or broadening the cargo-preference laws) would probably become *less* attractive and less frequent. The crucial point is that the presence of (or potential for) public salience is likely to make politicians more willing to stake out positions and objectives that go beyond (or defy) the wishes of the organized groups with the most immediate and tangible stakes in the area. This is not to say that serving group and "public" interests will always be viewed as incompatible. The extent to which they are so perceived will partially depend on the policy maker's identifications and ideology, as the contrast between Kennedy's and Rogers' self-styled pursuits of the "public interest" in health policy suggests. Nor should one conclude that increased public salience, while it alters patterns of responsiveness, will *necessarily* make for more serious and sustained legislative involvement; in fact, many members seem prone to engage in a kind of media-oriented "position-taking" on major issues that moves in the opposite direction.[74] The fact remains, however, that legislators tend to see issues as either publicly salient or not, and to vary the frequency and content of their initiatives accordingly.

Variations in Issue Perceptions. Commerce Committee performance suggests two further observations. First, issues may traverse the cells of Table 2, sometimes rather rapidly, over time; the movement of consumer, and then environmental, affairs toward the upper left-hand cell in recent years furnishes rather striking cases in point. A dramatic recent example is the defeat of the Sugar Act in 1974. A sharp rise in sugar prices made for increased public awareness of and puzzlement over the subsidy program. Levels of conflict and hostility concomitantly were heightened—between aroused consumer spokespersons and the growers, and also among particularistic groups (e.g., the suppliers and such users of sugar as beverage manufacturers) with an intense interest in the policy area. The sugar program suddenly became more salient and controversial; the incentives and constraints facing congressional policy makers shifted, and a program previously thought invulnerable was killed. Such shifts are often attributable to "objective" trends or events, such as ecological disasters or the "energy crisis," but advocates and journalists like Ralph Nader and Jack Anderson can play an important role in both heightening and exploiting a problem's visibility. Nor is a legislator necessarily passive before these environmental conditions. Congressional proponents of coastal zone and other ocean-related legislation have taken pains, through well-publicized attacks on the administration and the authorization of a comprehensive "Ocean Policy Study," to heighten their issue's public salience. The idea, in part, is to enable (and motivate) colleagues to transcend the opposition pressures that might come from particularistic interests, but coastal zone proponents have also dealt with the conflict variable directly—for example, by drafting legislation so as to leave the devising of environmental control standards largely to the states.

Second, the centrality of each of these environmental variables to the perceptions and calculations of legislators may vary considerably with time, between chambers, and so forth. In particular, Commerce Committee members, and perhaps national legislators in general, seem to be regarding more and more issues in terms of their actual or potential public salience and to be shifting their legislative interests accordingly. The number of days of hearings devoted by Senate Commerce to its most visible policy areas—consumer affairs, energy, and the environment—increased from 28 in the 88th Congress to a peak of 151 in the 93rd, despite its quite constricted jurisdictional claims in the latter two fields. As Figure 1 shows, this represented an increase from 16 percent of the committee's total hearing time in the 88th Congress to 48 percent and 40 percent in the 93rd and 94th, respectively. This is no doubt partially a response to the perceived growth of issue awareness among voters—certainly on matters under Commerce Committee jurisdiction and perhaps on national issues in general[75]—and to the potential for influencing public attitudes which the new consumer and environmental advocates are felt to possess. Also important are shifts in the electoral situations which many legislators face: larger and more diffuse constituencies, television as the dominant communications medium, the erosion of party loyalty and personal acquaintance as bases of electoral choice. In such situations members of Congress must seek visibility—through large constituency-service and self-promotional operations, by claiming credit for federal largesse in their districts, and, increasingly, by identifying themselves with issues which are (or might be made) salient to the electorate.[76] Trends in this latter direction are especially evident nowadays in the Senate, where the constituencies that must be reached are larger, one's actions are more visible, ambitions for a national reputation are more widespread, and the average member's opportunities and resources for policy entrepreneurship are greater. Certainly the impact of an altered pattern of responsiveness on a legislator's or a committee's positions and priorities can be profound. A prime example is Senator Warren Magnuson, Chairman of the Senate Commerce Committee, whose near-defeat in 1962 helped stimulate him to shift his policy interests toward consumer affairs.[77]

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[73] Brian Barry's conceptual distinctions between private and public interests will suffice for our purposes here. *Political Argument* (London: Routledge & Kegan Paul, 1965), p. 190 and *passim.*

[74] The term is used by David Mayhew to point up the relatively superficial character of the policy ventures which the desire for electoral visibility encourages. *Congress,* pp. 61–73, 110–25. But Mayhew may underestimate the incentives to "follow through" that stem from the quest of the legislator (particularly if he or she holds a position of committee or subcommittee leadership) for a "professional reputation" within the chamber and in the Washington community.

The Intervening Variable: Executive Involvement. Committee members respond not merely to the political peril or promise of a given mode of issue-involvement, but also to the observed or expected behavior of coordinate policy-making units inhabiting the same jurisdictional terrain. Of prime importance, for example, are the relationships of coordinate committees with one another. Committees may differ, as Table 1 and our policy-area surveys suggest, in their responses to environmental conditions; these responses may in turn shape and reinforce intercommittee roles and relationships.[78] Still, our committees are strikingly similar in the way they have handled most of the policy areas under their care. If environmental variables help explain this, so also do the relationships the committees share with those all-important "outsiders," the executive and independent agencies.

Congress and the executive deal somewhat differently with the environmental factors we have highlighted. The White House, to overgeneralize a bit, is more strongly drawn by matters of broad public salience and is less easily deterred or stymied by conflict—partly because of its superior capacity to publicize and thus to *create* the kind of salience that alters the incentives surrounding an issue. Executive agencies may be as particularistic in their orientation as congressional committees, and on most everyday matters such agency viewpoints may prevail.[79] But in developing legislative proposals and priorities, the executive is less likely to focus on the demands of narrowly-based groups and constituencies and more likely to perceive trade-offs between these demands and other objectives. The presumption in favor of "distributive" policies, in other words, and the tendency to recast proposals in a distributive mold, will be less pronounced than in the more decentralized and constituency-oriented Congress. At the same time, the executive has a superior capacity to resolve, or at least mute, conflict within its ranks and to move decisively in controversial policy areas.[80]

Two aspects of executive and commission activity help determine the incentives a committee has to seek an independent role. The first is simply the extent of involvement. Executive "neglect" of maritime affairs and the commercial fisheries, for example, helps explain congressional solicitude, while the roles of OTP and the FCC have sometimes preempted, sometimes hindered, congressional assertiveness in communications policy. But of course the content of the executive's moves is also important, and particularly its congruence with the expressed interests of the groups dominating the congressional landscape.[81] Communications and transportation regulation offer few incentives to extensive congressional involvement in any case, but the few instances of congressional intervention we have observed came when commission decisions upset perceived group equilibria (disadvantaging, for example, water carriers, charter airlines, and broadcast license holders). Similarly, administration attempts to consolidate and economize in areas such as AMTRAK and health policy provoked predictable responses among legislators attuned to the affected groups and constituencies.

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[75] For a summary of the evidence that the electorate has become "more informed and activist" since the fifties, see Richard W. Boyd, "Electoral Trends in Postwar Politics," in *Choosing the President*, ed. James David Barber (Englewood Cliffs: Prentice-Hall, 1974), Ch. 7.

[76] Mayhew, *Congress*, pp. 49–77.

[77] "Onto the old Magnuson, interested mainly in fishing, shipping, and Boeing aircraft, and running a rather sleepy committee, was grafted a new one: the champion of the consumer, the national legislative leader, and the patron of an energetic and innovative legislative staff." Price et al., *Commerce Committees*, p. 15.

[78] For fuller accounts, see the expanded version of the present article forthcoming in the Institute of Government Research Publication Series (Tucson: University of Arizona Press); and Price, et al., *Commerce Committees*, pp. 65–75.

[79] See Lester B. Salamon and Gary L. Wamsley, "The Federal Bureaucracy: Responsive to Whom?" in *People vs. Government: The Responsiveness of American Institutions*, ed. Leroy N. Rieselbach (Bloomington: Indiana University Press, 1975), pp. 151–88.

[80] See Lowi, "American Business," pp. 692–715; and Price, *Who Makes the Laws?* pp. 322–29.

A comparison of the relative mix of congressional and executive responsibility for consumer and environmental initiatives suggests certain refinements of these observations. As Table 3 shows, most consumer bills in recent Congresses have originated in the Senate Commerce Committee, while in environmental affairs legislators have commonly worked with measures proposed and developed by the administration. As already noted, the two areas offer similar incentives for congressional involvement, but efforts to make a mark in the environmental area are tempered by desires to avoid "extremism," to deflect conflict, and to compensate for a felt lack of expertise. Legislators thus tend to regard the administration's agenda-setting and formulation efforts as crucial to their own success in the area. As Senate Commerce's Chief Counsel Michael Pertshuk put it: "I think EPA was necessary in the environment area. There's the matter of heightening public awareness of these issues, and the technical complexities are so great. [In other words, the committee couldn't match its performance in the consumer area *without* an agency like EPA?] That's right."

It is important not to be overly deterministic about such matters: both branches can vary considerably in their handling of an area like environmental affairs, as the post-1972 role of EPA and the contrasting responses of House and Senate Commerce make abundantly clear. But the consumer affairs-environment comparison points up the impact executive activism can have on congressional incentives to enter a policy area. In short, as issues tend toward the upper left-hand cell of Table 2, legislators may often anticipate, and frequently will find they need, executive intervention.

Certainly the environmental protection case suggests that the relationship between executive and congressional assumptions of legislative responsibility is not to be viewed in zero-sum terms. Executive support may in fact be crucial in encouraging or facilitating a legislator's efforts, especially if general environmental conditions are unfavorable. Congressional champions of surface transportation and aviation deregulation, for example, have been visibly spurred in their uphill battles as Presidents Ford and Carter, responding to regulatory policy in terms of its inflationary and consumer impact, have stepped up their own efforts in these areas. Congressional intervention may also be rendered more likely and more promising by the possibility of exploiting divisions and conflicts *within* the executive. Commerce committee members often have little to lose and much to gain by securing strategic alliances at EPA, departmental consumer affairs offices, NOAA, NIH, CPB, and so forth.[82] The presence of *competing* FCC and OTP proposals for cable deregulation and development, for example, encouraged congressional interest and involvement. But the general point still holds: perceptions by legislators that the executive branch (or whatever forces are dominant within it) is likely to neglect a policy area which they deem important, or is likely to give short shrift to public or group interests which they favor, provide important incentives for "compensatory representation" and independent legislative involvement. This is true even where congressional dependence on executive formulation and information-gathering is extensive, as Senate Commerce's alteration of EPA's pesticide and toxic substances bills suggests.

Concluding Observations

We have argued that "environmental" factors—the degree of conflict an issue is thought to entail, its perceived salience to the electorate, and the level and direction of the executive's involvement—influence both the distribution of legislators' policy-making "investments" and the extent to which they take their bearings from broader public interests. Environmental conditions provide common incentives

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[81] Compare John Manley's discussion of "compensatory representation" on the revenue committees, *Politics of Finance*, pp. 354–68.

[82] See Price, *Who Makes the Laws?*, pp. 20–22, 26–31, 96–99, 189–94, 280–84, 327–29.

Table 3. Patterns of Committee-Executive Initiative on All Major Consumer and Environmental Bills Reported by the Senate Commerce Committee, 91st-93rd Congresses (1969–1974)

Proposal Initially Developed by:	Response of Administration			
	Oppose	Propose Less Extensive Bill	Endorse or Propose Similar Bill	Propose More Extensive Bill
Committee	No-fault auto insurance* Grants to states for consumer redress programs*** Protect petroleum franchises from supplier cutoffs* *Facilitate recycling***	Consumer product safety agency Increase FTC powers Warranty regulation Consumer class action suits** Automobile bumper standards and repair information Require no-cost repair of safety-related auto defects *Emergency chlorine allocation**	Extend NCPSC Toy safety Child-resistant packaging Cigarette advertising Fire safety *Safe drinking water* *Low-emission vehicles*	Food plant surveillance*

Proposal Initially Developed by:	Response of Committee			
	Oppose	Propose Less Extensive Bill	Endorse or Propose Similar Bill	Propose More Extensive Bill
Administration	*Power plant siting*†		*Regulate ocean dumping*	*Toxic substances control* *Pesticide control* Fish plant inspection*

Roman type = consumer bills; italics = environment bills.

*Passed Senate but not House.

**Reported by Senate Committee but did not pass Senate.

†Not reported by Senate Committee.

and constraints for a diverse range of members and create wide but predictable variations in how a committee handles the various policy areas under its jurisdiction. The picture of congressional policy making thus gained is not a complete or determinate one, however, and it is important to consider how the approach taken here might intersect with other modes of analysis.

In the first place, this essay has been deliberately one-sided in viewing legislators as rational "economic" actors and neglecting the "sociological" side of their existence, their lives as members of committees and chambers that superimpose certain roles and norms on their self-interested pursuit of the "profits" and "costs" offered by outside groups and the electorate. We have stressed how members respond as "maximizing individualists" to the threats and promises their environment offers. A fuller account would need to explore further the significant differences between the two Commerce Committees (and among various factions within them), particularly in their handling of the "publicly salient" areas of their jurisdiction. To explain these differences one would need to compare the recruitment patterns of the two committees, the collective norms into which members of the groups are socialized, the ways resources and authority are distributed within the organizations, the character and availability of staff services, the two committees' styles of leadership and partisan accommodation, and so forth.[83] Members must operate as participants in ongoing *institutional* traditions and processes. The present study suggests that Congress and its committees are increasingly permeable, as much shaped by as shaping the "economic" pursuits of the politicians who comprise them. Still, congressional policy making cannot be understood apart from the institution's *collective* life, for it is in that realm that the pursuit (or evasion) of policy-making responsibility may be decisively facilitated or deflected and, indeed, that the "maximizing" legislator encounters an additional, all-important group of "others" whose favor and support must be sought.

Secondly, however, even within the confines of methodological "individualism," economic rationality does not exhaust the possibilities of purposive behavior. Calculations of profit and cost as we have conceived them are probably only rarely absent from the thinking of most legislators. But they rarely require a single course of action and, in any event, often point to opportunities which legislators can modify or manipulate in light of their own purposes. "Member goals," in other words, must remain an independent variable in any analysis of the realities and potentialities of congressional policy making, but it is doubtful that these goals can be reduced to the simple and standardized form often assumed by "economic" and other "general" theories.[84] The values, orientations, and purposes of individual members and groups of legislators will continue to be an irreducible element in explaining both the shape of specific initiatives and more general patterns of congressional performance.

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[83] For a summary see Price, et al., *Commerce Committees,* Ch. 1.

[84] On this point compare Fenno, *Congressmen in Committees,* Ch. 1; Mayhew, *Congress,* pp. 2–6; and Price, *Who Makes the Laws?,* pp. 306–11.

Committee Decisions under Majority Rule: An Experimental Study *

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This article reports the findings of a series of experiments on committee decision making under majority rule. The committee members had relatively fixed preferences, so that the process was one of making decisions rather than one of problem solving. The predictions of a variety of models drawn from Economics, Sociology, Political Science and Game Theory were compared to the experimental results. One predictive concept, the core of the noncooperative game without side payments (equivalent to the majority rule equilibrium) consistently performed best. Significantly, however, even when such an outcome did not exist, the experimental results did not display the degree of unpredictability that some theoretical work would suggest. An important subsidiary finding concerns the difference between experiments conducted under conditions of high stakes versus those conducted under conditions of much lower stakes. The findings in the two conditions differed considerably, thus calling into question the political applicability of numerous social psychological experiments in which subjects had little or no motivation.

1. Introduction

Committee decision making constitutes a long-standing concern of political scientists.[1] Congressional committees have received a great deal of attention,[2] but committees at other levels of government have not escaped notice. Generally, it is fair to say that committee research is stronger on the empirical side than on the theoretical. The primary emphasis lies on understanding a specific committee and perhaps the larger political system (e.g., Congress) within which the committee exists. Only a secondary emphasis lies on formulating generalizations which could be applied to committee situations removed from those studied. And seldom is a committee study regarded simply as a data source against which broad theoretical generalizations could be checked.[3]

One finds quite a different emphasis in the mathematics, operations research, and economics literature. There one discovers numerous general models of committee decision processes, most of which build on game-theoretic and social choice-theoretic ideas.[4] But why are the general principles found here so seldom applied by political scientists? It is true that the theoretical literature is quite technical, and most political scientists have little mathematical training. But we daresay that political scientists would be crowding into mathematics classes if they really believed that the technical literature had much to offer.

No doubt a more important reason for the lack of contact between mainstream political science and the technical literature is the difficulty in evaluating the latter. The models embody a curious mixture of positive and normative concerns. They are developed and advanced without a hint of possible operational definitions—one can find proof upon proof, but one searches in vain for a detailed discussion of exactly how and when a model should be applied. By relying almost solely on logical validity, the technical literature has spawned many generalizations. But applied scientists simply cannot tell which of these might be usefully employed in field research. And they have neither the inclination nor the training to familiarize themselves with them all.

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*The financial support for this research was provided by the National Science Foundation, Economics and Political Science Programs. We gratefully acknowledge this support. For their invaluable research assistance and healthy skepticism we thank Randy Calvert, Linda Cohen, James Hong, and Darwin Niekirk. We also wish to thank John Jackson whose comments helped shape some aspects of this research.

[1] Woodrow Wilson, *Congressional Government* (New York: Meridian, 1956).

[2] Ralph K. Huitt, "The Congressional Committee: A Case Study," *American Political Science Review*, 48 (June 1954), 340–65; Richard E. Fenno, *The Power of the Purse* (Boston: Little, Brown, 1966).

[3] For example, in the introduction to *The Power of the Purse*, Fenno writes (p. xiii):

> The aims of the book are threefold. In order of their likely relevance and persuasiveness for the reader they are: first, to provide an empirical description of the contemporary appropriations process in Congress; second, to demonstrate the importance of the committee-centered analysis for increasing an understanding of Congress; and third, to suggest the usefulness of certain bits of theory for students of Congress and its committees.

[4] Charles R. Plott, "Axiomatic Social Choice Theory: An Overview and Interpretation," *American Journal of Political Science*, 20 (August 1976), 511–96; William Riker and Peter Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1973), passim; Peter Fishburn, *The Theory of Social Choice* (Princeton, N.J.: Princeton University Press, 1973); John Ferejohn and Morris Fiorina, "Purposive Models of Legislative Behavior," *American Economic Review Proceedings and Papers*, 65 (May 1975), 407–14.

If theorists blithely continue to prove more theorems, and applied scientists doggedly continue to gather more data, at some point data and theory might miraculously conjoin. But we regard such a union as more likely to result from a determined effort than from a fortuitous accident. We have begun a program of research intended to bring together formal models and actual behavior. In this paper we report on the initial section of our work, a program of experimental research into the decision processes of small committees operating under majority rule.

We are quite aware of (and even share) the discipline's skepticism about the usefulness of experimental methods. What makes us believe, for example, that we can use college students to simulate the behavior of Congress members? Nothing. Our beliefs are much more modest. We intend to use the laboratory as a screen for basic ideas: if a given model does not predict well relative to others under a specified set of conditions in the controlled world of the laboratory, why should it receive preferential treatment as an explanation of non-laboratory behavior occurring under similar conditions? While laboratory success by no means implies field study success for a model, laboratory failure raises grave doubts about a model's applicability in field studies. Thus, while we reject the suggestion that the laboratory can replace creative field researchers, we do maintain that it can help them decide which ideas deserve further consideration.

There are a number of other problems which we do not wish to avoid. Rather than raise them in an introduction, however, we prefer to consider them in the light of what we have done. Thus, additional discussion of our particular brand of experimental methodology appears in the concluding section of this article.

2. The Committee Process

We constructed the experimental setting to be a simplified case of the general class of committees which we wish to understand. This class of committees is defined by four conditions:

a. Individual committee members are not uncertain about the consequences of any committee decision, i.e., they fully understand the effect which alternative committee decisions will have on them.

b. Committee members have clear, constant preferences over the objects of the committee decision. The committees are making decisions about things that matter to them. They are not solving problems or making judgments about things for which they have no personal preference.

c. There is no previously fixed parliamentary procedure other than minimal rules of order and majority rule.

d. There are no extra-committee agreements or private deliberations among subsets of committee members.

Basically, we wish to explain what goes on within the "black box" of committee decision making. We focus on the period after biological, sociological and psychological processes have operated to instill clear preferences in committee members. The question then is simply how a committee whose members have clear, conflicting preferences arrive at a decision in the simple institutional setting we create. Clearly, the political world we have designed lacks the institutional complexity of more naturally occurring political committees, and it is possible that the existence of alternative conditions might induce behavior different from that which occurs in our simple situations. But we believe that our experimental design captures the essence of the basic majority rule committee process. And, as it turns out, for this most basic committee process several models work quite well indeed, while many other proposed models do not work at all.

3. Experimental Procedures, Setting and Design

Procedures and Subjects. The research consists of 65 committee decision-making sessions. All the committees were five-person and all made decisions by majority rule. Subjects in the experiments were typically students from the University of Southern California, Pasadena

City College, California State University at Los Angeles, and Los Angeles City College.[5] They were recruited from the classes of cooperating instructors and in cafeterias and other areas where students congregate. Each subject was permitted to participate in only one experimental session. Post-experiment conversations with instructors and subjects indicated that the subjects were cooperative in declining to discuss the experiment with outsiders after participating. Thus, subjects generally knew little or nothing about the experiment before reading the instructions.

Recruits were asked to appear at a designated time and place. At the maximum, four experiments were conducted simultaneously (20 subjects) but a few extra subjects always were recruited in order to assure the necessary number. Extras were paid $2.00 for time and trouble and dismissed (none was permitted to observe). Once the recruits were assembled, assignments to committees and assignments to positions within committees were determined randomly. After reading the instructions (Appendix A) each committee went to a separate meeting room where deliberations immediately began.

Experimental Setting. The substance of the decision was simple. Each committee was asked to select a single point on the blackboard by majority rule. A coordinate system was drawn on the blackboard and each subject was assigned a payoff function defined over ordered pairs of the coordinate system. That is, subject i was assigned a payoff function $U^i(x_1,x_2)$, $i = 1, 2, 3, 4, 5$, which indicated the amount of money he or she would receive from the experimenter expressed as a function of the point, (x_1,x_2) chosen by the committee. (See Appendix B for an example, and Appendix C for all functions.) The instructions clearly stated that no threats, side-payments or post-experiment deals were permissible. In fact, our instructions prohibited any discussion of monetary amounts.[6] So as long as individuals prefer more money to less, their preferences for points on the blackboard are represented[7] by the functions $U^i(x_1,x_2)$.

This setting seems to assure that our committees were of the type designated by conditions (a) and (b) in section 2. There appears to be little uncertainty and, as long as the functions $U^i(x_1,x_2)$ have adequate slope, there is no general indifference. The method of recruitment assured that condition (d) was satisfied. This leaves only (c) open to operational question.

The committees made their decisions under a simple set of parliamentary procedures. Each committee began at the point (200, 150). That is, the status quo in each experiment was the extreme northeast point in the issue space. Upon recognition by the chair (experimenter), any subject could propose an amendment (no second required) to the motion on the floor. If it passed (possibly following discussion in sessions where discussion was allowed), the amendment became the new motion on the floor and the process continued. At any point during the experiment a motion to end debate was in order. If a majority consented, the motion on the floor was then voted up or down. If the motion passed, the experiment ended, and if it failed, the floor was again thrown open for amendments. Thus, each of our committees pushed a point around the blackboard until a majority voted to quit and go home.[8] Subjects were paid in cash at the conclusion of the experiment.

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[5] An occasional working adult (e.g., gardener, secretary) appeared among our subjects. Our subjects included members of racial minorities and women. We are aware that some social psychologists prefer subject populations to be racially or sexually homogeneous. But the class of models in which we are interested gives us no reason to differentiate among blacks, whites, chicanos, men and women.

[6] This restriction serves two functions. First, it enhances control over the experiment. Outside deals which induce preference changes in subjects but not in the models are made difficult to negotiate. Furthermore, social stigmas regarding monetary endowments which also can induce uncontrolled preferences are minimized when monetary amounts are not public. Secondly, and perhaps more importantly, a cardinal measure of returns is not a general property of a broad range of committee processes. As in our experiments political decision makers can learn the *order* in which their fellow decision makers regard alternative proposals. They can attempt to communicate intensity through anger, other displays of emotion, and effort. But objectively given cardinal measures of returns are generally absent.

[7] For all (x_1, x_2) and (x'_1, x'_2) we know that $(x_1, x_2)R_i(x'_1, x'_2)$ if and only if $U^i(x_1, x_2) \geqslant U^i(x'_1, x'_2)$. So the binary relation R_i is the preference relation.

[8] It is interesting to compare this stopping rule with that used by Peter Halfpenny and Michael Taylor, "An Experimental Study of Individual and Collective Decision Making," *British Journal of Political Science*, 3 (1973), 425–44. In their experiments Halfpenny and Taylor operationally defined an equilibrium as a 5-time winner, i.e., the experiment ended when a motion on the floor had defeated five alternative

The above-described process allows the parliamentary procedures to be essentially endogenous and as "natural" as possible, consistent with order. The recognition feature was followed only loosely, with many informal proposals made and rejected during discussions in those experiments without communication restrictions. A variety of more complex procedures can be found in use, and the behavior of committees under each must ultimately be examined. But we take our procedures to be an operational interpretation of condition (c).

Thus, we created five-person committees, induced preferences for outcomes among the committee members, and gave them a decision rule (absolute majority rule). Beyond this they were free to do what they wanted. They sat together and communicated openly. If they wished to form coalitions, fine. If they wished to be "fair," fine. If they wished to attempt to maximize group gain, fine. If they wished to do nothing, fine. The lack of structure imposed by our instructions was demonstrated by the fact that subjects regularly asked "What are we supposed to do?" ("Get what's best for ourselves?" "Do what's fair?" etc.) We shrugged off such questions with a poker-faced, "Whatever you want," and read again the relevant sections of our instructions.

4. Experimental Series and Competing Models

Experimental Series. The three separate series reported here differ according to the induced preference configuration. These differences are reflected in Figure 1 for Series 1, Figure 2 for Series 2, and Figure 3 for Series 3, where the five points are the points of maximum payoff for subjects 1, 2, 3, 4 and 5, respectively. The circles on Figures 1 and 3 and the ellipses on Figure 2 indicate the shape of the indifference

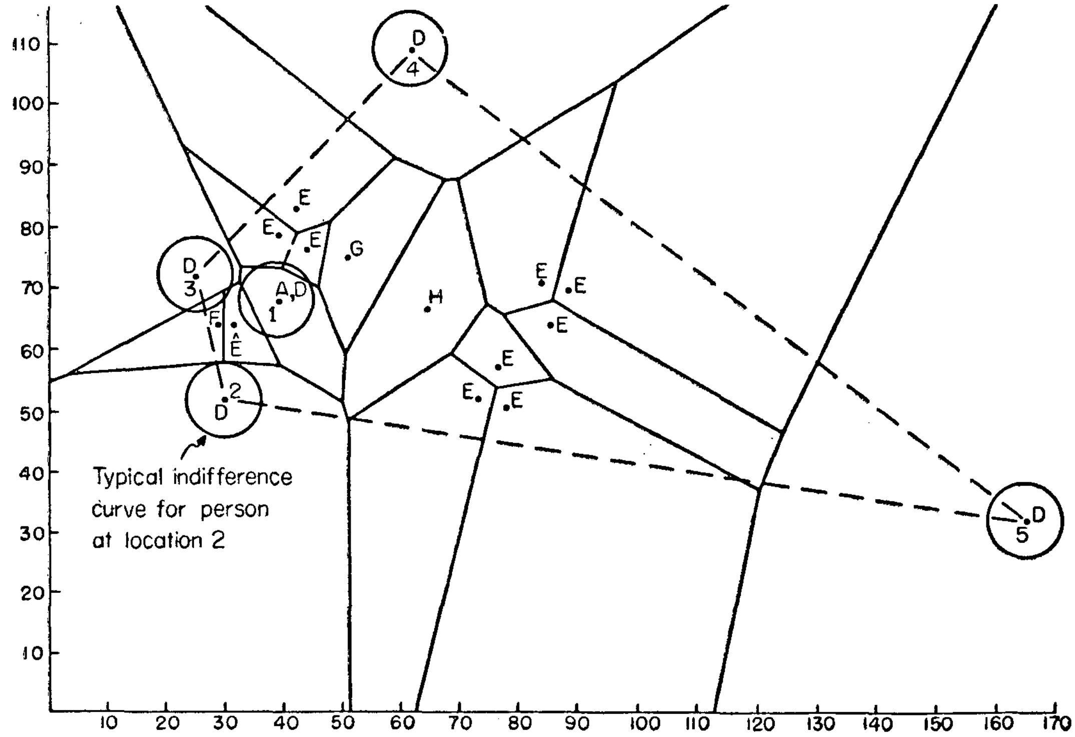

Figure 1. Series 1. Preference Configuration, Predicted Points and Partition of Space into Areas Closest to Predicted Points

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motions. In our Series 1 experiments 24 proposals in 17 experiments defeated 5 or more alternatives but did *not* go on to become the group decision. Thus, 17 of 40 experimental outcomes would have been different had we taken action to stop the experiments rather than allowing them to take their course (and assuming such a rule left individual behavior unchanged). This example graphically illustrates the potential impact of procedural variations on experimental outcomes.

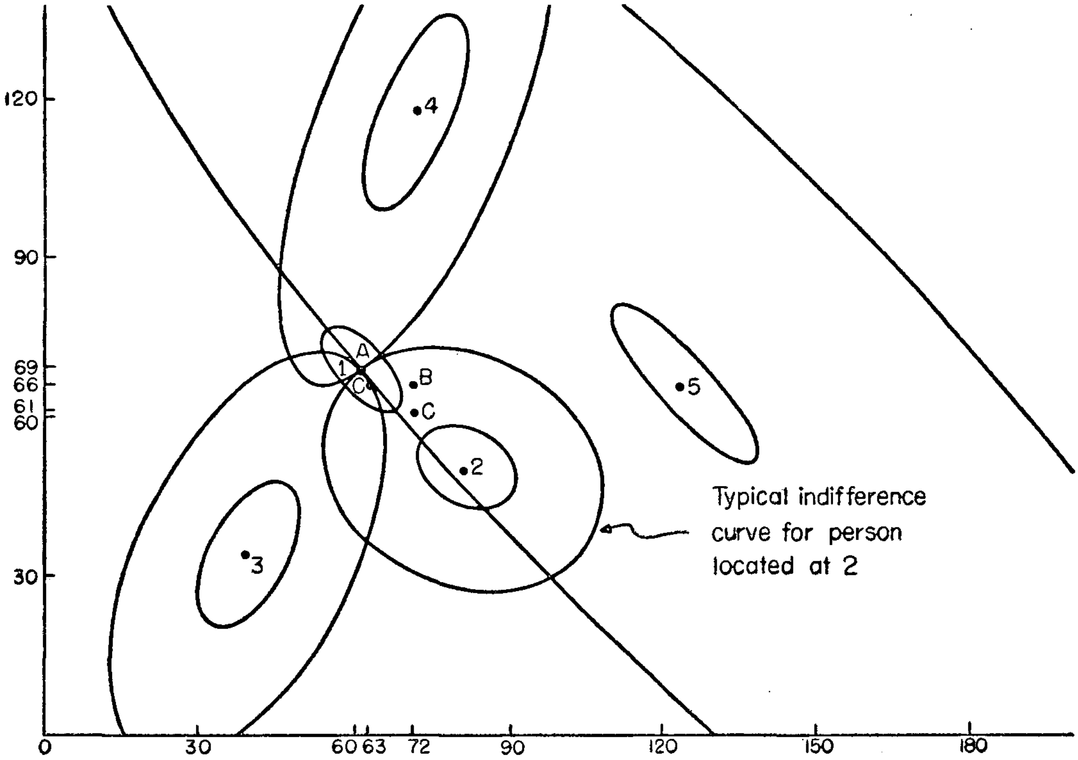

Figure 2. Series 2. Preference Configuration

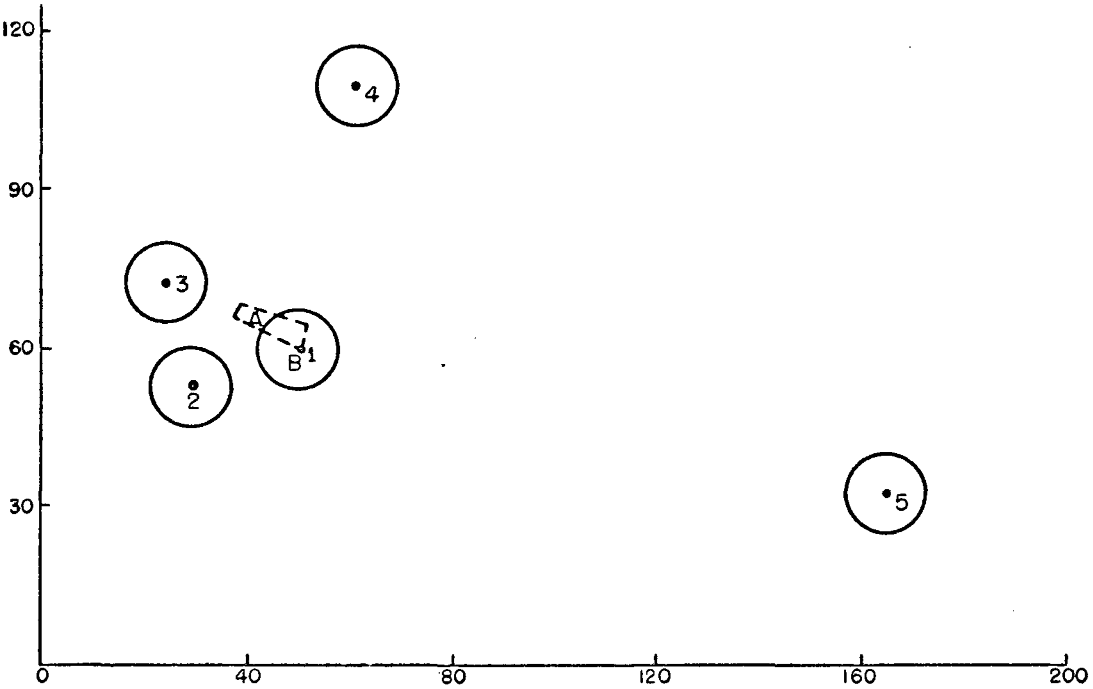

Figure 3. Series 3. Preference Configuration

curves. The particular shapes and configurations were chosen strategically, as will be explained below.

Pilot experiments as well as some theoretical concerns suggested that in certain respects committees with small incentives behaved differently from committees with large incentives. In addition, pilot experiments suggested that the amount and type of communication made a systematic difference. This led to the examination of two different payoff magnitudes and two different communications conditions.

The 40 experimental sessions of Series 1 were divided into 4 subseries of 10 experiments each. The high-payoff condition employed payoffs similar to the example contained in Appendix B. The low-payoff condition specified much lower maximum payoffs and a smaller rate of loss as the outcome departed from the maximum. Appendix C gives full details. The communication condition varied from full communication—in which subjects could talk about anything they wished, other than the monetary aspects of their preferences—to no communication. In the latter condition subjects could do nothing except make proposals and vote.

As discussed below, Series 1 confirmed the initial hypothesis that low-incentive committees and high-incentive committees behave substantially differently. Because the models with which we are most familiar were most applicable to the high-payoff case, and because the communication condition seemed to make little difference in this case, we chose to follow the high-payoff, full-communication path in the remainder of our experiments.

Series 2 and 3 were both constructed to discriminate among models which gave identical, accurate predictions in the high-payoff case in Series 1. The logic behind these particular designs is explained in the following section.

Competing Models. Given our simple setting, there are a remarkable number of potentially applicable models which differ from one another in their assumptions about the behavior patterns of committee members. Although these models are motivated by reasonably well-articulated theories, it is often unclear how these theories apply to our situation, if at all. Thus, of necessity, *we* have taken the liberty of identifying predictions which incorporate the spirit of the respective theories. Possibly we have erred. If so, we invite aggrieved parties to correct us.

We have divided the available models into two broad classes according to the nature of the motivating theory. The theories which lie behind the first class, called *egoistic*, have the common view of decision makers as self-interested maximizers, while the second class, *non-egoistic*, presupposes that committee members look beyond their individual interest to some type of collectively optimal, or consensus outcome. Egoistic theories can be subdivided further into four classes: game-theoretic, coalition-theoretic, voting-theoretic, and finally, agenda-based voting-theoretic.

Egoistic: Game-Theoretic. The theory of games without side payments and without transferable utility induces a natural way of modeling our committees. One may take the majority rule preference relation as the dominance relation. (There are other ways to form a dominance relation in our setting if patterns of communication, etc., are considered, but we propose this one as most natural.) From this base there are several alternative models, two of which are listed here.

1. *Core.* The core is the set of all undominated points.[9] In our experiments the core is point A in Series 1 and 2, and does not exist in Series 3.

2. *Van Neumann-Morgenstern Solution.* This set of points possesses internal and external stability properties.[10] If a core exists, it will be included in all N-M solutions. In Series 1 and 2 there is a unique N-M solution, point A. In Series 3 we have not as yet succeeded in computing the main simple solution.

Egoistic: Voting-Theoretic. These models purport to specify which outcome will be adopted by a group of individuals each of whom votes sincerely, i.e., in accord with his or her personal preferences.

3. *Voting Equilibrium.* Introduced by Black and formally developed by Plott and Sloss, a voting equilibrium is a point which can be beaten by no other point in a binary contest characterized by sincere voting.[11] Such a point is A on Figures 1

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[9] R. J. Aumann, "A Survey of Cooperative Games Without Side Payments," in *Essays in Mathematical Economics in Honor of Oskar Morgenstern,* ed. M. Shubik (Princeton, N.J.: Princeton University Press, 1967).

[10] Aumann.

[11] Duncan Black, *The Theory of Committee and Elections* (Cambridge: Cambridge University Press,

and 2. No such point exists on Figure 3. From a mathematical point of view the equilibrium is equivalent to the core of the appropriate cooperative game. But the core develops from concepts of coalition and joint strategy, whereas these concepts play no conceptual role in the theory of voting equilibrium.[12]

4. *Min-Max Set.* Developed by Simpson, this model predicts that the committee will gravitate to a set of points which have the property that the maximum number of individuals who would benefit from a move to some other point is minimized.[13] The theory predicts point A for all except Series 3, where it predicts the set of points in the quadrilateral at the center of Figure 3.
5. *Equilibrium under a City Block Metric.* There are suggestions that the city block metric is in some sense more natural than the usual Euclidean metric, and that decision makers might impose such a metric even if there are indications that other metrics are more appropriate.[14] If committee members' indifference curves are given by the city block metric, then the equilibrium under majority rule with an odd number of voters is the unique intersection of the perpendicular hyperplanes through the median voter along each dimension. This point is A in Series 1 and B in Series 2 and 3.

Egoistic: Agenda-Based Voting-Theoretic. The agenda procedures used by a committee can influence if not completely dictate the decision.[15] This fact raises the possibility that there is some "natural" agenda which committees tend to use even if it is not formally specified. A general model of committee decisions would be interpreted as having two steps. First, there would be a prediction about which agenda the committee would adopt, and then, secondly, a prediction about how the committee will behave once an agenda has been adopted.

6. *Obvious Agenda.* Each individual will propose his or her best point at some time during the meeting. In particular, individual 1 will propose his or her best point: A. Notice that point A is preferred by a majority when put against any other point. Thus, if individuals vote sincerely, which tends to be the case with a fixed agenda,[16] A will win whenever it is proposed. Although this reasoning suggests point A as a prediction for Series 1 and 2, it makes no prediction for Series 3.
7. *Dimension by Dimension Median.* A "natural" procedure might be to choose a point along one dimension by majority rule, then, holding that point fixed, choose a point along a second dimension, hold it fixed, and continue in similar fashion until a choice has been made along each dimension. If committee members vote sincerely, the chosen point would be A in Series 1, B in Series 3, and points C in Series 2. (This model can make as many as n! predictions, where *n* is the dimensionality of the alternative space, depending upon the order in which the dimensions are considered.)
8. *Sincere Binary Voting and Arbitrary Proposals.* Suppose all motions involve a comparison of two outcomes—a motion

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1958); Duncan Black and R. A. Newing, *Committee Decisions with Complementary Valuations* (London: William Hodge, 1951); Charles R. Plott, "A Notion of Equilibrium and Its Possibility under Majority Rule," *American Economic Review,* 57 (September 1967), 797–806; Judith Sloss, "Stable Outcomes in Majority Rule Voting Games," *Public Choice,* 15 (Summer 1973), 19–48.

[12] To draw a theoretical distinction between the formally equivalent concepts of core and voting equilibrium might strike some of our colleagues as unnecessary hair-splitting. To these skeptics we address one further point. In the typical case in which no core/equilibrium exists, where does one turn? If one has arrived at the core through the game-theoretic literature, one naturally turns to some other game-theoretic concept such as the N-M solution. But if one has arrived at the equilibrium through the voting-theoretic literature, one turns to some other voting-theoretic model such as the min-max set or an agenda theory. When it exists, the core/equilibrium is the intersection of several branches of theory. But that is not to deny that these branches are distinct.

[13] Paul B. Simpson, "On Defining Areas of Voter Choice," *The Quarterly Journal of Economics,* 83 (August 1969), 478–90.

[14] Douglas Rae and Michael Taylor, "Decision Rules and Policy Outcomes," *British Journal of Political Science,* 1 (1971), 71–90; also, Halfpenny and Taylor, "An Experimental Study."

[15] Charles R. Plott and Michael E. Levine, "On Using the Agenda to Influence Group Decision: Theory, Experiments and Application," *American Economic Review,* forthcoming.

[16] Plott and Levine, "On Using the Agenda."

on the floor and an amendment as outlined above. Amended motions are the new motion on the floor and are subject to amendment. Suppose further that individuals always vote sincerely and that amendments arise in some type of "hill climbing" fashion. The idea behind the model is, then, that the majority-dominance relation in some sense "drives" or "forces" the motion on the floor. For Series 1 the model predicts A. Because there is a transitive majority winner, with a "majority best" alternative, every chain of proposals leads there. For Series 2 the prediction is not clear because in this design cycles exist. Not every sequence in Series 2 will necessarily lead to the core or equilibrium. In Series 3 there is no core or equilibrium. McKelvey's theorem tells us that *any* two points in Series 3 can be connected via some majority-rule sequence.[17] Thus, we interpret the model as saying that "anything can happen" in Series 3.

9. *Agenda Manipulation.* Plott and Levine hypothesize that some individuals have good intuition about the influence of the agenda.[18] When no formally imposed agenda exists, the agenda becomes part of each individual's strategy set, and because it is known to greatly influence the outcome, the individual with the best intuition can use it to personal advantage. From this line of reasoning we predict the set of points D of individual maximums.

Egoistic: Coalition Theories. Coalition theories agree that the decision will be made as if it results from a two-step process—although they might differ as to which step comes first. One step is the formation of a coalition with the power to institute decisions. The other step is the choice of decisions for the coalition to implement.

10. *Minimum Winning Coalitions.* The idea developed by Riker is that only coalitions of minimal winning size will form.[19] We take it that the implemented decision will be the "center" of the formed minimal winning coalition. There are ten such coalitions, so the model predicts that the outcome will be one of the ten points labeled E on Figure 1. This model was rejected in favor of other models in Series 1. Thus, its predictions were not carried forward to Series 2 and 3. (Any model rejected in one series was eliminated from further consideration.)

11. *Resource Coalition Theories.* Sociologists such as Gamson, and Komorita and Chertkoff hypothesize that once a coalition forms, the payoff to coalition members will follow an equity norm or parity norm based on the resources supplied by each coalition member.[20] In our experiments the only "resource" a committee member possesses is a vote, and the situation is clearly one person—one vote. Thus, we interpret these models as identifying the point equidistant from the best point of each person in a coalition. Given these points, each person can rank each coalition according to his or her expected payoff in it. If some coalition is ranked best by all of its members, there is a presumption that it will form and behave according to expectations. In our experiment players 1, 2, 3 should join and choose $\underline{E}$.

12. *Minimal Winning Coalition of Maximum Value.* The coalition which could achieve the largest total payoff is {1, 2, 3}. The point F achieves this coalitional maximum.[21]

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[17] Richard McKelvey, "Intransitivities in Multidimensional Voting Models and Implications for Agenda Control," *Journal of Economic Theory*, 12 (1976), 472–82.

[18] C. R. Plott and Michael E. Levine, "On Using the Agenda."

[19] William Riker, *A Theory of Political Coalitions* (New Haven: Yale University Press, 1962).

[20] William Gamson, "A Theory of Coalition Formation," *American Sociological Review*, 26 (1961), 373–82; S. S. Komorita and Jerome M. Chertkoff, "A Bargaining Theory of Coalition Formation," *Psychological Review*, 80 (May 1973), 149–62.

[21] Obviously, our procedures (no communication of monetary amounts) work against any theory which specifies that the committee or a subgroup of it tries to achieve a group maximum (models 12 and 13). Of course, the political world also works against any such theory in that cardinal measures of preferences which group maximization theories presuppose are typically not available (footnote 3). And even if objectively known monetary payoffs are available, there still is no theoretical justification for asserting the equivalence of a maximum of group monetary payoff and group utility payoff. But for reasons discussed under model 13 we can not dismiss out-of-hand models which contain group or subgroup maximization processes.

Non-Egoistic. These theories have in common the premise that principles more general than individual selfishness explain group decision making. We content ourselves with four possibilities.

13. *Maximum Group Return.* Even though individuals cannot express monetary amounts, they can and do express the intensity of their feelings by shows of emotion, willingness, or unwillingness to cooperate, etc. In such ways the group interaction could carry the process to a point which maximizes group return.[22] If such behavior is observed, a deeper explanation could proceed in terms of some concept of group welfare. This is point G in Series 1.
14. *Fair Point.* Pilot experiments suggested that a likely candidate for a "fair" outcome was the average of the points of individual maximum. This point also happens to be the centroid, the point which minimizes the sum of Euclidean distance from the points of individual maximum.[23] In each series this point is designated H.
15. *Dominant Personality.* Perhaps some individuals have such strong personalities that deference overrides any other incentives experimentally induced in the other individuals. The idea, as presented here, is in part a "straw man" since we have found no clear reference from which to draw it. Nevertheless, a feeling that variables like this are important seems pervasive. If it is true that individual preferences are substantially influenced in group interactions, then we would be prepared to look in this direction for an explanatory model. We take the prediction here to be neighborhoods around the individual maxima, points D.
16. *Obvious Point.* Schelling's idea can be adapted to our scenario.[24] Is there a "prominent" or "obvious" place to go—perhaps other than one of the points outlined above? In each series there is one individual maximum in the interior of the convex hull of the individual maxima. We take the point A in Series 1 and Series 2 and point B in Series 3 as the obvious point predictions.

5. Experimental Results

Series 1. The Series 1 design affords an opportunity to make a first broad cut among the numerous models discussed in the preceding section. The design is such that the egoistic voting-theoretic, agenda-based voting-theoretic and game-theoretic models conflict with the egoistic coalition-theoretic and non-egoistic models. As mentioned above, pilot experiments suggested that both payoff size and amount of communication affected experimental outcomes. Thus, although most of the candidate models say nothing terribly precise about these variables, a class of models might possibly work well under one set off payoff/communication conditions, but not others. For example, we anticipated that the entire class of non-egoistic theories would perform better with communication and with low payoffs. Conversely, restricted communication and high payoffs would appear to maximize the chances of a good performance by certain of the egoistic theories. Thus, we conducted 40 experiments, 10 each in the 4 combinations of low/high payoffs, no/free communication described in section 4.

Table 1 contains the outcomes for the 40 experiments of Series 1. The findings for the low and high payoff conditions differ, so we will discuss them separately.

The mean outcome in the high-payoff condition is almost exactly the point, A, of Figure 1. No other model of those under consideration predicts the experimental outcomes nearly so well. The apparently complex but conceptually simple Figure 4 graphically illustrates the comparative performance of the three classes of models. In Figure 4 each point identified in Figure 1 is enclosed by a shape which separates all points closer (in Euclidean distance) to it than to any other predicted point. For purposes of display we have pooled the observations from the two communications conditions. Clearly, the point A wins hands down. Fourteen of 20 outcomes are closer to it than to any other predicted point. One outcome is closer to the committee maximum than to any other point. Of the remainder, two are closest to the

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[22] Bernhardt Lieberman, "Combining Individual Preferences into a Social Choice," in *Social Choice*, ed. Bernhardt Lieberman (New York: Gordon and Breach, 1971), pp. 5–25.

[23] R. A. Hanson and P. M. Rice, "Policy Outcomes in Decision Making Groups," paper delivered at the annual meeting of the American Political Science Association, 1972.

[24] Thomas Schelling, *The Strategy of Conflict* (Oxford: Oxford University Press, 1960).

Table 1. Experimental Outcomes: Series 1

	High Payoff		Low Payoff	
	Communication	No Communication	Communication	No Communication
Outcomes	(34,69)	(37,72)	(50,70)	(62,109)
	(36,66)	(39,68)*	(60,80)	(30,52)
	(32,65)	(55,71)	(39,66)	(45,68)
	(40,70)	(32,70)	(39,68)*	(39,68)*
	(46,68)	(40,71)	(39,68)*	(39,68)*
	(39,68)*	(40,67)	(25,72)	(30,60)
	(34,69)	(40,67)	(40,68)	(33,70)
	(37,70)	(25,65)	(100,70)	(30,65)
	(43,70)	(30,70)	(50,85)	(30,70)
	(30,68)	(39,68)*	(31,70)	(25,72)
Mean	(37,68)	(38,69)	(47,72)	(36,70)
Standard Deviation	5.2	8.3	21.9	(17.3)
*Majority Rule Equilibrium				
Location	(39,68)	(39,68)	(39,68)	(39,68)
Total Payoff	$37.86	$37.86	$32.49	$32.49
Group Mean				
Location	(64,67)	(64,67)	(64,67)	(64,67)
Total Payoff	$33.41	$33.41	$32.44	$32.44
Group Maximum				
Location	(51,75)	(51,75)	(75,68)	(75,68)
Total Payoff	$57.95	$57.95	$32.89	$32.89

mean of the (1, 2, 3) minimal winning coalition, two to the joint maximum of the (1, 2, 3) coalition (although one falls outside the Pareto optimals), and the last is closest to Player 3's ideal point.

Table 1 suggests rather strongly that when individuals have a good deal at stake, one had better model their behavior via a theory which includes point A among its predictions. The Series 1 high-payoff experiments provide grounds for rejecting models 9–15, in favor of 1–8, 16. Moreover, the Series 1 results also suggest a subtle way of distinguishing between models 1 and 2, and models 3–8. Note that the point, A, appears equally good as a prediction in *both* the communication and non-communication cases. This apparently supports the view that the processes driving the results are more likely to be similar to those underlying the voting-theoretic models than to those underlying the game-theoretic models, given that the latter seem to presuppose coalitions and joint strategy choices, i.e., a generally high degree of communication and coordination.

What about the low-payoff experiments? When full communication is permitted, the mean outcome is much farther to the right (47, 72) than in the high-payoff condition. Some committees attempt to accommodate the extreme individual.[25] Thus, charity or altruism apparently has its price and in these experiments, that price lies somewhere between a few cents and a few dollars per unit. When communication is not permitted the mean outcome is similar to that in the high-payoff case, although the variance is much higher. Thus, there is an interaction between communication and payoff conditions, with the difference between the payoff conditions being less in the absence of communication.

Overall, though, the differences in payoffs appear to be a more significant influence on the outcome of the committee process than differences in communication. Given high payoffs, the egoistic theories work equally well with or without communication. Given low payoffs, all

[25] Interestingly enough, this seeming altruism has an ironic twist. Subjects often made the incorrect assumption that their payoffs were identical. But the asymmetry in payoff structures (Figure 2) leads to the result that if Players 1, 2, 3 heeded the pleas of Players 4 and/or 5 to move farther north and/or east than the equilibrium, then the fringe players got significantly more in absolute terms than the more centrally located players, although the outcome remained relatively closer to the latter.

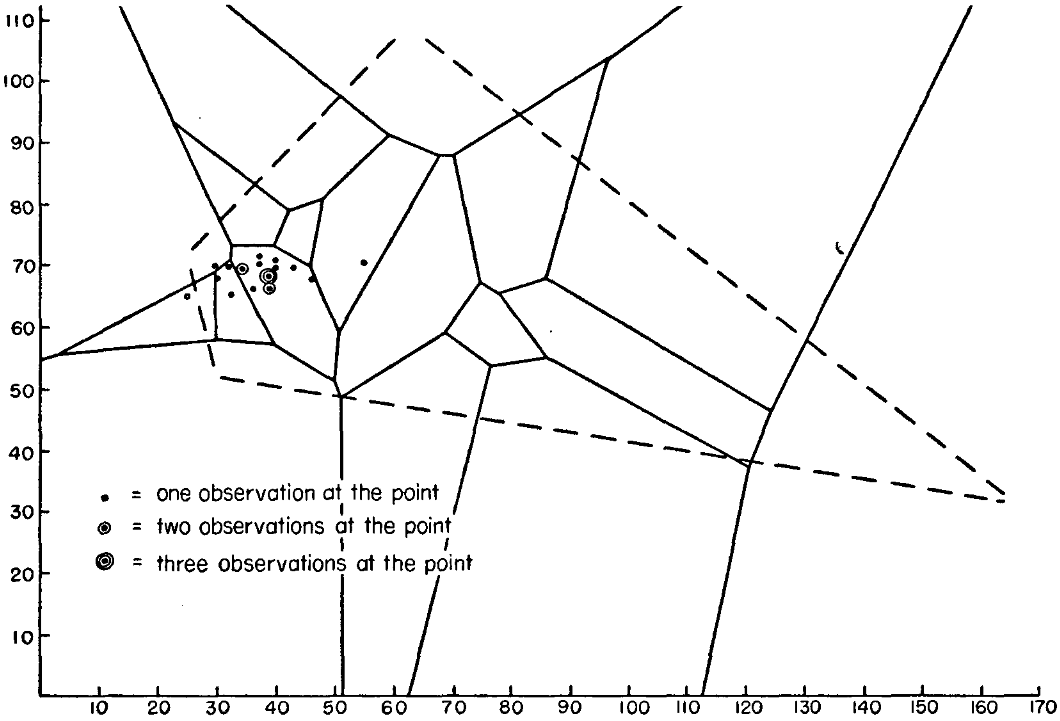

Figure 4. Series 1. High Payoff Pooled Observations

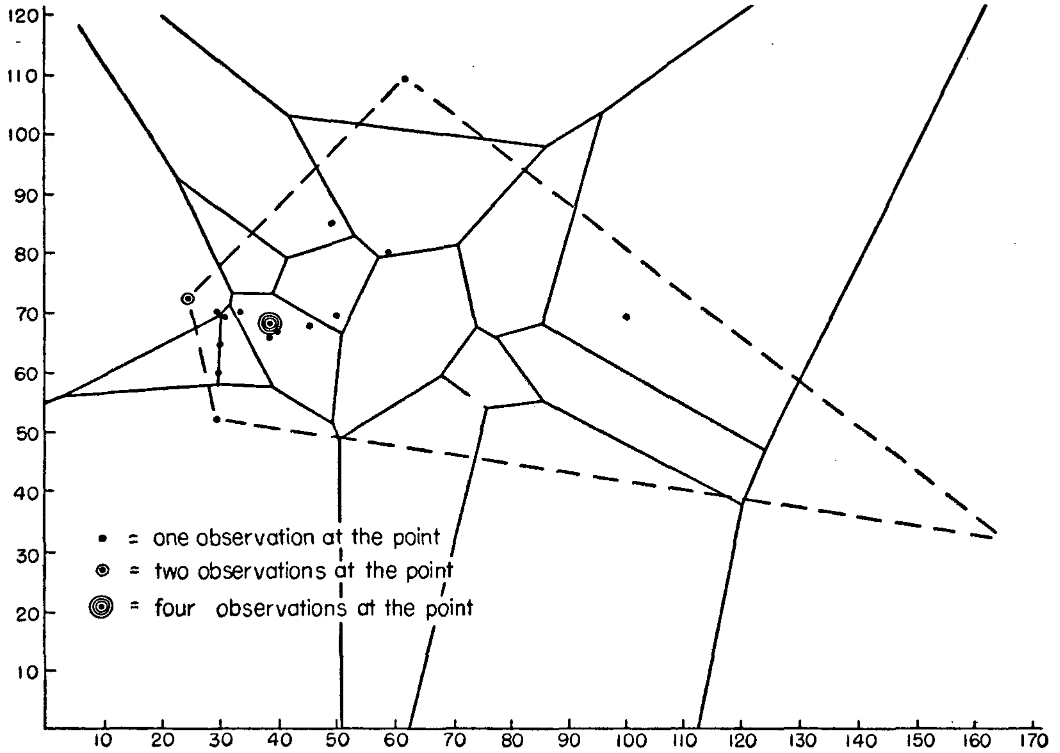

Figure 5. Series 1. Low Payoff Pooled Observations

the models work rather poorly with or without communication. The best of a bad lot of models in the low-payoff condition are the "agenda manipulator" and the "dominant personality" models—8 of 20 outcomes fall on individual ideal points. Strangely, though, 5 of these 8 outcomes occur in the *no* communication case where the operation of personal factors presumably is minimal. Thus, the agenda manipulator appears to be somewhat more plausible than the personality theory.

In addition to experimental outcomes, there are other more impressionistic findings to report. As one would expect, behavior in the experiments differed between the communication and non-communication conditions. In the HP non-communication condition subjects proposed and voted on amendment after amendment (mean 28). In the communication condition the subjects made fewer official amendments (mean 16) but actually considered many more informally. Subjects often became quite involved with the decision in the communication condition. They cajoled, pleaded, sometimes wrote on the board, invented fair division schemes (usually consistent with their own self-interest) and generally created an interesting and enjoyable observational context for the experimenter. Frequently "leaders" emerged in the communication condition. These were people who took it upon themselves to gather and organize information from the other subjects and "work out" an acceptable decision. Contrary to naive expectations, these individuals did not generally do better than non-leaders.

Coalitions were very rare in the Series 1 experiments. On only a handful of occasions did we observe a subgroup of the committee arrive at a decision without regard for the wishes of the remaining committee members. The atmosphere of the experiments tended to be friendly and cooperative; irritation sometimes showed, but seldom did personal animosity surface. Even suggestions about coalitions (e.g., "let's end it; we've got the votes") typically were rebuffed by one or more committee members. Perhaps if we were to encourage subgroups of our committees to bargain privately we might find more coalitional activity, but for the experiments reported, we must conclude that coalition theories do not provide a basis for explaining either the committee processes or outcomes.

Finally, we found it interesting that subjects sometimes resorted to intellectual short-cuts such as satisficing in our experiments (e.g., "I drew this square around my ideal point and voted against anything outside of it and for anything inside"). In these experiments each subject's preferences were clearly laid out with dollars and cents figures available for each comparison of alternatives. Nevertheless, some subjects opted for simpler ways of making a decision. In the real world such difficulties are compounded. How many of us know that our preference for one Defense Procurement Bill is 3.62 and for another, 3.95? How much more likely are rule-of-thumb intellectual short-cuts in such situations? Interestingly, nearly all the formal models of committee decision processes assume away costs associated with gathering information, identifying one's preferences and reaching a decision. Yet these factors loom very large in the experiments. In the low-payoff condition the experimental atmosphere might best be described as: "Choose any reasonable point and let's go." Perhaps some concept of decision-making costs together with the equilibrium considerations that receive reasonable support in the high-payoff experiments will explain the differential Series 1 and 2 results.

Series 2. Having found three classes of models which meet with some laboratory success (egoistic game-theoretic, egoistic voting-theoretic and agenda-based voting-theoretic), we elected to continue with the high-payoff experiments and attempt to make some finer distinctions among models 1–8. Moreover, given (1) that the full-communication committees are empirically more common, and (2) that communication did not appear to make much difference with high payoffs, we also elected to continue with only full-communication experiments.

As seen in Figure 2, Series 2 differed from Series 1 in the arrangement of the individual maximums and more importantly, in the *shapes* (elliptical v. circular) of the induced indifference contours. These changes provide the opportunity for a conclusive contest between models 1–4, 6, and 5, 7, all of which made identical predictions in Series 1. In Series 2 however, model 5 (city block) predicts B with coordinates (72, 66). Model 7 predicts points C with coordinates (63, 66) and (72, 61). Models 1–4, and 6 predict A with coordinates (61, 69). Table 2 contains the experimental outcomes for Series 2, while Figure 6 illustrates them. The results are clear. The mean outcome is (60, 72) with a standard deviation comparable to that for Series 1. Thus, the earlier success of models 5 and 7 proves to be illusory. When their predictions coincide with models 1–4, 6, they are upheld. When models 1–4, 6, make a different prediction, models 5 and 7 fail to

Table 2. Experimental Outcomes: Series 2

Outcomes	(70,60) (60,70) (61,69)* (54,72) (61,69)* (60,75) (65,80) (61,69)* (55,75) (55,80)
Mean	(60,72)
Standard Deviation	7.3
*Majority Rule Equilibrium	(61,69)

describe the data. Although subjects sometimes use intellectual short cuts, these apparently do not involve the imposition of simpler preference structures based on the city block metric.

Two other points deserve mention. First, we now see that the Series 1 results cannot be attributed simply to the existence of a transitive, majority-rule-preference relation in that design, i.e., the fact that every sequence of proposal adoptions eventually leads to the core or equilibrium. In Series 2 cycles exist. Thus, not every sequence of proposal adoptions will necessarily lead to the core or equilibrium.

Second, notice the occurrence of non-Pareto optimal outcomes in Series 2 (2 of 10). Such "social wastage" conflicts with coalition theoretic reasoning, and indeed, with all the fundamental ideas of cooperative game theory. In contrast, such pathologies are not uncommon in voting-theoretic models.[26]

Series 3. Those who conduct experiments always have an informational advantage over those who only see the results, in that forces at work in the experiment may not be evident from the outcomes alone. For example, one could explain the Series 1 outcomes by appealing to game-theoretic solution concepts such as the core or Von Neumann-Morgenstern solu-

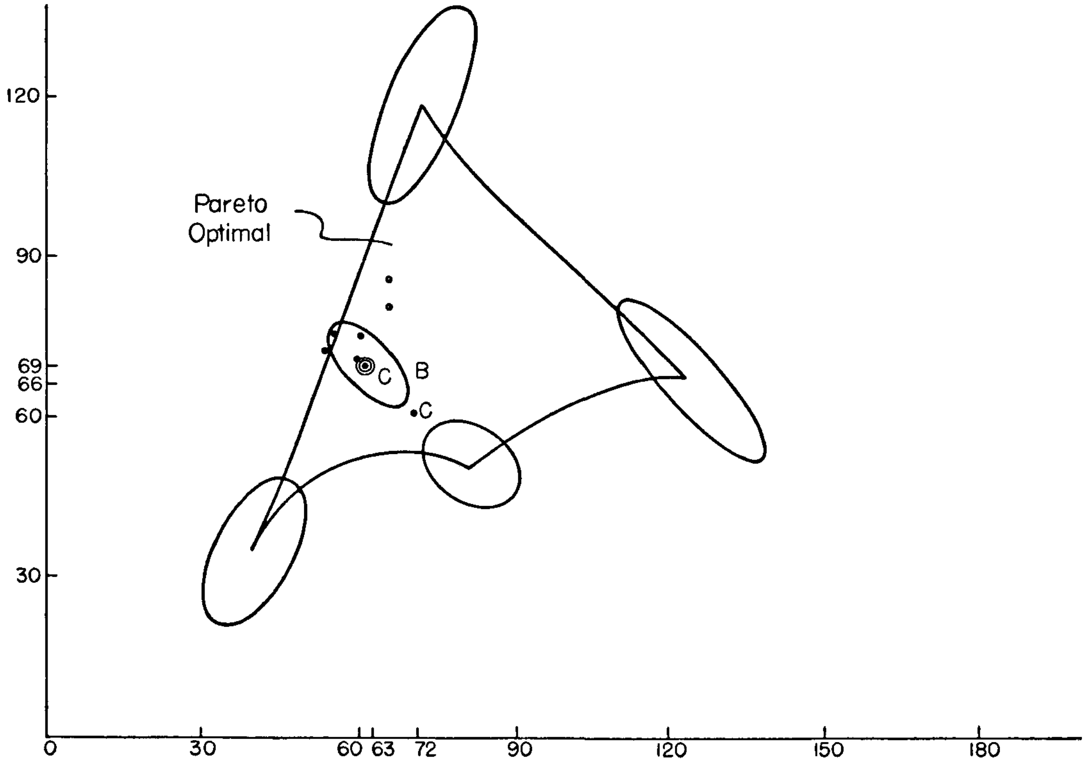

Figure 6. Series 2. Observations

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[26] Peter Fishburn, "Paradoxes of Voting," *American Political Science Review*, 68 (June 1974), 537–46.

tion. As we mentioned, however, the concept of coalition, on which such solution theories depend, appears foreign to our experimental decision makers. Thus, observation of the experiments clearly aids in their explanation. In this section we report on a third series of experiments which were partially motivated by some puzzling phenomena which occurred in Series 1.

Consider Figure 7. This figure shows the "path" taken by a committee in reaching a decision. The committee in Figure 7 made six moves. Note that although the committee ended up near (39,68), *at some point they were exactly on it but left.* According to models 1–4 the impossible happened. Thus, looking only at the experimental outcomes would lead us to a more positive appraisal of these models than would an examination of the experimental dynamics: the phenomenon illustrated in the figures is not uncommon.

How does one explain the kind of process shown in Figure 7? First, note that to move from the point (39,68), at least one person must vote against his or her self-interest. Generally, this is Player 1; the person whose ideal point is at the equilibrium chooses not to enforce it. Why? The experiments tend to proceed roughly as follows. Subjects begin with high hopes—many initially do not even suspect that the experimenters have placed them in conflict. A few proposals and votes quickly destroy the initial optimism. One sees frowns and hears mutterings as expectations are revised downwards. Subjects abandon hopes of getting their maximum and begin to work toward some reasonable outcome.

But what is a reasonable outcome? The alternative space contains 30,000 integer points. Only the five maxima are in any way distinguished, unless symmetry considerations point to outcomes such as (100,75) which, while sometimes mentioned, are quickly dismissed. Among the maxima Player 3 at (39,68) is the only one who is in any sense "centrally located." In fact, his is the only maximum in the interior of the Pareto-optimal surface. Thus, perhaps model 16 is driving the Series 1 experiments. Perhaps (39,68) is a Schelling "obvious point." During the course of debate

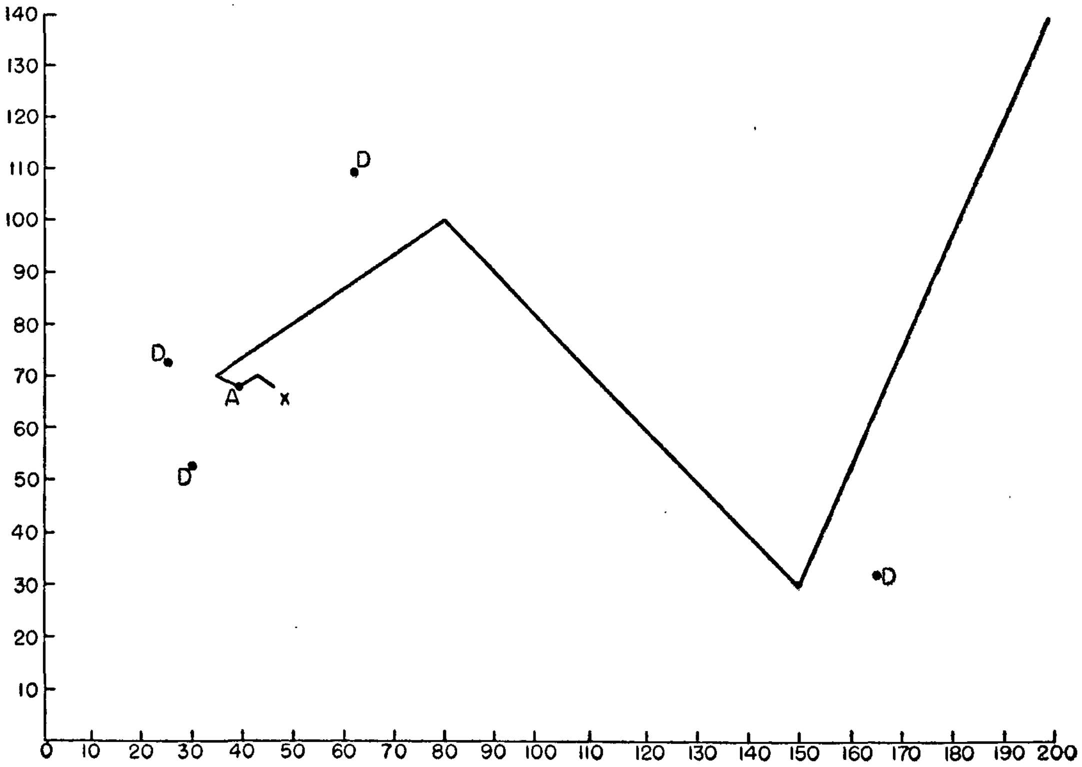

Figure 7. Sample Decision Path: From Series I–High Payoff
Final Outcome: (46,68)

(39,68)—give or take a little to be "reasonable"—might become the "focal point" of committee members' expectations independently of its equilibrium properties. Models 1–4 might *appear* to predict well even though the experimental results depended on quite different consideration.

Series 3 differs from Series 1 only in that Player 1 is moved from (39,68) to (51,59). Although not large, this move clearly destroys the equilibrium/core. If the experimental results remain clustered and simply "follow" Player 1 to (51,59), one would have grounds for rejecting the equilibrium model in favor of some coordinated expectations model. Table 3 contains the outcomes of the 15 experiments in Series 3, while Figure 8 illustrates them graphically.

Table 3. Experimental Outcomes: Series 3

Outcomes	(55,70)
	(50,60)
	(32,68)
	(50,60)
	(37,54)
	(46,62)
	(45,50)
	(40,73)
	(45,68)
	(48,66)
	(45,59)
	(47,59)
	(30,70)
	(51,65)
	(55,60)
Mean	(45,62)
Standard Deviation	10.3

Evidently, the results of Series 3 are ambiguous. To some extent the outcomes "track" Player 1—the mean shifts outward to (45,62). But Player 1's position (51,59) is certainly not the mean of the experimental outcomes as in Series 1, nor is it ever an experimental outcome. A comparison of the outcome variance yields a similarly ambiguous decision. On the one hand the standard deviation is twice as large as in the high-payoff, Series 1 experiments. But on the other hand, the outcomes continue to cluster in a particular region, rather than fall at widely dispersed position in the space.

In a recent paper McKelvey proves a rather striking result.[27] If committee members evalu-

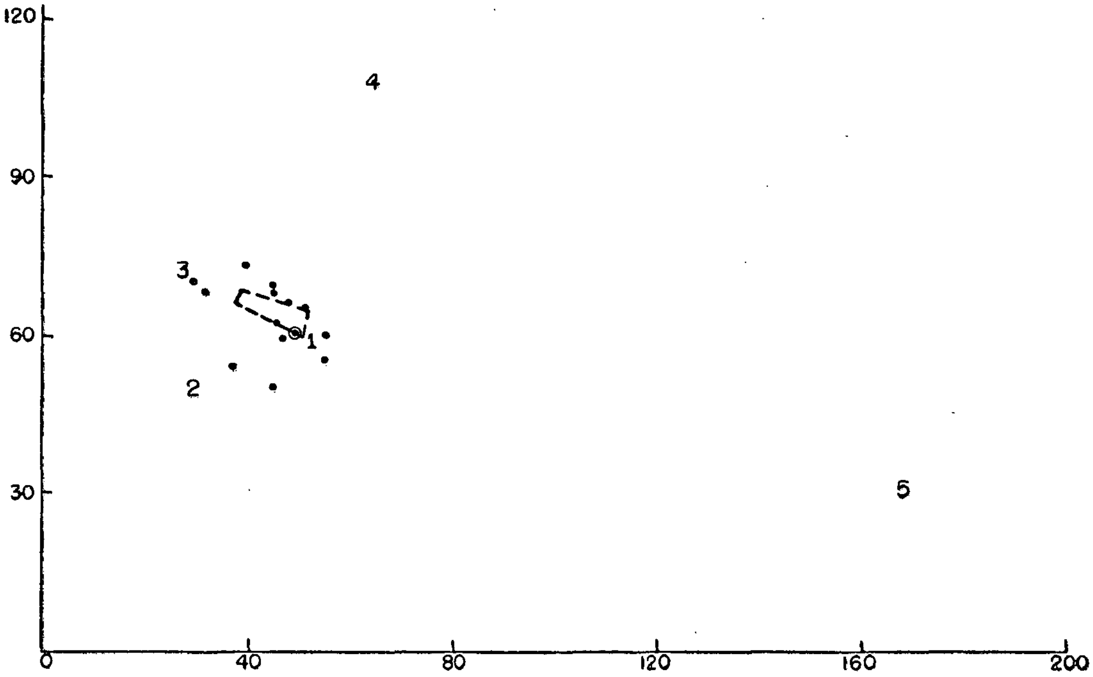

Figure 8. Series 3. Observations

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[27] McKelvey, "Intransitivities in Spatial Voting Games."

ate policies in terms of utility functions based on the Euclidean metric, then in the absence of an equilibrium, the space of potential outcomes coincides with *the entire policy space.* From any point in the space one can construct a sequence of alternative policies which under sincere voting lead to *any* arbitrarily selected point. McKelvey's result induces an interesting either-or hypothesis: if equilibrium exists, then equilibrium occurs; if not, then chaos, with the latter much more likely than the former. McKelvey conjures up the spectre purportedly exorcised by Tullock.[28]

Evidently, however, this pessimistic natural hypothesis induced by McKelvey's theorem is not operating in our experiments. In Series 1 the existence of an equilibrium is associated with a tightly clustered set of outcomes centered on the equilibrium. But in Series 3 the *nonexistence* of an equilibrium is *not* associated with experimental chaos. We did not notice any behavioral differences in the conduct of the two series; in particular, subjects in Series 3 appeared to have no greater difficulty in reaching a decision than did those in Series 1. Perhaps some general theory exists which could explain both Series 2 and 3. If some as yet undeveloped theory is driving the Series 3 experiments, it had better specialize to the equilibrium/core when the latter exists. The min-max set (model 4) is one such theory, but it really does not do very well: of 15 Series 3 outcomes, only 3 are contained in it.[29]

In sum, Series 3 both settles and raises questions. On the one hand the pattern of results leads us to believe that model 16 is not driving the Series 1 results, that the existence of an equilibrium in Series 1 makes a real difference. On the other hand, the pattern of experimental findings does not explode, a fact which makes us wonder whether some unidentified theory is waiting to be discovered and used.

6. Conclusions

What have we learned from the research just described? What we *expected* to learn changed during the course of the project. We began with the presumption that none of the models found in the literature could survive a careful experimental evaluation, that our experimental committees would produce outcomes "all over the map," so to speak. In all candor, we suspected that formal models of committee processes had little to recommend them other than logical rigor.

This negative presumption went by the board rather quickly—after a few pilot experiments, in fact. Some of the models actually appeared to work. Thus, our task changed from the easy one of generating negative results to a more difficult and painstaking one of determining why some models work, and when. This necessitated careful specification of the available models and careful design of experiments capable of differentiating among them. This conclusion summarizes the results of our first efforts, our thoughts about the significance of our results for existing theories, and the relevance of our results for applied research.

Implications for Basic Committee Research. Consider the following question: Is there a model which will predict the decision of a group under the following conditions?

a. There is no uncertainty about the consequences of any decision;

b. There is no indifference or lack of personal interest on the part of committee members;

c. The committee uses majority rule but little or no additional formal procedure such as an agenda; and

d. There are no caucuses or extra-committee meetings among members of the committee.

The answer: If the configuration of preferences is such that an equilibrium exists and if the preferences are strongly held, then, within narrow limits the committee's choice will be the equilibrium. If the committee members have low incentives, then several models do equally badly.

This answer implies several additional conclusions and raises some interesting additional questions:

(1) Numerous widely known models which have natural interpretations in this setting do not work. Applied scientists who wish to study situations falling within our specifications should not look to these models for help. Table 4 provides a list of the models discussed in section 4 arranged against the three experimental series. A ✓ indicates a "best" model and

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[28] Gordon Tullock, "The General Irrelevance of the General Impossibility Theorem," *Quarterly Journal of Economics*, 81 (May 1967), 256–70.

[29] There are various game-theoretic solution concepts (N-M Solution, Bargaining Set, Competitive Solution) not as yet computed which also remain live candidates.

Table 4. Comparative Performance of Models

	High Payoff								Low Payoff			
	Series 1 (Figure 1)								Series 1 (Figure 1)			
	Full Communication		No Communication		Series 2 (Figure 3)		Series 3 (Figure 5)		Full Communication		No Communication	
	Prediction	Result	Prediction	Result	Prediction	Result	Prediction	Result	Prediction	Result	Prediction	Result
Egoistic												
Game-Theoretic												
1. Core	A	✓	A	✓	A	✓	NP	–	A	?	A	?
2. V.M. solution	A	✓	A	✓	A	✓	PU	?	A	?	A	?
Voting-Theoretic												
3. Equilibrium	A	✓	A	✓	A	✓	NP	–	A	?	A	?
4. Min-max set	A	✓	A	✓	A	✓	A	✓	A	?	A	?
5. C.B. dec. rule and equilibrium	A	✓	A	✓	B	X	PR	PR	A	?	A	?
Agenda-Based Voting-Theoretic												
6. Obvious agenda	A	✓	A	✓	A	✓	PU	?	A	?	A	?
7. Median along coordinates	A	✓	A	✓	C	X	PR	PR	C	?	C	?
8. Binary amendment	A	✓	A	✓	PU	?	everything	X	A	?	A	?
9. Agenda manipulation	D	X	D	X	PR	PR	PR	PR	D	X	D	✓
Coalition-Theoretic												
10. Minimum winning	E	X	E	X	PR	PR	PR	PR	E	X	E	X
11. Resource coalition	Ê	X	Ê	X	PR	PR	PR	PR	Ê	X	Ê	X
12. Max valued min win	F	X	F	X	PR	PR	PR	PR	F	X	F	X
Non-Egoistic												
13. Max group return	G	X	G	X	PR	PR	PR	PR	G	?	G	X
14. Fair point	H	X	H	X	PR	PR	PR	PR	H	?	H	X
15. Dominant personality	D	X	D	X	PR	PR	PR	PR	D	X	D	✓
16. Obvious point	A	✓	A	✓	A	✓	B	X	A	?	A	?

X = model rejected at this point
✓ = model remains in acceptable set of models
PR = previously rejected
PU = prediction uncertain
NP = no prediction
? = ?

an X indicates a point in which a model was eliminated from further consideration.

(2) In the high-incentive case ($1 to $3 per unit) coalition models, most of the agenda models and all of the non-egoistic models do a very poor job relative to equilibrium models.

(3) In the low-incentive case (1c to 5c per unit), if full communication is allowed, all models do poorly. Non-egoistic models, especially those based on "fairness," receive their only hint of support under these conditions. If we were forced to generalize, we would say that when people are personally indifferent or near indifferent, they try to be fair, thereby allowing the decision to be pulled further from them than the equilibrium outcome.

(4) In the low-incentive case, if no communication is allowed, all models do poorly. If we were forced to generalize we would say that the most clever agenda manipulator is the best of a poor lot. The incentives are low. People want to hurry up, vote, and go. The person who interjects a favorite motion at the proper time just might get it accepted.

(5) The fact that different models apply according to the magnitude of the incentives implies an important methodological conclusion. A large sample size will not substitute for large payoffs in differentiating between models designed for understanding high-incentive groups.[30] Models intended for application in high-incentive contexts should not be rejected on the basis of data from low-payoff experiments.

(6) When an equilibrium does not exist in a high-incentive circumstance, the process does not "explode." The min-max set does best among the models considered, but only because of very weak competitors. Nevertheless, the resulting pattern of decisions seems "well contained" and suggests that a further generalization of the equilibrium notion may exist.

(7) Several ideas in the game theory literature provide models which specialize to the equilibrium when it exists and also make predictions when it does not. Application of these models requires precise operationalization of the coalition concept—a surprisingly difficult task, in our view. As mentioned, we saw little of what the literature would have us look for in identifying a coalition. Moreover, we observed equilibrium behavior even in the absence of communication (Series 1). Were there "phantom" coalitions at work in the latter experiments? Consider also the occurrence of non-Pareto-optimal outcomes in Series 2. These are inconsistent with all theories of coalition behavior.

(8) Which of the surviving models is "really" the right one? We don't know yet. The data are consistent with four general modes of thought, that producing the core, the solution, the equilibrium and the obvious agenda. Which, if any, ultimately prove accurate remains to be determined.

(9) What will happen as conditions (a) through (d) are relaxed or varied? As uncertainty about the outcome is introduced and the group takes on a problem-solving as well as conflict-resolving aspect, members may organize themselves for purpose of information processing in a manner which induces coalition-type behavior. Such an environment, in which trust and belief loom large, may also revive the personality or other non-egoistic models. If the committee organizational environment is conducive to private meetings among subsets of members, the coalition models may receive a breath of life. But in any case we emphasize that all of these questions are resolvable within the laboratory setting.

Experiments and Applied Committee Research. A more difficult question now arises. We have indicated what we appear to have learned from the standpoint of basic science. But have we learned anything that makes a contribution to applied science? Specifically, do our experimental findings have implications for behavior in the larger political world? A significant proportion of our colleagues would undoubtedly answer in the negative. What arguments or beliefs underlie their skepticism? Having thought the matter over at some length, it seems to us that the most commonly heard arguments fall into one of three classes.[31]

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[30] cf. Anatol Rapoport and Albert Chammah, *Prisoner's Dilemma* (Ann Arbor: University of Michigan Press, 1965); Peter Halfpenny and Michael Taylor, "An Experimental Study of Individual and Collective Decision Making."

[31] Actually, each of the three arguments discussed in this section is a member of a more general class which holds that the laboratory environment is *artificial* (the word is usually pronounced loudly and repeatedly). The specifics of the general criticism often are not spelled out—we have tried to do so in this section—but at base the criticism appears to stem from acceptance of a gestalt psychology view. That is, the subtleties of the empirical context are too many and/or too complicated to be enumerated. Moreover, these variables may interact in such a way that they

The first argument simply asserts that "real" politicians are different from student subjects. Moreover, the argument runs, such differences are fundamental and do not merely reflect the more complex contexts in which professionals operate. This argument suggests that were we to conduct our experiments using state legislators as subjects, the experimental results would differ systematically and significantly from those previously obtained. In principle one could check the validity of the criticism by replicating the experiments using professionals as subjects. We have not done so, and therefore have no real defense against this first argument. We did use a relatively heterogeneous subject group and we encourage others to explore the bounds of the results across other populations.

The other two arguments emphasize the fact that naturally occurring political committees do not exist in splendid isolation. Instead, they are frequently embedded in some larger ongoing institutional context. This embedding raises the following potentially critical question: if the committee decision is regarded by the members as only one stage in a sequence of games, might behavior in the committee reflect strategic considerations from the larger game? If so, a model which explains the behavior in the larger game might produce implications for the committee stage which differ substantially from those implied by models successful in explaining the processes of isolated committees.

The preceding argument raises an obviously important consideration: behavior in interdependent, sequential decisions might reflect laws and principles different from those underlying behavior in isolated decisions. On balance, we think this second argument makes a convincing case for *not* extrapolating our results too quickly to the larger political world. We make the following observation, however. One can gradually (and *carefully*) complicate a research design to make it more analogous to real-world political processes. By doing so, we can determine whether the complications destroy the applicability of models which work in simpler contexts. If they do, we stand a good chance of both identifying the precise complications which do the damage, and modifying the models to incorporate such complications. Thus, we do not take the second argument as a criticism of what we have done or of experimental methodology in general. Instead, it suggests that more experimental work needs to be done. We agree.

A final, third class of arguments also emphasizes the complexity and interdependence of naturally occurring political processes. Even if a model applies in principle to a naturally occurring situation, there may be no practical way to apply it. In laboratory studies we have easy access to the relevant parameters. How, in the "real world" where there are old friendships and enmities, established working relationships, debts, etc., does one identify the alternative space and the preferences over that space? For example, through exhaustive interviews and some suitable scaling technique we might construct a policy space in which a congressional committee, school board, etc., is presumably operating. Perhaps we could even approximate preferences over this space. Even so, the argument runs, isn't the policy space only a tiny subspace of the larger alternative space which includes all the aforementioned complicating factors? Clearly this is an empirical question. Answers to such questions can only come from the experience (yet to come) of trying to apply the models within the limits of prevailing measurement technology. The limits of the practical are constantly changing, presumably in a positive direction. To reject an applicable model because of the current practical difficulties of using it seems short-sighted, if not foolish.

The preceding arguments should make us cautious about assuming correspondences between laboratory and naturally occurring political behavior. But neither separately nor in combination do these arguments negate the existence of those correspondences. The important thing is to resist the temptation to leap prematurely from the laboratory to the field. That transition should come through slow, well-chosen steps based on a carefully designed program which integrates theory, laboratory and field research.

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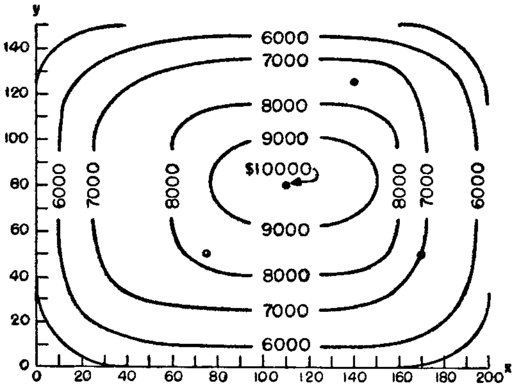
are effective only when moved together—precisely the kind of variation not allowed in the controlled laboratory environment. This view must be taken seriously, and ultimately there is only one convincing rejoinder, the same that an experimentalist in the physical sciences must give to a critic who claims that the laws which operate in test tubes or other laboratory environments differ from those which operate in the "real world." One can only point to laboratory-generated information which has been helpful in field or engineering endeavors. Granted, experimental social science has not yet progressed to such a level, but we are optimistic that it eventually will.

Appendix A.
Instructions for Full Communication Experiments

General. You are about to participate in a committee process experiment in which one of numerous competing alternatives will be chosen by majority rule. The purpose of the experiment is to gain insight into certain features of complex political processes. The instructions are simple. If you follow them carefully and make good decisions, you might earn a considerable amount of money. You will be paid in cash.

Instructions to Committee Members. The alternatives are represented by points on the blackboard. The committee will adopt as the committee decision one and only one point. *Your* compensation depends on the particular point chosen by the committee (see attached payoff chart). For example, suppose your payoff chart is that given in Figure 1 and that the committee's final choice of alternative is the point (x,y) = (170,50). Your compensation in this event would be $7,000. If the policy of the committee is (140,125) your compensation would be computed as follows:



The point (140,125) is halfway between the curve marked $7,000 and the curve marked $8,000. So, your compensation is halfway between $7,000 and $8,000, i.e., $7,500. If the policy is one-quarter of the distance between two curves, then your payoff is determined by the same proportion (i.e., at (75,50) which is one-quarter of the way between $8,000 and $9,000, you get $8,250).

The compensation charts may differ among individuals. *This means that the patterns of preferences differ and the monetary amounts may not be comparable. The point which would result in the highest payoff to you may not result in the highest payoff to someone else.* You should decide what decision you want the committee to make and do whatever you wish within the confines of the rules to get things to go your way. *The experimenters, however, are not primarily concerned with whether or how you participate so long as you stay within the confines of the rules.* [Under no circumstances may you mention anything *quantitative* about your compensation. You are free, if you wish, to indicate which ones you like best, etc., but you cannot mention anything about the actual monetary amounts. Under no circumstances may you mention anything about activities which might involve you and other committee members after the experiment, i.e., no deals to split up afterward or no physical threats.]*

Parliamentary Rules. The process begins with an existing motion (200,150) on the floor. You are free to propose *amendments* to this motion. Suppose, for example, (170,50) is the motion on the floor and you want the group to consider the point (140,125). Simply raise your hand and when you are recognized by the chair, say "I move to amend the motion to (140,125)." The group will then proceed to vote on the amendment. If the amendment passes by a majority vote, the point (140,125) is the new motion on the floor and is subject, itself, to amendments. If the amendment fails the motion (170,50) remains on the floor and is subject to further amendment. Thus, amendments simply change the motion on the floor. You may pass as many amendments as you wish.

At any time during the consideration of an amendment or the motion on the floor a *motion to end debate* is in order. If there are no objections, an immediate vote will take place. If there are objections, the motion to end debate will itself be put to a majority vote. If the motion to end debate fails, the amendment process continues. If it passes, a vote on the amendment or motion will take place.

To sum up, the existing motion on the floor is (200,150). You are free to amend this motion as you wish. The meeting will not end until a majority consents to end debate and accept some motion. Your compensation will be determined by the motion on the floor finally adopted by the majority.

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*This section omitted in no-communication condition.

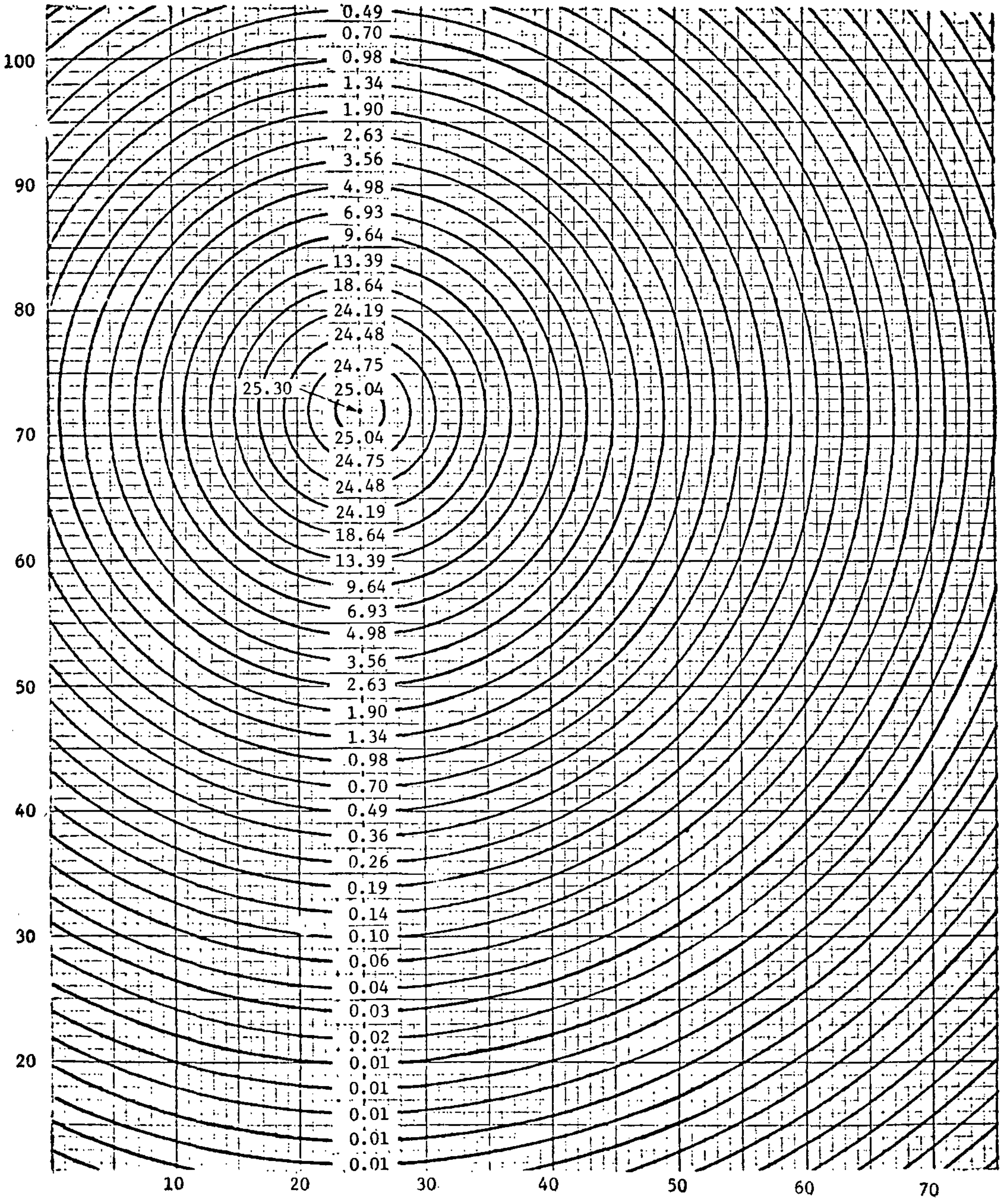
Are there any questions?

We would like you to answer the questions on the attached page. These should help you understand the instructions.

Test

1. At ______ I would make the most possible money. The amount I would receive is ______.
2. At ______ I would make the least possible money. The amount I would receive is ______.
3. Suppose (200,150) is the motion on the floor and an amendment to move to point (199,149) passes (fails), then the new motion on the floor is ____,____ (____,____)?
4. Suppose an amendment to move to (100,100) passes and no further amendments pass. If the motion on the floor is then adopted by a majority, my compensation is ______.

Appendix B. Sample Indifference Map–Player 3, Series 1, High Payoff



Appendix C: Functional Forms[1] for Subject Incentives; In Dollars, All Series

Subject Number	Series 1: High Payoff	Series 1: Low Payoff	Series 2[3]	Series 3
1	$9.45e^{-.153[(x-39)^2+(y-68)^2]^{1/2}}$	$6.0 - .01[(x-39)^2+(y-68)^2]^{1/2}$	$(x^i,y^i) = (61,69)$ a = 3.3345, b = 4.6689, c = 3.3345 A B 12 .65 for $0 \leqslant r \leqslant 10$ 5.5 .20 for $10 \leqslant r \leqslant 20$ 3.50 .15 for $20 \leqslant r \leqslant 30$ 2.00 .10 for $30 \leqslant r \leqslant 40$ 1.00 .05 for $40 \leqslant r \leqslant 60$ 0 0 for $60 \leqslant r$	$9.45e^{-.153[x-51)^2+(y-59)^2]}$
2	$27.75 - .102[(x-30)^2+(y-52)^2]^{1/2}$ for (x,y): $0 \leqslant [(x-30)^2+(y-52)^2] \leqslant 12$ $172.5e^{-.69[(x-30)^2+(y-52)^2]^{1/2}}$ for (x,y): $12 \leqslant [(x-30)^2+(y-52)^2]^{1/2}$	$7.0 - .02[(x-30)^2+(y-52)^2]^{1/2}$	$(x^i,y^i) = (81,50)$ a = 1.1306, b = 0.5602, c = 1.6007 A B 26 .60 for $0 \leqslant r \leqslant 20$ 2 .10 for $20 \leqslant r \leqslant 60$ 0 0 for $60 \leqslant r$	Same as Series 1: High Payoff
3	$25.30 - .141[(x-25)^2+(y-72)^2]^{1/2}$ for (x,y): $0 \leqslant [(x-25)^2+(y-72)^2]^{1/2} \leqslant 8$ $97.02e^{-.165[(x-25)^2+(y-72)^2]^{1/2}}$ for[2] (x,y): $10 \leqslant [(x-25)^2+(y-72)^2]^{1/2}$	$7.0 - .02[(x-25)^2+(y-72)^2]^{1/2}$	$(x^i,y^i) = (39,34)$ a = 3.5126, b = −2.5605, c = 1.6523 A[4] B 26 .40 for $0 \leqslant r \leqslant 30$ 14 .60 for $30 \leqslant r \leqslant 50$ 2 .10 for $50 \leqslant r \leqslant 70$ 0 0 for $70 \leqslant r$	Same as Series 1: High Payoff

Appendix C (continued)

Subject Number	Series 1: High Payoff	Series 1: Low Payoff	Series 2[3]			Series 3
4	$38.25 - .1125[(x-62)^2 + (y-109)^2]^{1/2}$		$(x^i, y^i) = (72,118)$			
	for (x,y): $0 \leqslant [(x-62)^2 + (y-109)^2]^{1/2} \leqslant 38$	$9.0 - .051[(x-62)^2 + (y-109)^2]^{1/2}$	a = 7.5769, b = –5.3145, c = 2.0736			Same as Series 1: High Payoff
	$11931.75e^{-.165[(x-62)^2 + (y-109)^2]^{1/2}}$		A	B		
	for (x,y): $38 \leqslant [(x-62)^2 + (y-109)^2]^{1/2}$		25	.20	for $0 \leqslant r \leqslant 40$	
			17	.60	for $40 \leqslant r \leqslant 60$	
			5	.25	for $60 \leqslant r \leqslant 80$	
			0	0	for $80 \leqslant r$	
5	$26.50 - .05[(x-165)^2 + (y-32)^2]^{1/2}$		$(x^i, y^i) = (124,66)$			
	for (x,y): $0 \leqslant [(x-165)^2 + (y-32)^2]^{1/2} \leqslant 122$	$11.0 - .034[(x-165)^2 + (y-32)^2]^{1/2}$	a = 7.8964, b = 13.7929, c = 7.8964			Same as Series 1: High Payoff
	$(2.34)\,10^5 e^{-.145[(x-165)^2 + (y-32)^2]^{1/2}}$		A	B		
	for[2] (x,y): $124 \leqslant [(x-165)^2 + (y-32)^2]^{1/2}$		36	.15	for $0 \leqslant r \leqslant 140$	
			15	.20	for $140 \leqslant r \leqslant 160$	
			11	.25	for $160 \leqslant r \leqslant 180$	
			6	.20	for $180 \leqslant r \leqslant 200$	
			2	.10	for $200 \leqslant r \leqslant 220$	
			0	0	for $220 \leqslant r$	

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[1] Incentives differed from these in two ways. (i) Payoff charts consisted of the level surfaces of these functions at two unit (Euclidean distance) intervals. Subjects were told to use linear interpolations for points between level surfaces.

[2] (ii) Linear interpolations were used to define incentives for points that are not otherwise defined.

[3] Functional form for series 2 is

$$\text{dollar payoff} = A - Br$$

$$r = [a(x-x^i)^2 + b(x-x^i)(y-y^i) + c(y-y^i)^2]^{1/2}$$

[4] These parameters for subject three are correct for all points except the maximum (39,34) which, due to a calculation error was \$27 on the payoff chart rather than \$26.

The Competitive Solution for *N*-Person Games Without Transferable Utility, With an Application to Committee Games*

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This essay defines and experimentally tests a new solution concept for n-person cooperative games—the Competitive Solution. The need for a new solution concept derives from the fact that cooperative game theory focuses for the most part on the special case of games with transferable utility, even though, as we argue here, this assumption excludes the possibility of modelling most interesting political coalition processes. For the more general case, though, standard solution concepts are inadequate either because they are undefined or they fail to exist, and even if they do exist, they focus on predicting payoffs rather than the coalitions that are likely to form.

The Competitive Solution seeks to avoid these problems, but it is not unrelated to existent theory in that we can establish some relationships (see Theorems 1 and 2) between its payoff predictions and those of the core, the V-solution and the bargaining set. Additionally, owing to its definition and motivation, nontrivial coalition predictions are made in conjunction with its payoff predictions.

The Competitive Solution's definition is entirely general, but a special class of games—majority rule spatial games—are used for illustrations and the experimental test reported here consists of eight plays of a 5-person spatial game that does not possess a main-simple V-solution or a bargaining set. Overall, the data conform closely to the Competitive Solution's predictions.

Much of *n*-person cooperative game theory concerns the special case of games with transferable utility—games in which utility "acts like money" and can be transferred among players.[1] The transferable utility assumption, however, seriously limits the relevance of the corresponding theory, so that, as we argue later, theoretical consequences such as Riker's (1962) size principle cannot be usefully applied to the empirical analysis of, e.g., legislative coalition formation. (For an equivalent argument see De Swann, 1974, but note that the current debate over the size principle's theoretical validity assumes transferable utility. C.f., Butterworth, 1971; Shepsle, 1974; McKelvey and Smith, 1975; and Hardin, 1976.) But if we adopt a more general and appropriate model of coalition processes that does not assume transferable utility, we find that game theory is itself deficient as an analytical tool. In the transferable utility case, numerous solution concepts such as the *V*-set, the kernel, the nucleolis, and various bargaining sets exist to treat games with empty cores—games without undominated imputations. For games that do not assume transferable utility, on the other hand, these concepts are either undefined or, like the core, they are typically empty. Hence, for most politically important games, game theory often provides no predictions about outcomes.

Our objective is to define and to illustrate the application of a new solution concept—the *Competitive Solution*—and to report a modest experimental test of its empirical validity. This concept's definition is entirely general and thus treats games with and without transferable utility. We justify the necessity for such a concept and illustrate its ap-

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* The authors would like to thank the National Science Foundation for its support of this research. We also thank James D. Laing for his many helpful suggestions in preparing this paper and in the conduct of our experimental research, and Phillip Straffin for his perceptive comments.

[1] In decreasing order of generality, there are three classes of games: (1) without sidepayments and without transferable utility; (2) with sidepayments but without transferable utility; and (3) with sidepayments and with transferable utility. While Aumann proves some results about (2), this case is best treated by the notation and concepts developed for (1). Hence, throughout we talk simply of the transferable—nontransferable dichotomy (c.f., Aumann, 1967).

plication, though, by examining several examples of logrolling and spatial committee games—with particular emphasis on spatial games. In a logrolling game, the players (the members of a legislative committee) must decide by majority rule which of several bills to pass and which to fail. An analysis of the cooperative version of these games, then, reveals the consequences of vote-trading. In spatial games, the alternatives correspond to an m-dimensional subset of Euclidean space and the committee's task is to choose some point in this subset as the social choice. These games, therefore, are often used to model party parliamentary coalition processes.

We emphasize that this essay is concerned principally with developing a new solution concept and is only secondarily concerned with the analysis of specific committee processes. With logrolling and spatial games, however, we can illustrate not only the necessity for abandoning the transferable utility assumption, but also the inadequacies of existent theory. *First*, this theory focuses on predicting the payoffs to players and is almost wholly silent on the question of what coalitions form. In a logrolling game, then, the specific vote trades that occur to yield a particular payoff outcome are not identified. This leads some scholars such as Michael Taylor (1972) to conclude that formal solution theory is irrelevant to the study of legislative and parliamentary coalition formation processes. *Second*, we can readily formulate spatial games for which the usual solution concepts of the Core, the V-set, and the several bargaining sets render no prediction (i.e., are empty). *Third*, concepts such as the V-set are principally mathematical abstractions without behavioral rationale. Hence, it is difficult to assess their applicability when particular rules constrain bargaining or negotiation procedures in committees.

Admittedly, there are several unresolved questions about the solution proposed here. The important theorems concerning existence and uniqueness await proof or counterexamples. Also, as the current debate over the theoretical validity of the size principle illustrates, this principle as well as the several ad hoc hypotheses designed to model coalition processes (e.g., only connected or compact coalitions form), should be linked to the Competitive Solution. The particular examples considered in this essay suggest that these hypotheses are consistent with the Competitive Solution; but, again, the relevant theorems await proof. Nevertheless, the Competitive Solution seems the most promising candidate for usefully applying game theory. First, it predicts coalitions as well as payoffs and links coalitions to specific payoffs. Second, it solves those spatial games that serve as counterexamples to the existence of V-sets, etc. Moreover, the Competitive Solution necessarily exists and is equivalent to the Core or a main-simple V-solution if either of these exist (see Theorems 1 and 2). Hence, it is a natural extension of these classical solution concepts. Third, like bargaining set theory, it is motivated principally by a specific conceptualization of the bargaining process.

Section 1 briefly reviews the differences between games with and without transferable utility and illustrates the necessity for a nontransferable utility formulation of committee processes. This section also develops the spatial conceptualization of a simple majority game and illustrates the shortcomings of standard solution theory. Section 2 defines the Competitive Solution for cooperative games in general, and, via two theorems, reports on this solution's relationship to the core, to the main-simple V-set and to bargaining set theory. Section 3 applies this concept to some simple spatial games, while Section 4 describes the results of eight 5-person experiments that test the solution's empirical validity.

Transferable Utility, Committee Games, and Solution Theory

We begin with some preliminary notation:

$N = \{1, 2, \cdots, n\}$: the set of *players*,
$C \subseteq N$: a *coalition*,
$|C|$: *the number of players in coalition C*,
$u = (u_1, \cdots, u_n) \in R^n$, an ordered utility payoff vector, where u_i denotes the utility of player i.
$H \subseteq R^n$: the set of possible *payoff vectors or outcomes*.

The theory of n-person games is formulated now in terms of a *characteristic function*, $v(C)$, which specifies the payoffs that the members of C can jointly secure for themselves, regardless of the actions of other players. The most familiar version of the characteristic function equates $v(C)$ with some real number, r_C, with the implication that r_C is a quantity of utility that the members of C can secure and divide among themselves. This is referred to as the transferable utility case. More generally, though, we can abandon the transferable utility assumption by letting $v(C)$ be a set of payoff vectors, i.e., $v(C) \subseteq R^n$.[2]

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[2] The conditions imposed on $v(C)$ vary in the literature. But, the three most common are: (i) for all $C \subseteq N$, $v(C)$ is closed and convex; (ii) for any $C \subseteq N$, if $u_i \leq w_i$ for all $i \in C$ and if $w \in v(C)$, then $u \in v(C)$; (iii) for any $C_1, C_2 \subseteq N$ with $C_1 \cap C_2 = \Phi$, $v(C_1) \cap v(C_2) \subseteq v(C_1 \cup C_2)$. These conditions are imposed typically to establish particular results or to render analysis tractable. We state them here, then, only as a matter of convention. One assump-

To see why this reformulation of $v(C)$ is essential, consider *simple games*, which are important because they model committee processes that entail formal voting procedures. A game is *simple* if it is "superadditive" and if a coalition can guarantee its members any payoff vector in H (plus all lesser payoffs), or nothing, in that it cannot guarantee its members more than they can obtain individually. Thus, for all coalitions C, either $H \subseteq v(C)$, in which case C is *winning*, or $v(C) \cap H^{int} = \Phi$ in which case C is *losing*. A simple game is superadditive if any superset of a winning coalition is also winning. Further, a simple game is *strong* (decisive) if for every coalition $C \subseteq N$, exactly one of C, $N-C$ is winning (i.e., there are no blocking coalitions), and it is *symmetric* if $v(C)$ depends only on the size of C (e.g., all players possess equal weight in the voting). We let W denote the set of winning coalitions, and then define the set of *minimal winning* coalitions,

$$W^* = \{C \in W \mid C - \{i\} \notin W \quad \text{for all } i \in C\}.$$

If we intend, now, to model a committee that uses majority rule to choose one outcome from the set A, and if the number of players, $n = |N|$, is odd so that there are no blocking coalitions, the set of winning coalitions is

$$W = \left\{C \subseteq N \middle| |C| > \frac{n}{2}\right\}.$$

Letting each player's utility be a function over A, we can associate the utility vector $u(\theta) = (u_1(\theta), u_2(\theta), \cdots, u_n(\theta))$ with each θ in A, and express the set of all possible payoff vectors as (ignoring lotteries) $H = \{u \mid u = u(\theta),\ \theta \in A\}$. Since winning coalitions secure any alternative, if C is winning,

$$v(C) = \{u \mid \text{for some } \theta \in A,\ u_i \leq u_i(\theta) \text{ for all } i \in C \supseteq H\}$$

while the value of a losing coalition is simply the intersection of the values of its members. (Thus, for each $i \in N$,

$$v(\{i\}) = \{u \mid u_i \leq \min_{w \in H} w_i\}$$

and

$$v(C) = \bigcap_{i \in C} v(\{i\})$$

if C is losing.)

To illustrate, suppose a 3-member legislature is confronted with three bills and that it must decide by majority vote which bills to pass and which to fail. Hence, $A = \{\theta = (\theta_1, \theta_2, \theta_3) \mid \theta_i = 0 \text{ or } 1 \text{ for } i = 1, 2, 3\}$, so that, excluding lotteries, the committee must choose one of $2^3 = 8$ possible outcomes, including the passage of no bills or the passage of all bills. Table 1 portrays some possible utility payoffs for each legislator over the eight outcomes.

Since player *1* is not a majority, he cannot be certain that his least preferred outcome—bills *2* and *3* passing—does not prevail. Hence, $v(1) = \{u \mid u_1 \leq -2\}$. Similarly, for legislator *2*, $v(2) = \{u \mid u_2 \leq -4\}$. Together, however, players *1* and *2* can secure any outcome, and Figure 1a graphs the joint payoffs that these two players can attain as a winning coalition. Assuming that players *1* and *2* also consider all possible lotteries, the shaded region in Figure 1a describes $v(1, 2)$.

Figure 1a, then, illustrates a characteristic function defined simply as some collection of utility vectors. For the more familiar transferable utility case, on the other hand, $v(C)$ is summarized by a single number thus:

$$v(C) = \left\{u \middle| \sum_{i \in C} u_i \leq r_C\right\},$$

which is to say that C can secure any distribution of utility among its members provided that the sum over its members (given suitable normalizations) does not exceed r_C. (In the traditional development of games with side payments and transferable utility, $v(C)$ is treated as a real valued function rather than a set valued function; and in this case

Table 1. 3-Person, 3-Bill Logrolling Example

	Bills Passed by the Committee							
Legislator	**None**	**1**	**2**	**3**	**1 and 2**	**1 and 3**	**2 and 3**	**All**
1	0	5	−1	−1	4	4	−2	3
2	0	−2	5	−2	3	−4	3	1
3	0	−3	−3	6	−6	3	3	0

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tion, however, warrants discussion. In particular, by letting $v(C) \subseteq R^n$, such that no constraints are placed on u_j, $j \notin C$, $v(C)$ is a "cylinder." Some treatments assume that $v(C) \subseteq R^{|C|}$. We emphasize that adopting one assumption over the other is made simply on the basis of notational convenience. Hence, while we let $v(C) \subseteq R^n$, when graphing $v(C)$ in Figures 1a and 1b, we graph only the $|C|$-projection.

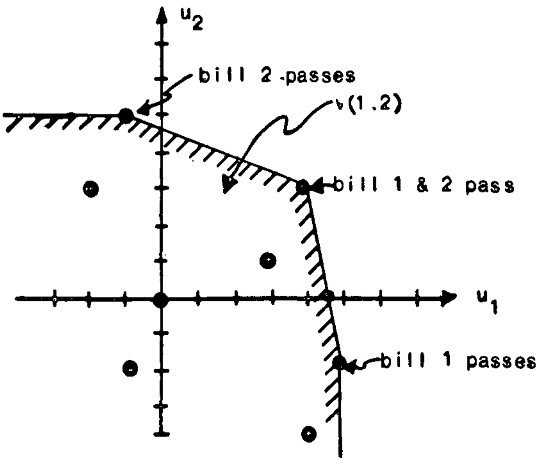

Figure 1a

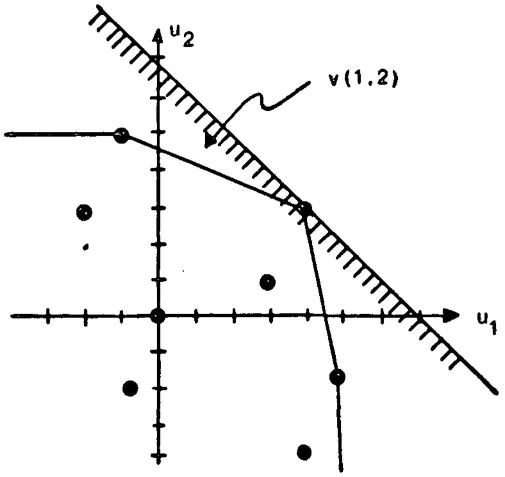

Figure 1b

we have $v(C)=r_C$.) In the example, however, no single number adequately summarizes $v(1, 2)$. To suppose that such a number exists necessitates assuming, for instance, that the payoffs in Table 1 are dollars and that each legislator's utility is linear in money, so that the members of a coalition could, after agreeing to a particular outcome, *transfer utility* by transferring money from one player to another.[3] For example, legislators *1* and *2* could pass bills *1* and *2*, secure a total dollar payoff of *7* and then transfer dollars between themselves to secure any utility payoff in the shaded region of Figure 1b—any payoff distribution between *1* and *2* such that $u_1+u_2\leq 7$. To model this logrolling example with transferable utility, then, necessarily limits the interpretation or scope of the analysis. If the legislators' payoffs correspond to votes won or lost in their respective constituencies, then in addition to trading votes on bills, we must also assume that their constituents' votes (and hence utility) can be transferred as well. Or, equivalently, we must assume that legislators can compensate one another in dollars for lost votes. While we might be interested in studying the consequences of such legislatures, it is inappropriate to so limit ourselves theoretically.

If we abandon the transferable utility formulation of $v(C)$ and hypothesize (for simplicity) that players do not consider lotteries, then, for simple games at least, a far more analytically convenient representation of $v(C)$ can be used. Specifically, instead of assuming that $v(C)$ is in the utility space, R^n, we can assume that $v(C)\subseteq A$, where A is the set of feasible outcomes. If $v(C)$ is the outcomes in A that C can secure then,

$$v(C) = A \text{ if } C \text{ is winning}$$

$$v(C) = \Phi \text{ if } C \text{ is losing.}$$

The utility function u_i, in turn, defines a preference relation for player i over A thus: for any θ, $\theta' \in A$,

$$\theta \left\{\begin{matrix} \succsim_i \\ \succ_i \\ \sim_i \end{matrix}\right\} \theta' \text{ if and only if } u_i(\theta) \left\{\begin{matrix} \geq \\ > \\ = \end{matrix}\right\} u_i(\theta')$$

Thus, in the logrolling example, if C is a majority coalition, $v(C)$ is simply the set of all possible dispositions of all bills while Table 1 summarizes the preferences over these outcomes.

To develop this further and to illustrate the inadequacies of existent solution theory, suppose, as

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[3] To understand the assumptions implicit in the transferable utility formulation of $v(C)$. suppose player i is a consumer and let $x^i=(x_1^i, x_2^i, \cdots, x_m^i)$ denote the amount of each of m commodities that he consumes or possesses. Suppose further that at least one commodity, say the m^{th}, is separable, i.e., that we can write

$$u_i(x_1^i,\cdots,x_m^i) = u_i^{\circ}(x_1^i,\cdots,x_{m-1}^i) + u_i'(x_m^i)$$

which is to say that player i's utility for commodity m is independent of his utility for the first $m-1$ commodities and vice versa. Third, suppose that u_i' is linear in x_m^i. If these assumptions hold for all players and if the m^{th} commodity is freely transferable between and among all players, then we can assume transferable utility. Specifically, if C is winning, it can pick the distribution of the m commodities that maximizes the sum of the utilities of the members of C, and trade commodity m from there. Since utility is separable and linear in x_m, transferring m is equivalent to transferring utility. Hence, we set r_c equal to this maximum attainable sum. We emphasize that the non-transferable utility representation of $v(C)$ does not preclude the possibility that the players can exchange resources, and in fact admits transferable utility as a special case.

many analyses of parliamentary processes assume (e.g., tests of the size principle), that conflict occurs over issues or ideology as when party leaders attempt to form governments by manipulating their policy commitments. In particular, let us adopt the spatial structure used in the several extensions of the "Downsian" model of election competition (c.f., Ordeshook, 1976). That is, we suppose that the set of alternatives, A, confronting a committee is *any* subset of m-dimensional space (in the previous example, A is a discrete subset of R^m), that the dimensions correspond to the criteria players use to evaluate alternative outcomes (so that an outcome can be characterized by its position on each dimension), and that each player possesses a utility function, u_i, over this space. The particular form assumed for u_i is that all players evaluate alternatives in terms of a Euclidean metric: For all $i \in N$ and $\theta \in A$, there is a point $x_i \in R^m$ such that

$$u_i(\theta) = f(\|\theta - x_i\|), \qquad (1)$$

where $f: R \rightarrow R$ is a monotone-decreasing function. The point x_i represents player i's ideal point in A—the issue positions he most prefers—while the utility player i associates with other alternatives in A decreases the further one moves from x_i. Thus, for two dimensions (two issues), a player's indifference contours are concentric circles about x_i, where circles of smaller radius correspond to higher utility levels.

Figure 2 illustrates a 3-player 2-issue representation of a spatial game, and it also illustrates the necessity for abandoning the transferable utility assumption. Specifically, suppose that the coalition $\{1, 2, 3\}$ considers only policies on the line connecting x_1 and x_2 and that u_1 and u_2 are linear along this line so that $u_1 + u_2$ equals a constant. However, $u_1 + u_2 + u_3$ is not a constant since player *3* prefers x_2 to x_1. Thus, $v(1, 2, 3)$ cannot be summarized by a single number. The problem with transferable utility becomes even more apparent for larger games so that to impose this condition we must, as with the logrolling example, assume that the game entails some unspecified, divisible and unrestrictedly transferable commodity over which each player possesses a linear utility function and that each player's utility for this commodity is independent of policy. Schofield (1976) usefully assumes that the allocation of ministerships approximates these conditions, but by thus constraining $v(C)$, the essence of the original problem—policy conflict—is lost in favor of the distribution of some commodity. For example, Riker's size principle, the theoretical justifications of which assume transferable utility, must be interpreted as irrelevant to a broad range of parliamentary coalition processes or, minimally, in need of proof in the more general non-transferable context.

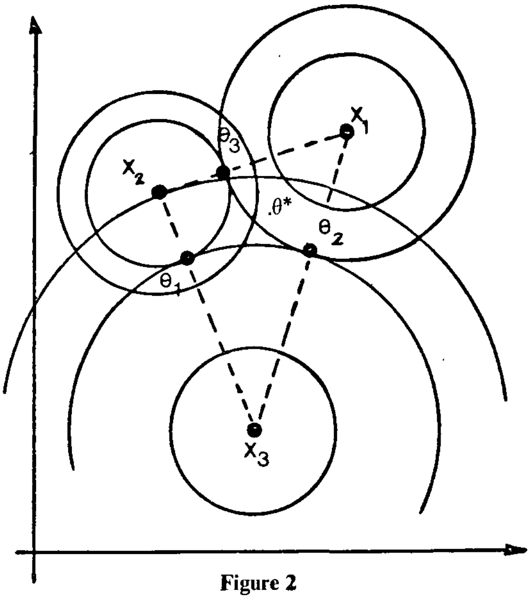

Figure 2

Game theory can be usefully applied only if we let $v(C)$ be a collection of utility n-tuples or, in the case of simple games without lotteries, if we let $v(C)$ be A if C is winning and the empty set if C is losing. A spatial map, such as the one portrayed in Figure 2, is used then to summarize individual preferences over A. Simple spatial games, though, also illustrate the problems one encounters when applying existent solution theory.

Turning to this theory, we consider first the core, $\mathscr{C}$. Briefly $\mathscr{C}$ is defined as all payoff n-tuples in H that are undominated. In the present context, $\mathscr{C}$ corresponds to all outcomes in A that cannot be defeated in a majority vote by some other outcome in A (i.e., $\mathscr{C} = \{\theta \in A \mid \text{for no } \theta' \in A \text{ and } C \in W \text{ is } \theta' \succ_i \theta \text{ for all } i \in C\}$). It is well known that if a committee is concerned with a single issue (if preferences are single-peaked), then $\mathscr{C}$ is non-empty and corresponds to the median preference. For more than one issue, however, the existence of a non-empty core is not as readily assured. For both the 3-person and 5-person examples in Figures 2 and 3, $\mathscr{C}$ is empty. This follows from Plott's (1967) necessary and sufficient conditions for majority rule equilibrium and Sloss's (1973) extension of them. Specifically, if at most one player's ideal policy is at a point presumed to be in $\mathscr{C}$, then that point is in the core *if and only if* we can pair all remaining voters so that the contract curve of each pair includes it. For the case of circular indifference contours, the contract curve is the line connecting the two players' ideal points. Hence, the existence of a non-empty core requires a form of symmetry in the distribution of ideal points that is not satisfied in Figures 2 and 3 and that is sufficiently restrictive to lead to the conclusion that, in general $\mathscr{C}$ is empty for majority rule spatial games.

Spatial counter-examples to the existence of $\mathscr{C}$ are well known, and the lack of existence provides the motivation for considering other game theoretic solution notions, such as the V-set and Bargaining sets. Unfortunately, as we show subsequently, these solutions also cannot be guaranteed to exist generally, and when they do exist, they possess other problems.

In the present context, $V \subseteq A$ is a *V-set* or Von Neumann-Morgenstern solution if it is (1) *internally stable*—if no element of V dominates (defeats under majority rule) another element of V—and (2) *externally stable*—if for every element not in V, there is at least one in V that dominates it. The problem with most games, however, is that they possess too many V-sets and, hence, to restrict predictions in simple games, the ad hoc concept of a main-simple V-set is introduced. Briefly, a V-set is *main-simple* if and only if for each minimal winning coalition, C, there is an element of V, say θ, such that θ is weakly preferred by each member of C to any other element of V.[4]

For the game in Figure 2, note first that $\{\theta_1, \theta_2, \theta_3\}$ is internally stable. For example, player *1* prefers θ_2 to θ_1, player *2* prefers θ_1 to θ_2. and player *3* is indifferent. Hence, θ_1 and θ_2 do not dominate each other under majority rule. It is easily shown that $\{\theta_1, \theta_2, \theta_3\}$ is externally stable as well, since every point in the space (except θ_1, θ_2, and θ_3) is outside at least two of the three tangent circles and, hence, is dominated by the point corresponding to the intersection of these two circles. Thus, $\{\theta_1, \theta_2, \theta_3\}$ is a V-set. And, further, it is a main-simple V-set since the minimal winning coalition $\{1, 2\}$ weakly prefers θ_3 to θ_1 and θ_2, $\{1, 3\}$ weakly prefers θ_2 to θ_1 and θ_3, and $\{2, 3\}$ weakly prefers θ_1 to θ_2 and θ_3.

Because the predictions of main-simple V-sets are intuitively satisfying, most experimental tests of V focus on them (c.f., Riker, 1967; Riker and Zavoina, 1970; Kahn and Rapoport, 1972; Horowitz and Rapoport, 1974). Their definition, however, contains no behavioral justification for supposing that players adopt outcomes in them. As such, V-sets and main-simple V-sets are mathematical inventions without a behavioral rationale. For this reason, Aumann and Maschler (1964) define several variants of the bargaining set—a solution theory with predictions that often correspond closely to the main-simple V-set, but which nevertheless is motivated by some behavioral considerations.

The intuitive idea behind bargaining set theory is that for a particular payoff vector and coalition structure to be "stable," the members of every coalition must be able to "defend" their payoff agaist the possible objections of their partners. In a sense, the players must possess counter-threats to all conceivable threats against them. Rather than present any formal definitions, though, we note simply that the points θ_1, θ_2, and θ_3, in conjunction with the respective coalition structures $(\{2, 3\}, \{1\})$, $(\{1, 3\}, \{2\})$, and $(\{1, 2\}, \{3\})$ are in all of the several variants of the bargaining set. The simple game portrayed in Figure 3, however, is more interesting, since this game does not possess a bargaining set in the traditional sense and since from this it follows that it does not possess a main-simple V-set either.

To begin, consider the point θ and the payoff configuration

$$(\theta; \{1, 2, 3\}, \{4, 5\}),$$

(i.e., the minimal winning coalition $\{1, 2, 3\}$ proposes to exclude players *4* and *5* and to pass θ). Suppose, however, that player *3* feels that the coalition should adopt a policy closer to his ideal preference and consequently he *objects* against player *2* with the configuration (see Figure 3):

$$(\theta^*; \{3, 4, 5\}, \{1, 2\}).$$

In accordance with the definitions of bargaining set theory, this is a legitimate objection since θ^* is Pareto optimal for $\{3, 4, 5\}$, $\{3, 4, 5\}$ is winning, and

$$\theta^* \succ_3 \theta,$$
$$\theta^* \succsim_4 \theta,$$
$$\theta^* \succsim_5 \theta.$$

For player *2* to defend what he is getting from θ, i.e., for $(\theta; \{1, 2, 3\}, \{4, 5\})$ to be a candidate for an element of a bargaining set, player *2* must find a counter-objection—a third payoff configuration such that *3* is excluded from the winning coalition, player *2* does at least as well as he does at θ, and *2*'s new coalition partners do at least as well at θ^* as at θ. To see that player *2* does not possess such a counter-objection, note that since he is prohibited from using player *3*—the objector—in a counter, and since (from the construction of Figure 3) he cannot find a point that gives him at least $u_2(\theta)$ while simultaneously giving player *4* at least $u_4(\theta^*)$, player *2*'s only possible counter is with the coalition $\{1, 2, 5\}$. But, as Figure 3 shows, he cannot lure player *5* away from θ^* and recover $u_2(\theta)$.

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[4] That is, if $C \in W^*$ and if V is main-simple, then there exists a $\theta \in V$ such that $\theta \succsim_i \theta'$ for all $\theta' \in V$. This definition is taken from an equivalence theorem (Theorem 2) by Robert Wilson, (1971).

Hence, there exists an objection that cannot be countered, so (θ; {*1, 2, 3*}, {*4, 5*}) is not in any bargaining set.

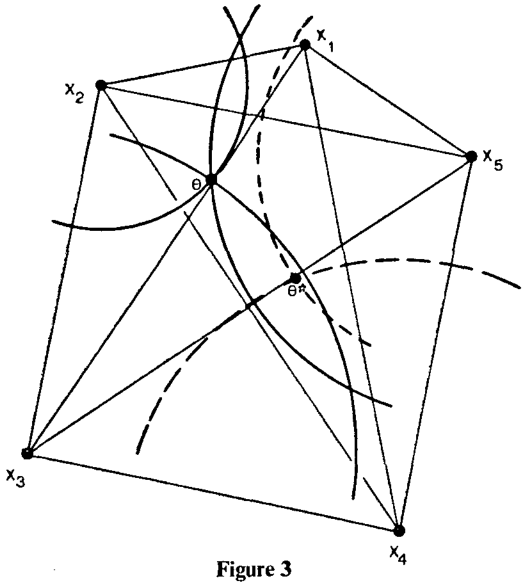

Figure 3

While the choice of θ in this example is not entirely arbitrary (it is a point in the solution we propose later) it can be shown that for every point and for every such coalition, there exist objections that cannot be countered. Thus, by the usual definition of existence (i.e., for every coalition structure, there exists a payoff configuration in the bargaining set), the various bargaining sets are empty. Further, from the correspondence Wilson proves between the main-simple *V*-set and the strong bargaining set and from his result that a main-simple *V*-set is an "admissible" set of proposals by each of the minimal winning coalition, we conclude that *the main-simple V-set is empty as well.*

Is this example a special case? We cannot answer this question definitively since Peleg's (1963) existence results are not relevant here. But, a succession of counterexamples using spatial games (including three-dimensional configurations of ideal points) suggest that, like the core, existence of other solution concepts is assured only under special conditions. An examination of the game in Figure 3 reveals why the principles underlying bargaining set theory are inappropriate for spatial games. Since a coalition cannot freely transfer utility, a coalition's proposal exhibits a particular type of externality. It cannot withhold the benefits of the policy it chooses from anyone, including players that are admitted to the coalition simply to render it winning. Player *2*, for example, cannot defend the payoff he associates with θ, but he cannot also be excluded from this payoff; players *1* and *3* necessarily give player *2* more than he is worth—in violation of the bargaining set notion that every player defend his payoff from *all* possible objections. Since these externalities are inescapable, bargaining set theory yields no stable payoff configuration for {*1, 2, 3*}.

There is, however, an alternative interpretation of this example and of bargaining set theory suggested by Laing and Morrison (1974) that yields an interesting contrast between this theory and the solution concept we propose. Suppose coalition structures that do not possess stable payoff configurations are interpreted as structures that bargaining set theory predicts will not form. For the game in Figure 3 it is possible to show that, with respect to the minimal winning coalitions, the bargaining set makes the following predictions,

Coalitions that will not form (i.e., have no stable payoff configurations)	**Coalitions that can form (i.e., have stable payoff configurations)**
{*1, 2, 3*}	{*1, 3, 4*}
{*2, 3, 4*}	{*2, 4, 5*}
{*3, 4, 5*}	{*1, 3, 5*}
{*4, 5, 1*}	{*1, 2, 4*}
{*5, 1, 2*}	{*2, 3, 5*}

That is, the only minimal winning coalitions with stable payoff configurations are those that geometrically "split the opposition." These configurations, moreover, correspond to proposals in the interior pentagon of Figure 3. It is important to keep these predictions in mind when we turn to the Competitive Solution, and to the experimental results.

The Competitive Solution

Turning to *n*-person games in general (as opposed to only simple or spatial games) the solution concept proposed here hypothesizes that potential coalitions must bid for their members in a competitive environment via the proposals they offer. Given that several coalitions are attempting to form simultaneously, each coalition must, if possible, bid efficiently by appropriately rewarding its "critical" members. Thus, if any one player or set of players is pivotal between two coalitions and if each coalition is to have a chance of forming, the pivotal players should be indifferent between the offers of both coalitions lest their preferences insure that one of the two cannot form. This can result in certain coalitions being unable to compete, and thus, we must identify not only the competitive offers of coalitions, but also, the coalitions that can make them,

To formalize this imprecise and anthropomorphic perspective, we define:

Proposal: A *proposal* of $C \subseteq N$ is an ordered pair $(u: C)$ such that $u \in v(C)$ and $u \in v(N)$.

Viable: For any two proposals $(u_1; C_1)$ and $(u_2; C_2)$, $(u_1; C_1)$ is *viable* against $(u_2; C_2)$ if it is not the case that $u_1 <_i u_2$ for all $i \in C_1 \cap C_2$.[5]

For any two coalitions C_1, $C_2 \subseteq N$, the set $C_1 \cap C_2$ is referred to as the *pivotal players between C_1 and C_2*. The proposal $(u_1; C_1)$ is viable against $(u_2; C_2)$, then, if the pivotal players do not strictly prefer the proposal of the coalition into which they pivot. Now let $\mathscr{K}$ be any set of proposals. A proposal $(u; C)$ is *viable in $\mathscr{K}$* if it is viable against all proposals in $\mathscr{K}$. Then,[6]

Balanced: $\mathscr{K}$ is *balanced* if
(i) For any distinct $(u; C)$ and $(u'; C') \in \mathscr{K}$, $C \neq C'$.
(ii) Every $(u; C) \in \mathscr{K}$ is viable in $\mathscr{K}$.

Condition i requires that each coalition represented in $\mathscr{K}$ can have exactly one proposal. Condition ii requires all proposals in $\mathscr{K}$ to be viable against each other.

There may exist many distinct balanced sets of proposals. We focus, then, on the class of proposals in which the coalitions represented in $\mathscr{K}$ make offers that are as attractive as possible to their respective critical members. To define this precisely, we say that

Upset: A proposal $(u; C)$ *upsets* $\mathscr{K}$ if
(i) $(u; C)$ is viable in $\mathscr{K}$.
(ii) for some $(u'; C') \in \mathscr{K}$ $u >_i u'$ for all $i \in C \cap C'$.

Then, letting $M_{\mathscr{K}}$ be the set of coalitions represented in $\mathscr{K}$, i.e., $M_{\mathscr{K}} = \{C \subseteq N \mid \text{for some } (u'; C') \in \mathscr{K}, C' = C\}$, we define

Competitively Balanced: $\mathscr{K}$ is *competitively balanced* if it is balanced and if for no $C \in M_{\mathscr{K}}$ is there a proposal $(u; C)$ that upsets $\mathscr{K}$.

Clearly, competitively balanced sets are particular to an assumed coalition structure: with a different initial assumption about the coalitions that make proposals, we might arrive at a different competitively balanced set. The dependence of $\mathscr{K}$ on $M_{\mathscr{K}}$ can be viewed as an advantage since it permits us to model situations in which there are legal or sociological restrictions on alignments. Nevertheless, a completely general solution should admit the possibility that *any* coalition can enter the bidding, and to this end we say that

$\mathscr{K}$ is a Competitive Solution if $\mathscr{K}$ is balanced and if there is no proposal $(u; C)$ that upsets $\mathscr{K}$.

As a possible further refinement of this definition, we note that the notion of "balanced" used to formulate this definition permits pivotal *groups* of players to weakly prefer one proposal over another. Later, we illustrate this possibility and show how it raises problems for the hypothesis of minimal winning coalitions. But, at least for the examples we examine, a stronger definition of "balanced" and, hence, of $\mathscr{K}$, excludes coalitions greater than minimal winning size.

Strongly Balanced: $\mathscr{K}$ is strongly balanced if it is balanced and, if for no $(u_1; C_1)$, $(u_2; C_2) \in \mathscr{K}$, is it the case that

$$u_1 \geq_i u_2$$

for all $i \in C_1 \cap C_2$, with $>_i$ holding for at least one $i \in C_1 \cap C_2$.

For a stronger version of $\mathscr{K}$, then, we have,

$\mathscr{K}$ is a Strong Competitive Solution if it is a Competitive Solution that is strongly balanced.

Before illustrating these definitions, we observe that there is a close correspondence between $\mathscr{K}$ and the solution concepts of the core, the V-set, and the bargaining set. This, of course, is desirable since the imputations identified by main-simple V-sets, for example, often are intuitively plausible. Further, there already exists some experimental support for these predictions as well as the predictions of the core and the bargaining set. Theorems 1 and 2, then, formally establish a relationship between $\mathscr{K}$ and the core, $\mathscr{C}$, and the V-set (for proofs, see McKelvey and Ordeshook, 1978).

Theorem 1: If $u \in \mathscr{C}$, then $\mathscr{K} = \{(u; N)\}$ is a Strong Competitive Solution.

Theorem 2: If V is a main-simple V-set for a strong simple game, then there is a Strong Competitive Solution $\mathscr{K}$ such that $(u; C) \in \mathscr{K}$ $\Rightarrow C \in W^*, u \in V$.

With respect to the bargaining set, Wilson shows that for all simple games, the "strong" bargaining set is equivalent to the main-simple V-solution. It follows from Theorem 2, then, that the strong bar-

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[5] In an earlier version of this paper we also required for viability that

$$u_1 \geq_i u_2 \quad \text{for all} \quad i \in C_1 - (C_1 \cap C_2)$$

i.e., that the non-pivotal players weakly prefer their coalition's proposal. For simple games this condition is redundant. For non-simple games, though, this modification in the definition of viability may be necessary to insure the existence of a Competitive Solution.

[6] The word "balanced" is unrelated to and should not be confused with the notions of "balanced collection" and "balanced game" that are used to establish sufficient conditions for non-empty cores. See, for example, Herbert E. Scarf [1967].

gaining set is a Strong Competitive Solution.

The links Theorems 1 and 2 establish between $\mathscr{K}$ and $\mathscr{C}$ and between $\mathscr{K}$ and V are important. A great deal is already known about $\mathscr{C}$ and V mathematically. More importantly, though, if the definition of $\mathscr{K}$ is behaviorally meaningful, these links suggest that $\mathscr{K}$ can provide a behavioral motivation for solution concepts that presently are little more than mathematical inventions.

To illustrate this, as well as the several earlier definitions, consider again the logrolling example from Table 1 and the following proposals:

Majority Decision	**Proposal**	
pass only bills *1* and *2*	$(4, 3, -6; \{1, 2\})$	$\equiv \mathscr{K}$
pass only bills *1* and *3*	$(4, -4, 3; \{1, 3\})$	
pass only bills *2* and *3*	$(-2, 3, 3; \{2, 3\})$	

Note that the coalitions offering proposals here are winning and, hence, these are legitimate proposals (i.e., $u \in v(C)$). Second, note that these proposals are viable against each other and thus are (strongly) balanced; for example, player *1* pivots between the proposals of $\{1, 2\}$ and $\{1, 3\}$ and holds no strict preference either way. Further, referring to Table 1, no minimal winning coalitions can offer an alternative proposal that makes all of its members strictly better off—these proposals cannot be upset by $\{1, 2\}$, $\{1. 3\}$ or $\{2, 3\}$. Hence, $\mathscr{K}$ is competitively balanced. Finally, to check whether $\mathscr{K}$ is a competitive solution, suppose that the coalition of the whole, $\{1, 2, 3\}$ attempts to enter the bidding with a proposal of its own. But, this is impossible since for any proposal by $\{1, 2, 3\}$, the pivotal players will correspond to a minimal winning coalition and we know already that there does not exist another possibility that makes all members of any such coalition strictly better off. That is, condition ii in the definition of upset cannot be satisfied by any proposal for $\{1, 2, 3\}$.

Consider, on the other hand, the following proposals:

$$\left\{\begin{array}{l}(4, 3, -6; \{1, 2, 3\})\\ (4, -4, 3; \{1, 3\})\\ (-2, 3, 3; \{2, 3\})\end{array}\right\} \equiv \mathscr{K}'$$

$\mathscr{K}'$ is identical here to $\mathscr{K}$ except that $\{1, 2, 3\}$ proposes the payoffs $(4, 3, -6)$ rather than $\{1, 2\}$. Following the earlier argument, it follows that $\mathscr{K}'$ is a competitive solution as well. It is *not*, however, a *strong* solution. For example, the pivots between $\{1, 2, 3\}$ and $\{1, 3\}$—players *1* and *3*—weakly prefer $(4, -4, 3)$ to $(4, 3, -6)$. In this instance, then, the strong but not the weak version of $\mathscr{K}$ predicts that only minimal winning coalitions form.

It is possible to show now that the payoffs in these three proposals also constitute a main-simple V-set. However, the advantage of $\mathscr{K}$ is that, by linking payoffs to specific coalitions, $\mathscr{K}$, but not V, tells us the vote trades that correspond to particular payoff outcomes. For example, the proposal $(4, 3, -6; \{1, 2\})$ implies that player *1* trades his vote on bill *2* for player *2*'s vote on bill *1*. Similarly, $(-2, 3, 3; \{2, 3\})$ implies that player *2* trades his vote on bill *3* for player *3*'s vote on bill *2*.

The preceding theorems and example should not be interpreted to mean that there is correspondence between $\mathscr{K}$ and V-sets for all games. To illustrate, consider the five-person, five-bill logrolling game described in Table 2. We assume as before that utility is separable across bills so that, for example, legislator *2*'s utility from passing bills *2*, *3*, and *4* is -4 (i.e., $0+1-2-3+0$). Excluding lotteries, the following outcomes correspond to the unique Competitive Solution to this majority game.

Coalition	**Majority Decision**
$\{1, 2, 3\}$	pass bill *1*, defeat remainder
$\{2, 3, 4\}$	pass bill *2*, defeat remainder
$\{3, 4, 5\}$	pass bill *3*, defeat remainder
$\{1, 4, 5\}$	pass bill *4*, defeat remainder
$\{1, 2, 5\}$	pass bill *5*, defeat remainder

Table 2. 5-Person, 5-Bill Logrolling Example

	Utility from Individual Bills if Passed					
Legislator	**None**	**1**	**2**	**3**	**4**	**5**
1	0	1	−2	−3	1	2
2	0	2	1	−2	−3	1
3	0	1	2	1	−2	−3
4	0	−3	1	2	1	−2
5	0	−2	−3	1	2	1

$\mathscr{K}$, then, does predict vote trading since, if everyone voted sincerely, all bills pass, yielding the payoff vector $(-1, -1, -1, -1, -1)$. More interestingly, though, note that the payoffs corresponding to outcomes in $\mathscr{K}$ do not constitute a V-set: the condition of internal stability is not satisfied. For example, the payoff vector $(1, 2, 1, -3, -2)$, which corresponds to $\{1, 2, 3\}$'s proposal, dominates $(-2, 1, 2, 1, -3)$, which corresponds to $\{2, 3, 4\}$'s proposal. In fact, excluding lotteries, this game does not possess a main simple V-set.

Spatial Games

The preceding example is a prelude to the application of $\mathscr{K}$ to spatial games. Recalling that $u_i(\theta)=f(\|\theta-x_i\|)$, the following definitions are useful for analyzing such games. Letting

> $\bar{V}_C = \{\theta \mid \theta \in A$ and for no other $\theta' \in A$ is it the case that $\theta' \succ_i \theta$ for all $i \in C\}$

then,

> a coalition $C \in W$ is *external* if there is some other coalition $C' \in W$ with $C \cup C' = N$, $C \cap C' = \{i\}$ for some $i \in N$ and $\bar{V}_C \cap \bar{V}_{C'} = \{x_i\}$. Any winning coalition that is not external is *internal*.

In Figure 3, for example, the coalitions not possessing stable payoff configurations—predicted not to form by bargaining set theory—are external while those with stable configurations are internal. This is particularly interesting in light of the following conjecture, which assists us in finding solutions:

> *Conjecture:* In any spatial game there exists a competitive solution, $\mathscr{K}$, such that if $(\theta; C) \in \mathscr{K}$, then C is an external coalition.

We can prove this conjecture for $n \leq 5$ person games and are unable to find a counterexample in any larger game for which $\mathscr{K}$ has been computed. Despite the fact that we are unable to prove this conjecture more generally, it is especially interesting because of two properties of external coalitions in majority rule games: (1) if C is external, then it is minimal winning, and (2) if C is external, then it is *connected*, where by connected we mean that for no $i \in N$, $i \notin C$, is it the case that x_i is in the interior of $\bar{V}_C$. Hence, the close correspondence between $\mathscr{K}$ and at least two hypotheses about legislative and parliamentary coalitions suggests the possibility of deducing these hypotheses directly from a formal solution concept.

Finding a solution based on the external coalitions, moreover, is rendered easier oftentimes by the following property: If $C \in M_{\mathscr{K}}$, and $i \in C$, i is *critical* if $C \cap C' = \{i\}$, for some $C' \in M_{\mathscr{K}}$. Then,

> *Property 1:* If a set of proposals $\mathscr{K}$ is balanced and if for all $(\theta; C) \in \mathscr{K}$, $\theta \in \bar{V}_{C^*}$, where $C^* \subseteq C$ is the set of critical members in C, then $\mathscr{K}$ is competitively balanced.

We illustrate the application of this result by proceeding directly to some examples of spatial games.

Example 1, Three Players. Consider the three-person spatial game portrayed in Figure 2, and recall that $\{\theta_1, \theta_2, \theta_3\}$ corresponds there to a main-simple V-set and to a bargaining set. Turning to the Competitive Solution, the set of minimal winning coalitions is $W^* = \{\{1, 2\}, \{1, 3\}, \{2, 3\}\}$, which is also the set of external winning coalitions. Hence, we let $M = W^*$ and seek a set of balanced proposals, based on M satisfying Property 1. We have then, the following coalitions, individual pivots and proposals.

External Coalitions	Individual Pivots	Policy Proposal
$\{2, 3\}$	2, 3	$\theta_1 \in \bar{V}_{23}$
$\{1, 3\}$	1, 3	$\theta_2 \in \bar{V}_{13}$
$\{1, 2\}$	1, 2	$\theta_3 \in \bar{V}_{12}$

Letting $\mathscr{K} = \{(\theta_1; \{2, 3\}), (\theta_2; \{1, 3\}), (\theta_3; \{1, 2\})\}$, where θ_i is the proposal of the coalition that excludes player i, $\mathscr{K}$ is balanced only if

$$u_1(\theta_2) = u_1(\theta_3)$$
$$u_2(\theta_1) = u_2(\theta_3)$$
$$u_3(\theta_1) = u_3(\theta_2)$$

otherwise player 1 is not indifferent between the two proposals, θ_2 and θ_3, for which he is a pivot, and so on. The solution to these three expressions is, of course, the joint pairing of tangencies portrayed in Figure 2.[7] Further, since θ_1 is obviously in $\bar{V}_{23}$, and so on, the conditions of Property 1 are satisfied. To check then whether $\mathscr{K}$ is a Competitive Solution, we need only consider the possibility that the coalition of the whole can upset $\mathscr{K}$ by entering the bidding. First it follows that any proposal of N, say θ, cannot correspond to θ_1, θ_2, or θ_3. For if it does, it will not upset $\mathscr{K}$. But if $\{1, 2, 3\}$ proposes some other policy, say θ^*, θ^* is not viable against θ_1 since the pivots between $\{2, 3\}$ and $\{1, 2, 3\}$ both prefer θ_1. Thus, $\mathscr{K}$ is a Competitive Solution.

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[7] The exact location of the solution points is given by

$$\theta_i = \frac{r_j}{r_j + r_k} x_i + \frac{r_k}{r_j + r_k} x_j \quad \text{for} \quad i, j, k \in \{1, 2, 3\}$$

where for $1 \leq i \leq 3$,

$$r_i = \frac{\|x_i - x_j\| - \|x_j - x_k\| + \|x_i - x_k\|}{2}$$

It is easily seen, moreover, that $\mathscr{K}$, as defined above, constitutes a *Strong* Competitive Solution. We note, however, that,

$$\{(\theta_1; \{2, 3\}), (\theta_2; \{1, 3\}), (\theta_3; \{1, 2, 3\})\}$$

which admits a greater than minimal winning coalition, is a Competitive Solution as well, but it is *not* a strong solution. Specifically, the pivots *2* and *3* between $\{2, 3\}$ and $\{1, 2, 3\}$ weakly prefer θ_1 to θ_3 (i.e., player *2* is indifferent between θ_1 and θ_3, but player *3* strictly prefers θ_1 to θ_3). Thus, as in the previous logrolling examples, *a Strong Competitive Solution eliminates coalitions that are greater than minimal winning.*

Example 2, Four Players. A four-person spatial game with Euclidean utility is interesting here only if the dimensionality of the policy space exceeds *2* or if the players are not equally weighted. Otherwise, the game possesses a Core. Consider, then, a four-person weighted voting game in which $W^* = \{\{1, 2\}, \{1, 3\}, \{1, 4\}, \{2, 3, 4\}\}$, which could model a cabinet formation game, where the voting weights, w_i, of the players (parties) are: $w_1 = 40$, $w_2 = w_3 = w_4 = 20$. Suppose now that the set of external coalitions is $\{1, 2\}$, $\{1, 4\}$, $\{2, 3, 4\}$, as portrayed in Figure 4. (Note that $\{1, 3\}$ is not external since it "divides the opposition.") We proceed, then, by setting M equal to this set and by solving for points that satisfy Property 1.

External Coalitions	Individual Pivots	Policy Proposal of Coalition
$\{1, 2\}$	$1, 2$	$\theta_{12} \in \bar{V}_{12}$
$\{1, 4\}$	$1, 4$	$\theta_{14} \in \bar{V}_{14}$
$\{2, 3, 4\}$	$2, 4$	$\theta_{234} \in \bar{V}_{24}$

Figure 4 portrays the appropriate competitively balanced set:

A quick check reveals that these three proposals are *strongly* competitively balanced. To see that they constitute a strong *Competitive Solution* we need to check whether $\{1, 3\}$ and the greater than minimal winning coalitions can upset $\mathscr{K}$ by entering the bidding. For brevity, we consider only $\{1, 3\}$ here. First, if $\{1, 3\}$'s proposal is to be viable, it must provide player *1* with at least as much utility as does θ_{12} and θ_{14}, since player *1* is pivotal between $\{1, 3\}$ and $\{1, 2\}$ and between $\{1, 3\}$ and $\{1, 4\}$. Similarly, $\{1, 3\}$'s proposal must give player *3* at least as much as he gets from θ_{234}. But, as is evident from Figure 4, both conditions cannot be satisfied simultaneously. Hence, $\{1, 3\}$ cannot compete. Since a similar argument holds for the greater than minimal winning coalitions, $\mathscr{K} = \{(\theta_{12}; \{1, 2\}), (\theta_{14}; \{1, 4\}), (\theta_{234}; \{2, 3, 4\})\}$ constitutes a Strong Competitive Solution.

It is interesting to observe in this example that $\mathscr{K}$ also constitutes a V-set (but, not main-simple). However, unlike V-set theory, $\mathscr{K}$ also predicts that *only* minimal winning coalitions form *and* that the minimal winning coalition $\{1, 3\}$ does not form. $\mathscr{K}$, then, provides a stronger and more readily tested prediction.

To ascertain the influence of spatial location, though, consider the four-person configuration in Figure 5, which differs from the configuration in Figure 4 only in that $\{1, 3\}$ is now external. Hence, we set $M = W^*$, and

External Coalitions	Individual Pivots	Policy Proposal of Coalition
$\{1, 2\}$	$1, 2$	$\theta_{12} \in \bar{V}_{12}$
$\{1, 3\}$	$1, 3$	$\theta_{13} \in \bar{V}_{13}$
$\{1, 4\}$	$1, 4$	$\theta_{14} \in \bar{V}_{14}$
$\{2, 3, 4\}$	$2, 3, 4$	$\theta_{234} \in \bar{V}_{234}$

Figure 5 portrays the corresponding (strong) competitively balanced set, which is (without proof) a

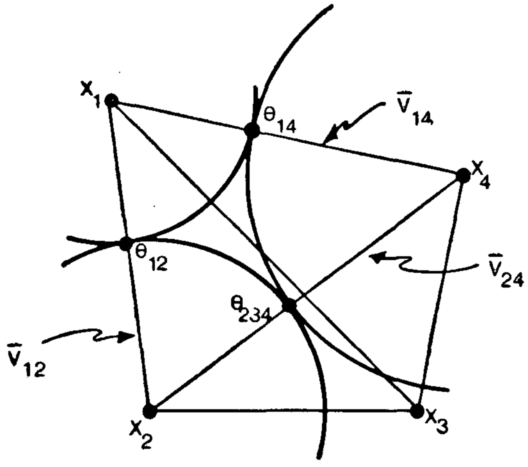

Figure 4

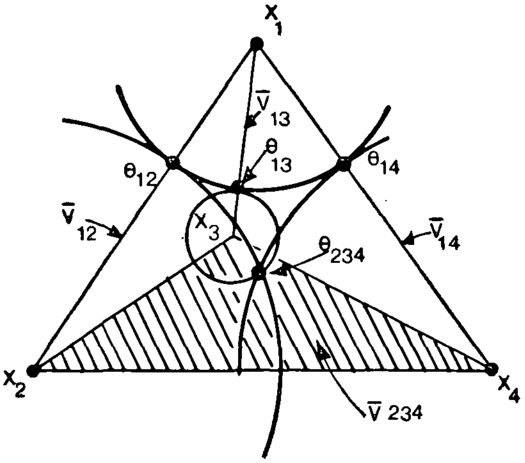

Figure 5

Strong Competitive Solution. Clearly, then, $\mathscr{K}$ is sensitive to spatial location since, now, $\{1, 3\}$ has a viable proposal.

Example 3, Five-Person Games. The variety of types of spatial games becomes quite rich for five-player games, and it is here that the correspondence between $\mathscr{K}$ and the V-set disappears. To ease exposition we consider only simple games in R^2 without weighted voting. Nevertheless, the location and number of the external coalitions can vary considerably, depending on the configuration of ideal points. Specifically, let $G \subseteq R^m$ be the convex hull of the players' ideal points, and let G^{int} be the interior of this set. There are, then, three classes of ideal point configurations:

a) no player's ideal point is in G^{int}
b) exactly one player's ideal point is in G^{int}
c) exactly two player's ideal points are in G^{int}

We illustrate here only cases (a) and (b).

(a) *No* $x_i \in G^{int}$: A typical configuration of type (a), which reproduces Figure 3, is portrayed in Figure 6. Since here $\{\{1, 2, 3\}, \{2, 3, 4\}, \{3, 4, 5\}, \{4, 5, 1\}, \{5, 1, 2\}\}$ is the set of external coalitions, we let M equal this set and, let $\mathscr{K}$ be the set of proposals on the joint tangencies of the pivotal players, as illustrated in Figure 6.[8]

External Coalitions	Individual Pivots	Group Pivots	Policy Proposal of Coalition
$C_1 = \{5, 1, 2\}$	$5, 2$	$\{5, 1\}, \{1, 2\}$	$\theta_1 \in \bar{V}_{52}$
$C_2 = \{1, 2, 3\}$	$1, 3$	$\{1, 2\}, \{2, 3\}$	$\theta_2 \in \bar{V}_{13}$
$C_3 = \{2, 3, 4\}$	$2, 4$	$\{2, 3\}, \{3, 4\}$	$\theta_3 \in \bar{V}_{24}$
$C_4 = \{3, 4, 5\}$	$3, 5$	$\{3, 4\}, \{4, 5\}$	$\theta_4 \in \bar{V}_{35}$
$C_5 = \{4, 5, 1\}$	$4, 1$	$\{4, 5\}, \{5, 1\}$	$\theta_5 \in \bar{V}_{41}$.

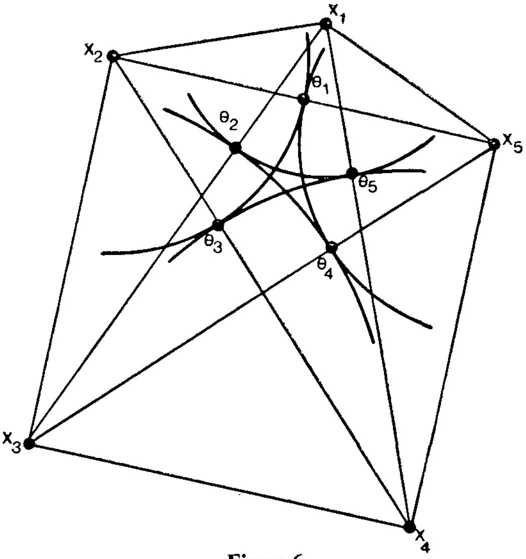

Figure 6

We emphasize that this method of solving for the Competitive Solution is valid only if the pivotal *groups* do not hold preferences among the proposals of the coalitions for which they are pivotal. A quick glance at Figure 6 reveals that this condition holds. For example, $\{1, 2\}$ pivots between θ_1 and θ_2; but while player *1* prefers θ_1 to θ_2, player *2* prefers θ_2 to θ_1. This argument reveals also, of course, that $\mathscr{K} = \{(\theta_i, C_i) | 1 \leq i \leq 5\}$ is a *Strong* Competitive Solution. It is not, however, a V-set; while $\mathscr{K}$ is externally stable it is not internally stable. Specifically, θ_5 dominates θ_1 via the coalition $\{3, 4, 5\}$, θ_2 dominates θ_5 via $\{2, 3, 4\}$, and so on. This game does possess V-sets, but, while we may be missing something, those we can identify are discriminatory and consist of arc segments that together justify every point in the interior pentagon of Figure 6.[9] None possess the simple

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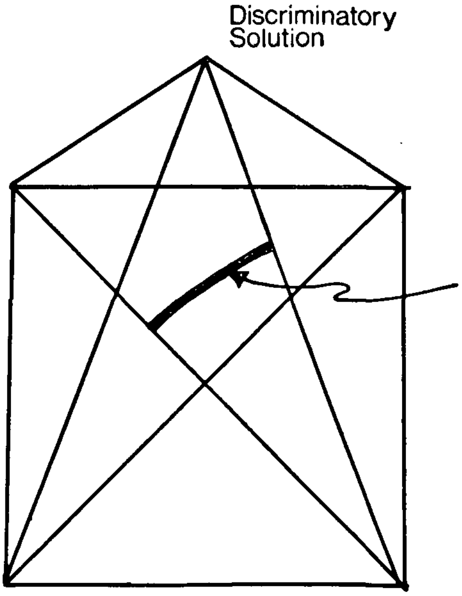
[8] The exact location of the solution points is as follows: If j, k, l, m, $n \in N$, with $k = j + 1$ (mod 5), $l = k + 1$ (mod 5), etc., then

$$\theta_j = \tfrac{1}{2}[s_{km} - s_{mj} + s_{jl} - s_{ln} + s_{nr}]$$
$$r_n = \tfrac{1}{2}[s_{nk} - s_{kn} + s_{mj} - s_{jl} + s_{ln}]$$

and $s_{pq} = \|x_p - x_q\|$ for any p, $q \in N$. C.f. McKelvey and Ordeshook (1978) for a description of an algorithm for locating $\mathscr{K}$ for general configurations of ideal points and also for more general types of preferences.

[9] An illustrative discriminatory V-solution is portrayed in the figure below:



intuitive geometric plausability of $\mathcal{K}$. More importantly, though, *$\mathcal{K}$ predicts exactly those coalitions that bargaining set theory can be interpreted to predict will not form, while none of the coalitions predicted by bargaining set theory are predicted by $\mathcal{K}$.* The experimental use of this example, then, should provide a critical test of these alternative solution theories.

(b) *Exactly one* $x_i \in G^{im}$: Thus far in the spatial examples, all Competitive Solutions are also strong solutions. Consider, however, the configuration in Figure 7. The set of external coalitions here is $\{\{1, 5, 4\}, \{1, 5, 2\}, \{2, 5, 3\}, \{4, 5, 3\}, \{2, 3, 4\}, \{1, 3, 4\}\}$, and these are the coalitions for which we first attempt to find a competitively balanced set of proposals.

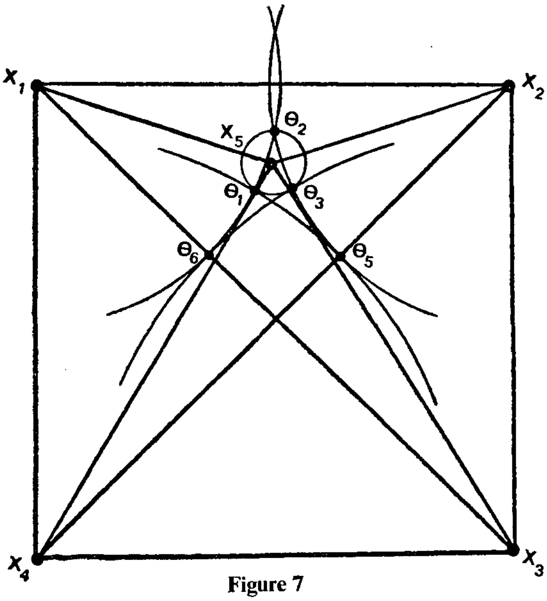


Figure 7

External Coalitions	Individual Pivots	Group Pivots	Policy Proposal of Coalition
$C_1=\{1,5,4\}$	$4,5$	all	$\theta_1 \in \overline{V}_{45}$
$C_2=\{1,5,2\}$	$1,5,2$	$\{1,5\},\{2,5\}$	$\theta_2 \in \overline{V}_{152}$
$C_3=\{2,5,3\}$	$3,5$	all	$\theta_3 \in \overline{V}_{35}$
$C_4=\{4,5,3\}$	5	all	θ_4
$C_5=\{2,3,4\}$	$2,4$	$\{2,3\},\{3,4\}$	$\theta_5 \in \overline{V}_{24}$
$C_6=\{1,3,4\}$	$1,3$	$\{3,4\},\{1,4\}$	$\theta_6 \in \overline{V}_{13}$

In this example, however, we cannot use Property 1. To see this, note that $\theta_4 \in \overline{V}_5$ implies $\theta_4 = x_5$. And, since player *5* is the sole individual pivot between the coalitions $\{4, 5, 3\}$ and $\{1, 5, 2\}$, *5* must be indifferent between θ_2 and θ_4, i.e., $\theta_2 = x_5$. But, now the proposals θ_5 and θ_6 are preferred by $\{3, 4\}$ to the proposal of $\{4, 5, 3\}$ and, since $\{3, 4\}$ pivots between $\{4,5,3\}$ and $\{2,3,4\}$, the conditions for $\mathcal{K} = \{(\theta_i, C_i) | 1 \le i \le 6\}$ being balanced are violated. Despite the fact that we cannot use Property 1, we can nevertheless find a balanced set of proposals. We do not solve for them here analytically, but the five proposals portrayed in Figure 7 constitute a Competitive Solution. Note that the proposal θ_4 is excluded since the coalition $\{3, 4, 5\}$ has no viable proposal.

As before, the five proposals of coalitions in $\mathcal{K}$ which are portrayed in Figure 7 do not constitute a V-solution: while they are externally stable, they are not internally stable. Specifically, θ_3 and θ_1 dominate θ_2 via the coalition $\{3, 4, 5\}$. Unlike our previous examples, however, $\mathcal{K}$ is not a *Strong* Competitive Solution: the pivots $\{1, 4\}$ weakly prefer θ_6 to θ_1. In fact this particular configuration does not appear to possess a strong solution.

Experimental Results

This section reports a preliminary test of the Competitive Solution's empirical validity. The correspondence established in Section *2* between $\mathcal{K}$ and the Core, the V-set, and bargaining set theory suggest that much experimental research which tests these concepts is also a test of the Competitive Solution. We cannot review this literature here except to say that its conclusions are encouraging but fragmentary. But also, almost without exception, this experimental research assumes that utility is linear in money and that money (and thus utility) is freely transferable. The experiments we report here use money to induce preferences, but we do not permit money to be transferred among players. Hence, the context of these experiments is a game without transferable utility.

The game we use is the simple five-person spatial game portrayed in Figure 6, which is particularly interesting because, first, it does not possess a main-simple V-set. Second, the discriminatory V-sets justify outcomes in the interior pentagon of Figure 6, while the Strong Competitive Solution predicts five distinct points on this pentagon's hull. Third, $\mathcal{K}$ is not internally stable and thus any apparent empirical support for the Competitive Solution here would seriously question the behavioral imperative of the internal stability property of V-sets. Finally, $\mathcal{K}$ predicts that only five of the ten minimal winning coalitions possess proposals and these five are exactly those that do not correspond to stable payoff configurations under bargaining set theory.

Briefly, eight games were played using 33 students from the MS program of C-MU's Graduate School of Industrial Administration. All participants had played a three-person spatial game previously to familiarize them with the game's concepts and rules, but no two subjects played together more than once. Further, no subject received any formal training in game theory nor were any of them, at any time, instructed about the Competitive Solution.

The format of these experiments is detailed elsewhere and so we note only briefly here that the players' task is to reach a majority agreement on some point on a grid corresponding to the game in Figure 6.[10] To induce preferences, each player is given a grid that portrays several of his indifference contours and each contour is assigned a dollar value that decreases as the distance from x_i increases. No player knows another's payoff (but they do know each other's preferences, including each other's ideal preferences). To retain the non-transferable utility context of the experiment, players are prohibited from any discussion of schemes to divide their winnings or, in fact, from any mention of money whatsoever during the experiment. Bargaining is allowed to proceed freely, and the experiment is terminated when members of a majority coalition agree to jointly sign a form indicating the final agreement. Each player (including those not in the majority coalition) is then paid in accordance with his payoff schedule and the agreement point.

The players' payoff schedules take either of two forms—linear or nonlinear (see Figure 8). We did not, however, mix forms in a game. While this procedure might permit players to infer the schedules of others from their own, the players were told that there is no symmetry to payoff schedules and that they cannot infer the schedules of others from their own. Ideal point payoffs range between $*12* and $*23* while zero is a player's minimum payoff. The approximate total payoff in each game is $*25*.

Assuming the validity of these procedures, we know that this game does not possess a main-simple *V*-set or a bargaining set for the external coalitions, whereas Figure 6 displays its Strong Competitive Solution. Figure 9 contrasts this solution with the actual outcomes of the experiments while Table 3 summarizes the relevant data.

Even a casual glance at these results reveals a remarkable correspondence between outcomes and the Competitive Solution's predictions. First, in only one game (No. 5) is there a "significant" deviation from the Solution. Second, all outcomes lie in the Pareto optimals of the winning coalitions that proposed them. Finally, *all five external minimal winning coalitions predicted by $\mathcal{K}$ formed at least once and only these coalitions formed.* Slated differently, *no coalition formed that corresponds to a stable payoff configuration under bargaining set theory.*

Figure 9 reveals another interesting pattern in the outcomes. Noting that the various discriminatory *V*-sets predict outcomes in the interior pentagon, note also that no outcome in these experiments lies in this pentagon. *To the extent, then, that the data support the Competitive Solution, they also weaken the argument for V-sets as appropriate definitions of a solution.* Instead, all deviations occasioned by, for example, the coali-

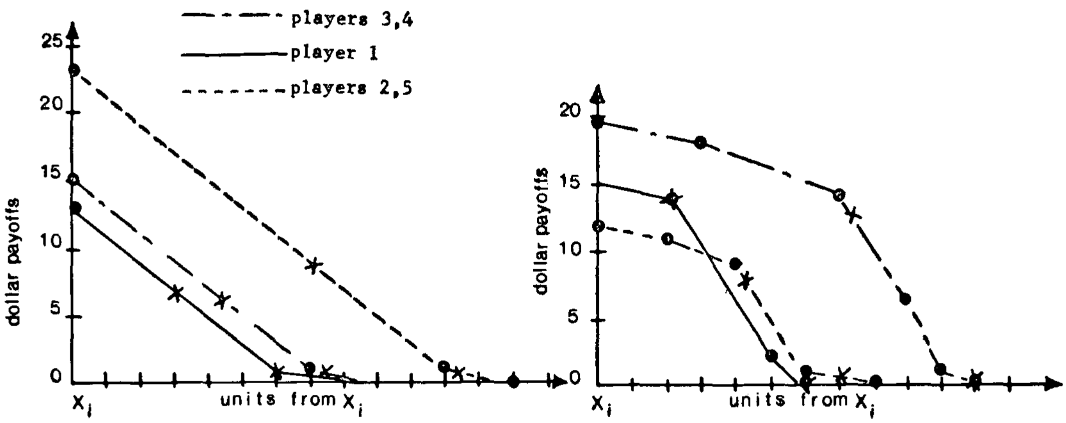


Figure 8

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[10] In the context of testing the core as a solution concept, the structure of these experiments and the instructions read to subject (with obvious appropriate modifications) is given in J. Berl, R. McKelvey, P. C. Ordeshook, and M. Winer, 1976. We would like to note, in addition, that the procedures used here closely parallel those of Morris Fiorina and Charles Plott (1978).

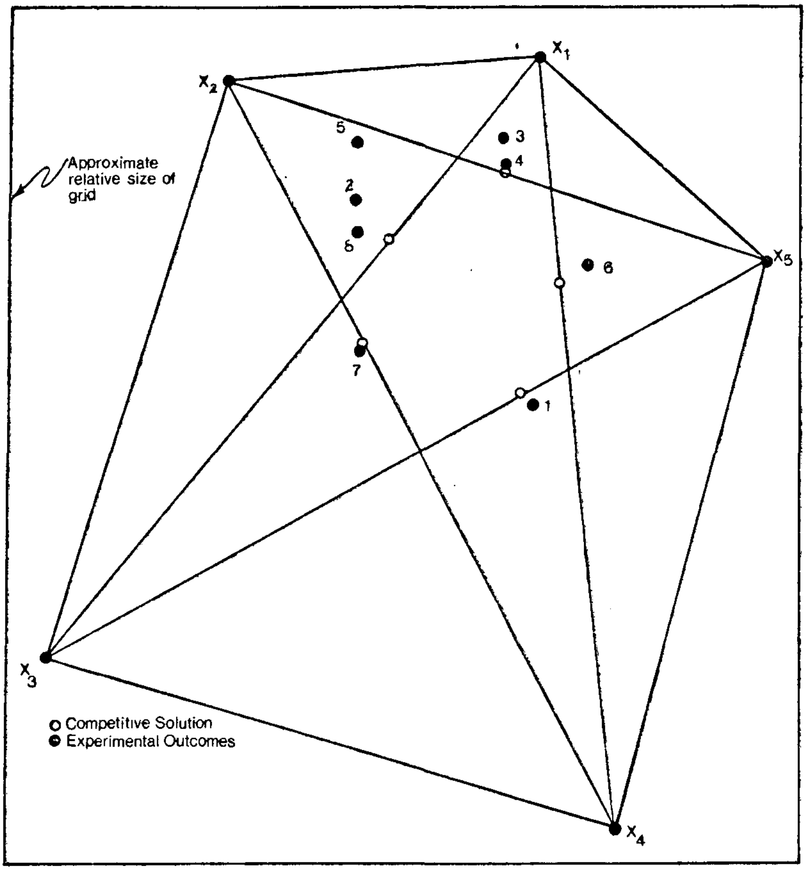
X1
X2
X3
X4
X5
Approximate relative size of grid
5
3
4
2
6
8
7
1
○ Competitive Solution
● Experimental Outcomes

Figure 9

Table 3. Summary of Experiments

Game and Type	Payoffs—Predicted/Actual 1	2	3	4	5	Coalition
1. Linear	1.25/1.00	.75/.75	5.00/5.00	9.00/9.00	5.00/5.00	{3, 4, 5}
2. Non-linear	6.00/6.00	9.25/10.00	6.00/6.00	.50/0	1.50/1.50	{1, 2, 3}
3. Non-linear	13.75/14.25	5.00/5.00	0/0	0/.50	5.00/5.00	{1, 2, 5}
4. Linear	8.50/8.50	5.00/5.00	1.00/1.00	1.00/1.00	5.00/5.00	{1, 2, 5}
5. Linear	5.00/6.00	8.25/10.00	5.00/3.25	2.00/1.00	1.50/1.00	{1, 2, 3}
6. Non-linear	6.00/7.50	.50/.50	.50/0	6.00/4.00	9.25/9.75	{1, 4, 5}
7. Non-linear	.50/0	5.00/5.00	13.00/13.00	6.00/6.00	.50/.25	{2, 3, 4}
8. Non-linear	6.00/5.00	9.25/9.75	6.00/7.00	.50/0	.50/.50	{1, 2, 3}

tion {*1*, *2*, *3*} favor player *2*, those occasioned by the coalition {*5*, *1*, *2*} favor player *1*, and so on. That is, in every game the two players furthest apart (the critical members of the coalition) appear to be "buying" the third off. Thus, it seems as if the third player has some bargaining power in the coalition, which, of course, is untrue according to the theory since this player does not pivot into another coalition with a proposal in $\mathcal{K}$ (i.e., the player is not critical). Given the properties of the players' indifference contours, however, we note that small deviations towards *2*'s ideal away from player *1* and *3*'s contract curve decrease *1* and *3*'s payoff by only pennies, whereas the benefit to *2* can be considerable (on the order of fifty cents to a dollar). Only three players in winning coalitions received less than predicted whereas six received more, thereby adding weight to the argument that the third non-critical player is being "bought" at near-zero cost to his partners.

Finally, we note that in no game was the coalition of the whole seriously considered, and the issue of equity or fairness arose explicitly in the bargaining only once (the idea of an equitable proposal was quickly rejected and never mentioned again). Most subjects recognized the instability of the situation and a few noted after the game that their objective was to form a winning and acceptable coalition as quickly as possible (despite the fact that several games ran for nearly an hour). Hence, in some cases a form of satisficing appears to have affected calculations and strategies. Again, though, several players commented on the necessity for revising their assessment of "acceptable payoffs" as the game progressed.

Clearly, much additional analysis and experimentation is required. The subjects in these experiments are not known for their lack of competitiveness and, doubtless, this improves our results. Further, alternative configurations of preferences and utility functions should be considered. In particular, a configuration with an internal ideal point should provide a more critical test of $\mathcal{K}$ since a Strong Solution does not exist there and since the meaning of internal and external coalitions is less obvious.

Conclusions

The Competitive Solution's motivation is most compatible with the unrestricted bargaining process of our experiments. It seems less well suited to processes that use formal voting procedures or to procedures that admit secret negotiations. $\mathcal{K}$, nevertheless possesses several important advantages over classical concepts. First, in many examples, the proposals by coalitions in $\mathcal{K}$ reduce to the main-simple *V*-set and thereby provide a behavioral rationale for hypothesizing outcomes in *V*. Second, neither the core nor the *V*-set identify the coalitions that form to support payoff vectors in them, while bargaining set theory in general takes the coalition structure as a given and identifies the proposals it can support. $\mathcal{K}$, on the other hand, isolates the coalitions that can compete successfully for members as well as the proposals they offer. And, in one instance where bargaining set theory can be interpreted as making coalition predictions, the experimental evidence conclusively supports $\mathcal{K}$ as opposed to that theory's predictions.

Third, in all spatial examples, the (strong) Competitive Solution corresponds to proposals made only by minimal winning and connected coalitions (i.e., no exluded player lies within the convex hull of a coalition with a proposal in $\mathcal{K}$). And, while we may have missed some possibilities, we are unable to find solutions in these examples that entail coalitions that are other than minimal winning and connected. Hence, these are good reasons for conjecturing that the minimal winning and connected coalitions hypotheses can be deduced from the properties of $\mathcal{K}$.

Finally, the Competitive Solution treats those examples that classical concepts fail to solve. Since these examples correspond to the implicit or explicit conceptualizations others use to model legislative or parliamentary coalition processes, the implication is that $\mathcal{K}$ is the more useful concept.

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Reassessing the Mediating Impact of Legislative Capability

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The model of comparative state policy focusing on economic development and interparty competition was reformulated by Edward Carmines to include the mediating impact of state legislatures. This study challenges his initial conclusions, poses an alternative hypothesis, and finds that interparty competition has a consistently stronger impact on "progressive" state policies among states with organizationally weak, ineffective legislative institutions, than among those with organizationally strong, capable legislatures. The findings also reveal that legislative decision-making capability is distinct from the concept of legislative professionalism.

For well over a decade, scholars of American state politics have debated the relative importance of political and economic determinants of policy. Research innovations have been notably lacking, but a shift in focus was provided by Edward Carmines in an article in the *American Political Science Review* (1974). He suggested a reformulation of the theoretical linkage between interparty competition and state policy (specifically welfare policy) to include the mediating influence of state legislatures. Carmines concluded that interparty conflict has a significantly stronger impact on welfare expenditures "among those states which have organizationally strong, effective legislatures than among those states which have weak legislative systems" (1974, p. 1119).

The theoretical reformulation encompassing the mediating impact of legislative institutions is both appropriate and important. However, the findings presented here strongly suggest the opposite conclusion: interparty competition will be shown to have a consistently stronger relationship to more progressive, activist state policies among states with less developed and less effective legislatures. This brief study presents the theoretical justification for an alternative hypothesis, an empirical test of that hypothesis and the substantive and methodological reasons for the initial conclusion.[1]

V. O. Key (1949, pp. 298–311) is usually credited with suggesting that a more competitive state party system will result in more liberal welfare policies. Subsequent studies arguing, for example, that competitive parties must fight for the support of lower socioeconomic groups, or that groups are better able to perceive their economic interests, produced a full range of conclusions about the impact of party versus economic development.[2] Conflicting findings and methodologies left the question in doubt.

In reformulating the theoretical linkage between interparty competition and welfare policies, Carmines questioned the assumption in the previous formulation that state legislatures were capable of converting party conflict into public policies.

> Since many state legislatures provide only a minimum of services to legislators, meet relatively infrequently, and in general, can be considered organizationally ineffective, it is understandable that even the most salient forms of interparty conflict do not have a firm, consistent impact on state policies. On the other hand, in those states which have organizationally strong, effective legislatures, the political system at least has the potential capacity to translate party conflict into state policies. (Carmines, 1974, p. 1119)

Using Grumm's index of professionalism to dichotomize states (Grumm, 1971, pp. 298–322), Carmines examined zero order correlation, partial correlation, and standardized partial regression coefficients between party competition and welfare expenditures in both groups. Controlling for socioeconomic factors, he reported finding the relationship between party competition and welfare policies consistently stronger among states ranking higher on professionalism: party competition had a strong linkage to welfare policies in "professional" states, while only a weak linkage existed in "less professional" states.

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[1] The author acknowledges a number of limitations in this study. Because of the research-note format, many important theoretical and methodological issues must be ignored. The purpose of this study is to focus on a very narrow question of the differential impact of party competition on state policy between different types of legislatures. While progressive policies are caused by a variety of political, cultural and ideological factors, they are beyond the scope of this study.

[2] Party competition may affect policy results in a variety of ways but this extensive body of literature cannot be reviewed here. Readers are directed to Carmines (1974), Fenton and Chamberlayne (1969), and Jacob and Lipsky (1968) for summaries of some of the research conducted in this area.

An Alternative Hypothesis

An alternative hypothesis concerning the impact of legislative capability is equally plausible. Increased institutional capability per se should facilitate the translation of public needs and demands into more responsive policy. More streamlined structures and processes, greater public access to materials and proceedings, professional staff, independence of other state institutions and increased information-gathering capabilities would hopefully allow not only more expeditious decision making but more activist and progressive policies. In such a context, conflict between the political parties may help in shaping and selecting alternatives but would be part of a more differentiated arena of policy demands and responses. On the other hand, in state legislatures with less streamlined institutional structures, unwieldy processes, less public input, relatively greater dependence on lobbyists and executive branch, and limited information-gathering capabilities, access may be highly restricted to internal party organization. It can be logically argued that in states with reformed, capable legislatures, competitive parties may assist but are not necessary to translate public demands into progressive policies. In states with unreformed, less capable legislative institutions, interparty competition may be a *necessary condition* for more liberal welfare payments, progressive taxes, government interventionism, higher per capita spending, and other state policies.

As an alternative hypothesis to Carmines' proposition and findings, it can be proposed that *the linkage between interparty competition and "progressive" state policies will be stronger among states with less effective, organizationally weak legislative systems than among states with more effective, organizationally strong legislative systems.*

In addition to the a priori theoretical appeal of this alternative hypothesis on the mediating impact of state legislatures, there are several important limitations in the research design of the Carmines study. First is the restriction to state welfare policies. Given his initial positive finding, it is important to determine if the relationships are consistent in a wider range of state policy alternatives. Second, the use of legislative professionalism as an indicator of institutional development and capability is highly problematic and the findings in support of the initial hypothesis can be explained on the basis of this choice.

Data and Methodology

Professionalism versus Institutional Capability. While measurement is crucial in any meaningful social scientific research, the operationalization of key concepts is particularly paramount in this case. The distinction between legislative professionalism and institutional capability is not simply a methodological curiosity; it can structure the analytical results. For this reason, the substantive content of alternative measures must be considered with great care. The theoretical grounds for both the Carmines hypothesis and the alternative hypothesis proposed here revolve around the concept of the *effectiveness and capability of legislative institutions*. While certainly related, this is conceptually distinct from an amateur-professional dimension of legislators. The best available comparative measure of legislative capability is provided by the Citizens Conference on State Legislatures (CCSL) in their report on *State Legislatures: An Evaluation of Their Effectiveness* (1971).[3] The CCSL study resulted in an ordinal scale of overall legislative decision-making capability determined by the aggregation of 73 weighted subcriteria (1971, pp. 9–37).

In justifying the dichotomization of the states on the basis of legislative professionalism, Carmines cites the CCSL capability rankings as evidence of the construct validity of Grumm's index (1974, p. 1120, n. 10). The assumption that the two are measures of the same thing fails to recognize substantive differences in the concepts and the relative independence of the indicators. Legislative capability and professionalism are correlated at only $r_s = .25$ and the correlations between professionalism and the five subscales ranges from a high of $r_s = .39$ to a low of $r_s = .04$. States with "professional" legislatures, as defined by Grumm, are not necessarily those with the most effective or capable legislatures.

Substantively, the index of professionalism is based primarily on money allocated to legislative institutions (legislator's compensation, legislative expenditures and amount of services available). Legislative capability encompasses a broader range of decision-making processes and structures. Using capability and professionalism as dependent variables, multiple regression reveals that economic development is highly related to professionalism. Over 77 percent of the variance in legislative professionalism is explained by state economic development (b = .88); less than 15 percent of the variance in legislative capability is similarly explained (b = .38). "Professionalism" appears to be something that the wealthier states can afford, but many reforms, structures, and procedures that make legislatures more capable are not bound by high monetary costs. Not only does this suggest that professionalism and capability are empirically distinguishable, but for substantive reasons, the CCSL index of legislative capability has advantages for use in an analysis of the mediating impact of state legislatures.[4]

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[3] The Citizens Conference on State Legislature has since changed its name to "Legis 50." A summary of their complete study was published under the title of, *The Sometimes Governments.*

Design of the Study and Operational Indicators. Following Carmines' methodology and using the CCSL index of legislative capability, states are dichotomized into the highest and lowest 25. In this manner, the relative impact of party competition on state policy, controlling for economic factors, can be compared for the two groups of states. The analysis consists of multiple regression and comparisons of the standardized beta coefficients, with checks on the unstandardized coefficients to ensure that no distortion occurs through unequal variance in the two groups. Analysis of the standardized betas serves to specify the relationship and the explanatory power of an independent variable (interparty competition) on the dependent variable while controlling for the effects of other variables in the model (economic development) (Blalock, 1971, pp. 139–51).

In an attempt to test the hypothesis for a wider range of state policies than welfare expenditures, we used a number of policy indicators in the analysis.[5] Table 1 lists the 15 policy indicators and their sources. State "interventionism" groups 3 separate policy indicators: Scope of government (Crittenden, 1967, pp. 989–1001), state innovation (Walker, 1969, pp. 880–89), and the state share of education expenditures. The 3 indicators of state tax systems were selected to reflect progressiveness-regressiveness. Also included are measures of welfare liberalism and per capita expenditures. Interparty competition is measured using the Ranney index where the highest scores represent the most competitive situation (1971, pp. 82–121). Economic development is also measured by a single composite score based on Metro-urbanism, state affluence, and industrialization.[6]

Findings

The results of multiple regression analysis for the 15 policy variables are shown in Table 2. The outcome of the analysis is startling both in consistency and the magnitude of differences between the two groups of states, clearly tending to support the alternative hypothesis. In 14 out of 15 cases, interparty competition has a substantially higher impact among the states ranking low on legislative capability than among states ranking high. The standardized regression coefficients meet the requirements for statistical significance in 12 cases among low-ranking states versus only one case among the high-ranking states. These are the opposite relationships predicted and reported by Carmines. Differences between the two groups of states are particularly large when one examines measures of the maximum rate of income taxes, elementary and secondary pupil expenditures, and per capita expenditures for highways, national resources and police. The mediating influence of legislative capability is consistent across policy groupings; not only is interparty competition associated with welfare liberalism in the less capable states, but with interventionism, tax progressiveness and higher per capita expenditures as well.

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[4] The CCSL data is not without its own limitations and problems. For example, some of the items were used to generate more than one subranking, and certain criteria (like annual sessions) may be overweighted. In balance, however, their study represents the most comprehensive comparative study of legislative capability, including a large number of respondents (legislators, staff) as well as direct observation. Although there are some weaknesses in the data, this largely untapped source provides an opportunity to expand upon and compare with existing measures such as professionalism.

[5] The expanded list of dependent variables raises several questions that should be briefly discussed. Because of the interaction of both federal and local governments with the states, there is inevitably some level of analysis blurring. A second question concerns the primacy of state legislatures (as opposed to the executive branch) in making these policy decisions. Both are problems endemic to comparative state policy research that affect the more familiar welfare expenditure measures as well as the additional variables. Recognizing the impact both of other institutions and levels of government on some of these policy measures, the expanded list offers an opportunity for a broader test of this hypothesis across of range of actions where state legislatures play a crucial, if not exclusive role.

[6] The index of economic development combines measures of metro-urbanism, affluence, and industrialization. The construction and scores for the index are found in the CCSL Report (1971, pp. 82–83).

The results in Table 2 do not suggest that party competition has no impact on welfare liberalism or other policy actions among the states with more capable legislatures. It can be seen that the employer contribution to unemployment (b = .28), average Aid to Dependent Children payment (b = .29) and per capita spending for employment security (b = .35) are somewhat related to party competition among the higher group. In each case, however, the relationship is substantially higher among states ranked low on legislative capability, increasing to b = .58 for ADC payments and b = .65 for employment security spending, and statistically significant at the .01 level. Given the limitations in the amateur-professional dichotomy as an

Table 1. State Policy Indicators, Party Competition, and Economic Development

Policy Indicators

1. State Interventionism
   - a. *Scope of Government Score* (Crittenden)–highest loadings, per capita state and local taxes, government employment and spending
   - b. *Innovation Index* (Walker)–reflects the relative speed that states have adopted 88 programs in a variety of policy areas
   - c. *State Share of Education, 1971*–the percentage of state funds in total education spending in a state *(Book of the States, 1972)*
2. Tax Progressiveness
   - a. *Maximum Rate of State Personal Income Tax, 1971* (Book of the States, 1972)
   - b. *Percentage of State Revenue Derived from Personal Income Tax, 1971* (computed on basis of figures in Book of the States, 1972)
   - c. *Rate of Employer Contribution to Unemployment Compensation, 1971* (Book of the States, 1972)
3. Welfare Liberalism (all data taken from Book of the States, 1972)
   - a. Average Aid to Dependent Children monthly payment, 1971
   - b. Per capita education expenditures, 1971
   - c. Per capita welfare expenditures, 1971
   - d. Per pupil elementary and secondary education expenditures, 1971
   - e. Average Old Age Assistance payment, 1971
4. Per Capita Expenditures (all data taken from Book of the States, 1972)
   - a. Per capita highway expenditures, 1971
   - b. Per capita natural resource expenditures, 1971
   - c. Per capita police expenditures, 1971
   - d. Per capita employment security expenditures, 1971

Explanatory Variables

1. *Party Competition* (Ranney Index)
   - a. Average Democratic vote for governor
   - b. Average percentage legislative seats held by Democrats
   - c. Average period of Democratic control of governorship and legislature (values greater than .5 subtracted from 1.0 so highest score reflects most competitive situation)
2. *Economic Development* (CCSL): Composite index based on
   - a. Metro-urbanism
   - b. Affluence
   - c. Industrialization

independent measure, and these findings, there are persuasive reasons for questioning earlier conclusions about the mediating impact of legislative capability.

Summary and Conclusions

While this study does not provide a final answer to the question, the results indicate that the linkage between interparty competition and progressive, activist state policies is clearly stronger among states with weaker and less effective legislative institutions. Far from being an anomaly to be explained, this finding makes a great deal of sense. In less-developed legislatures, party competition is a necessary ingredient to translate public demands into policy. A "professional" legislature is not necessarily a "capable" legislature, nor is an amateur legislature necessarily less capable. But measures of institutional capability are the weakest link in understanding the processes at work. While the CCSL study may be preferable to Grumm's index, it is somewhat problematic and static. Measures of institutional change over time are necessary for any major breakthroughs in understanding the mediating impact of legislatures.

Yet the results here are suggestive. They reaffirm an important role both for the competition between political parties and the importance of state institutions in understanding policy differences. While reforms increasing the effectiveness of legislatures may make party conflict less requisite for more progressive policies in these states, it appears to be an important catalyst in the many state legislatures that remain institutionally backward and relatively ineffective.

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Table 2. Mediating Effect of Legislative Capability: The Impact of Interparty Competition on State Policy

	Interparty Competition	
Policy Indicators	States High on Legislative Capability	States Low on Legislative Capability
State Interventionism		
Scope of government score	.05	.30*
Innovation score	.00	.22
State share of education expenditures	.08	–.61**
Tax Progressiveness		
Maximum rate income tax	.09	.53**
Percent state revenue from personal income tax	.12	.43**
Employer contribution unemployment compensation	.28	.40**
Welfare Liberalism		
Average ADC payment	.29	.58**
Per capita education expenditure	.12	.31*
Per capita welfare expenditure	.03	.18
Elementary and secondary pupil expenditure	.15	.53**
Average Old Age Assistance payment	.09	.18
Per Capita Expenditures		
Highways	.12	.64**
Natural resources	.04	.68**
Police	.09	.52**
Employment security	.35*	.65**

Standardized beta coefficients, multiple regression, controlling for economic development.

*Significant at .05 level.

**Significant at .01 level.

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COMMUNICATIONS

1976 Update

TO THE EDITOR:

Analyses of the contributions of particular groups to a party's electoral coalition have long been a mainstay of both scholarship and strategy. In recent years, this kind of calculation has also become a vehicle for the assertion of claims of political merit. Black leaders who remind Carter of the contribution of black voters to his victory are the latest of these claimants.

Having published an analysis of electoral coalitions for each presidential election from 1952 to 1972,[1] I am pleased to be able to provide an update for the 1976 election. Few people are in a position to generate the required statistics from survey data readily because an iterative procedure is necessary to correct for overreporting of the turnout.[2]

The figures are presented with the data from the earlier studies so that all seven presidential elections since 1952 can be compared. The coalitions are described in terms of the three components of a group's contribution to a party, namely the size of the group, its turnout, and its loyalty to the given party. The *contribution* of a group to the electoral coalition of a party is the proportion of all the party's votes that come from members of that group. The *loyalty* of a group to a certain party is simply the proportion of the votes of that group which are cast for that party.

Two well-known facts about the 1976 election are worth elaborating before we look at the coalitions themselves. The first is that the election was close. Only 2.1 percent of the popular vote separated Carter from Ford.[3] This is quite a contrast from only four years before when Nixon's lead over McGovern was 23.2 percent. The second basic fact is that the turnout was very low: only 54 percent of the voting-age population voted, the lowest turnout in the seven elections analyzed. Especially significant in this regard is that only 41 percent of those under thirty voted.

For the Democrats, the New Deal coalition made a comeback in 1976. For the first time since the Johnson landslide of 1964, the Democrats got a majority of the votes from each of the six diverse minorities which make up their traditional coalition: the poor,[4] blacks, union families, Catholics,[5] southerners, and city dwellers (Table 1, columns 19–24). The blacks have continued their pattern of very high loyalty, in this year giving 88 percent of their vote to the Democrats (col. 20), but with a turnout of only 44 percent (col. 14). Thus with 11 percent of the population (col. 8), they contributed 16 percent of Carter's vote (col. 2). Union families and Catholics (and other non-Protestants) provide an interesting contrast to the blacks. Both of these groups provided more than twice as many votes to the Democrats as did the blacks. They voted 13 and 7 percent respectively more Democratic than the nation as a whole (col. 29 and 30), they had comparatively good turnout (col. 15 and 16), and most importantly they were more than twice as numerous as the blacks to begin with (col. 9 and 10). Finally, there is the South, including the border states. Surprisingly, the South gave only 53 percent of its votes to Carter (col. 23). This can be understood as the combination of the trend since 1960 whereby the South is moving away from its loyalty to the Democrats (col. 31), and the

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[1] "Where the Votes Come From: An Analysis of Electoral Coalitions, 1952–1968," *American Political Science Review*, 66 (March 1972), 11–20, and "Communication," *American Political Science Review*, 68 (June 1974), 717–20. All definitions and procedures used here are as described in the original article. The survey material is again taken from the Survey Research Center of the University of Michigan, and this time has a sample size of 2248 (or 2868.5 weighted). I gratefully acknowledge the Institute of Public Policy Studies of the University of Michigan for financial support of this study, and the Inter-University Consortium for Political and Social Research for making the survey data available.

[2] In the 1976 survey there was no overreporting of the winner's vote, so the only correction that was necessary was for turnout.

[3] In fact, if only 55,000 of Carter's votes had switched to another candidate, Carter would have been elected by a minority of the popular vote.

[4] The proportion of the people in families earning less than $3000 per year has continued to shrink, from 12 percent in 1972 to 9 percent in 1976 (Table 1, col. 7). This was largely due to inflation, as seen by the change in the Consumer's Price Index (using 1957–59 = 100) which was 145.7 in 1972 and 198.3 in 1976.

[5] As before, the Catholic category includes all those with religious preferences other than Protestant. Tables 1 and 2 reflect a correction in the statistics for the 1972 religious categories.

Table 1. The Democratic Coalition, 1952–1976

	Percentage Contribution						= (Size						× Turnout						× Loyalty)						+ (NTxNL)	
Year	P	B	U	C	S	CC	P	B	U	C	S	CC	P	B	U	C	S	CC	P	B	U	C	S	CC	NT	NL
1952	28	7	38	41	20	21	36	10	27	26	28	16	46	23	66	76	35	68	47	83	59	57	55	51	63	44.4
1956	19	5	36	38	23	19	25	9	26	25	29	14	40	23	64	72	39	63	47	68	55	53	52	55	60	42.0
1960	16	7	31	47	27	19	23	10	25	25	23	13	46	31	60	74	50	74	48	72	66	82	52	65	64	49.7
1964	15	12	32	36	21	15	19	11	23	26	28	12	45	42	69	72	49	65	69	99	80	75	58	74	63	61.1
1968	12	19	28	40	24	14	16	11	24	26	31	10	44	51	61	68	53	63	44	92	51	61	39	58	62	42.7
1972	10	22	32	43	25	14	12	11	25	31	34	8	37	47	58	65	44	60	45	86	45	45	36	61	56	37.5
1976	7	16	33	35	36	11	9	11	23	30	32	8	32	44	62	55	57	58	67	88	63	57	53	61	54	50.1
Column	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26

P poor (income under $3,000/yr.)
B Black (and other nonwhite)
U union member (or union member in family)
C Catholic (and other non-Protestant)
S South (including border states)
CC central cities (of 12 largest metropolitan areas)
NT National turnout
NL National loyalty to Democrats

Year	Percentage Deviation in Loyalty to Democrats					
1952	+2	+38	+14	+12	+10	+ 6
1956	+5	+26	+13	+11	+10	+13
1960	−2	+22	+16	+32	+ 2	+15
1964	+8	+38	+19	+14	− 3	+13
1968	+1	+49	+ 8	+18	− 4	+15
1972	+8	+49	+ 8	+ 8	− 2	+24
1976	+17	+38	+13	+ 7	+ 3	+10
Column	27	28	29	30	31	32

immediate appeal of a Democratic candidate from the South.

The Republicans have experienced a dramatic comedown since 1972, when they attracted 60 percent or more among all six of their traditional coalition groups: the nonpoor, whites, nonunion families, Protestants, northerners, and those living outside the central cities (Table 2, col. 19–24). In 1976 they got no more than 53 percent from any of these groups, and failed to get even a majority of the loyalty of the nonpoor, northerners, and noncity dwellers (col. 19, 23 and 24).

One lesson from the 1976 election is that even when the Democrats can put together all of the elements of their traditional coalition, the election can be very close. The experience of the Democrats after running a Catholic candidate in 1960 shows that the gains in Catholic deviation in loyalty may not be lasting (Table 1, col. 30). The same may apply in the South after Carter. If the Democrats lose the South again, or if they lose the enthusiasm of the blacks or the loyalty of any of their other traditional groups, they will be in trouble. This could easily happen despite their large and steady lead in party identification.[6]

ROBERT AXELROD

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Making Distinctions in Theory

TO THE EDITOR:

I read with very great interest the exchange between Partha Chatterjee on one hand and Patrick McGowan and Robert Rood on the other concerning the latters' use of Poisson processes to investigate "balance of power" behavior. I was very favorably impressed by the original article by McGowan and Rood and by their reply to the questions raised by Chatterjee. However, some of their comments that are directly related to my work require, I believe, brief elucidation.

First, let me state that I agree that the theory in *System and Process* is not a theory in the strict sense used by those in the physical sciences. As McGowan and Rood know, the last sentence of the paragraph in the preface in which I define theory in the more rigorous sense disclaims the use of the strict deductive model.

With that out of the way, let me distinguish the question of logical levels from that of deductive relationship. This involves Arthur Burns' distinction—which Chatterjee, McGowan, and Rood accept—between rationality assumptions and descriptive statements. Although I regard Burns as the premier theorist in international relations, I do believe that he is not entirely correct in this case. Take the proposition "two plus two equals four." In that form, it is an analytical statement. "John adds two apples to two apples and has four apples" is a descriptive statement. "If John has two apples and wishes to have four apples, he should take two more apples" is a prescriptive statement. Simple analysis will show that every one of the essential rules in every one of the models in *System and Process* can be expressed in each of these three forms. There are three sets of equilibria for each of the "theories": (1) among the essential rules; (2) between the essential rules and the other system elements; (3) between the system and its environment. Each theory, or more properly, each theory sketch, is a statement of equilibrium conditions as they apply to all three categories. If the latter two are taken as givens, then the essential rules stand for prescriptions to the actors. If all the conditions are presumed to hold, then the theories "predict" certain descriptive features of the real world.

Arthur Burns' assertion that some of the rules can be derived from other rules, which he calls the rationality assumptions, is a far more considerable criticism. It would not surprise me if some criticism of this type is valid, for among other things I called my theory at that time "introductory" and hoped that further scholarly criticism and historical research would lead to its improvement. However, in the particular case, Burns is wrong. For instance, Burns says that rules 3, 5, and 6 describe the state of equilibrium by prohibiting a change in the number of actors and that this is what equilibrium is supposed to preserve. And he claims that this can be derived from rules 1, 2, and 4, which are sufficient to produce equilibrium. However, take a simple counterexample. Three actors defeat two actors and propose to divide the assets of the defeated among themselves. This is thoroughly consistent with rules 1 and 2 which call for an increase in capabilities. It is not inconsistent with rule 4 which says only that the actors should combine against an actor that is striving for predominance. In the counterexample, therefore, rule 4 does not apply, for all act in concert. Thus, in this case, in the absence of rule 3, such behavior will soon throw the system out of equilibrium. Counterexamples can easily be found for rules 5 and 6.

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[6] The 1976 party identification figures are 40 percent Democrat (the same as 1972), 23 percent Republican (down two percent since 1972), and 29 percent independent (up one percent). The debate about the relationship between party identification and vote is an active one. See for example Arthur H. Miller et al., "A Majority Party in Disarray: Policy Polarization in the 1972 Election," *American Political Science Review*, 70 (September 1976), 753–78, and the following comments and rejoinder.

Depending upon circumstances and examples, some of the rules may play more important roles than others in the derivation of conclusions. And I certainly agree with Burns that this is the process by means of which we derive partial explanations from the theory sketches in particular cases. Thus the theory can be and has been used to show that the deviant behavior of the European system after 1870—including rigid alliances and "total" war—is consistent with actual changes in parameter values. The peculiar configuration of the alliance process in the Italian city-state system can be related to the peculiar geographic conditions of that system. On the other hand, during certain periods of the "balance of power" system in ancient Greece, behavior that superficially conforms with predictions can be shown to be unrelated to theoretical system properties and contingent primarily upon logistic and other particular considerations. Thus the theory sketches do function in at least partial explanations that are falsifiable. Burns and McGowan contribute a sophisticated method of partial confirmation.

One further word may be in order. In fact, very little can be derived in any simple way from the essential rules. To argue that states should increase capabilities does not say very much about how much they should do so or what risks they should take in doing so. To find out how sensitive the system was to changes and variations in conditions of this kind, at one time we did have a computer project on this subject going at the university. We learned a number of things, including some of the conditions under which alliances would necessarily be four against one rather than three against two, as Riker's theory of minimum winning coalitions asserts.

I am in complete agreement with Rood and McGowan about the inadequacy of attempts to construct "general" theories in international relations. Indeed, much of my recent work in the philosophy of science is designed to show how and why such endeavors in the social sciences will necessarily fail. Even in game theory, there is a general theory only for the zero sum game which, with its peculiar utility conditions, comes closest to resembling the independently measurable general values of physical units. It is not at all surprising that attempts to cope with the subject matter of international politics in a fully general and deductive fashion, as McGowan and Rood point out, lack relevant texture.

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Applying Jöreskog

TO THE EDITOR:

In their June, 1977 article, "The Stability and Reliability of Political Efficacy" (*American Political Science Review*, 71:509–21), McPherson, Welch and Clark introduce a tool for evaluating measurement error that has been too long overlooked by political scientists. Karl Jöreskog's analysis of covariance procedures permits the estimation of complex structural equation models which include multiple indicators of unmeasured variables. Without this procedure, estimation of all but the simplest causal models of measurement is quite problematic. Multiple parameter estimates from invariably overidentified systems of simultaneous path equations are difficult to obtain, and impossible to reconcile in a statistically desirable manner. What is more, Jöreskog's maximum likelihood estimation routine provides a chi-square statistic for gauging overall goodness of fit, and for comparing alternative model specifications. This facilitates statistically sound, sequential model fitting and permits thorough testing of error assumptions—heretofore impossible tasks. For these reasons the authors' illustration of the technique is a noteworthy contribution to the literature on causal modeling, and a particularly important example for political scientists concerned with the languishing field of measurement theory.

Unfortunately, as the article now stands it also is *not* an example that should be followed closely by future users. Major errors flaw its use of causal modeling, goodness of fit tests, and ultimately its conclusions as well. The most serious errors are:

1. *Confusion of epistemic correlations and reliability coefficients.* Beginning on page 514 and continuing through the concluding discussion, the authors erroneously refer to epistemic correlations are reliabilities, and thereby far overstate the reliabilities of the efficacy indicators. The reliability of an indicator is given by the ratio of the true score variance to indicator variance, or, to phrase it in terms of

the causal model, by the proportion of indicator variance "explained" by the true score (Lord and Novick, 1968, p. 199; Wheaton, 1977, p. 108). Hence the reliability of an indicator is given by the *square* of the epistemic correlation in the measurement model. Although this correction does not alter the relative reliabilities of the efficacy indicators, it does substantially reduce the reliabilities of these indicators relative to typical standards for reliability coefficients. For example, the reliabilities of NO SAY and NO CARE in the authors' final model (Figure 5) actually average only .40 at the two points in time. In other words, 60 percent of the measured variance in these indicators is attributable to measurement error while only 40 percent is "explained by" the true efficacy score. By no standards are these "highly reliable indicators" (p. 520), as the authors conclude.

2. *Misuse of the chi-square statistic.* In their search for the "best measure of political efficacy, the authors follow a haphazard course guided largely by a priori knowledge. Their use of the chi-square statistic is unsystematic and frequently misleading. One obvious case of this occurs in their adjudication of alternative autocorrelation specifications for the four-indicator model. To support their preference of the model with two autocorrelation paths (Figure 4a), they state: "A comparison of chi-square values of the two models shows that this constraint (i.e., 2 additional autocorrelations) increases the chi-square fit only 14.6 units, which is quite a small difference for a sample of nearly 1000."

While the authors are correct in stating that sample size does have a proportional bearing on the chi-square statistics used to assess overall fit, it is also the case that the difference in chi-square values between two (nested) models is distributed as chi-square with degrees of freedom equal to the difference in degrees of freedom between the two models. And though this is also affected to some extent by sample size, at some point a standard is needed by which one can choose between models. In this case, the difference in chi-square is 14.6 with 2 degrees of freedom which is significant at beyond the .001 level, thus indicating that the autocorrelations of NO SAY and NO CARE (jointly) provide a significant improvement in fit over the model with only two indicators correlated over time. On the basis of this joint test it appears that none of the indicators can be considered completely free of autocorrelated disturbances.

Similar misuse of the chi-square test is evident throughout the analysis. While the authors correctly caution in the footnotes that chi-square statistics from models with different degrees of freedom cannot be directly compared, they frequently do so in the text. As a result the choice of competing models seems to be largely determined by the authors' personal preferences. One method that can be used in this situation is to divide the chi-square by degrees of freedom and compare this ratio across models (Wheaton, 1977, p. 99). On the basis of this, McPherson et al. come closest to having a properly specified model with the four-indicator version that includes four autocorrelations paths. Although there is no test for within-wave biases, this four-indicator version is superior to the two-indicator model preferred by the authors. The ratio of chi-square to degrees of freedom is far lower in the four-indicator than in the two-indicator model: 40.7/15 versus 18./1.

Since the absence of autocorrelation was a major criterion for the selection of a "best" measurement model, must we conclude that none of these indicators can provide a reliable measure of the stability of efficacy? Emphatically not. The purpose of employing multiple indicators in a causal measurement model is to estimate the hypothetical relationships between concepts and indicators, and to explicate the assumptions of measurement. When sufficient information (i.e., overidentification) is available, these assumptions can be tested. And when assumptions (such as random errors) are found to be wrong, the model can be respecified. Such tests can be accomplished with the ACOVS routine, either by testing new specifications one at a time with chi-square comparisons, or by checking the standard errors of possible non-random error paths. Thus, working with the Jöreskog framework, we draw the conclusion from this analysis that all four indicators in a *correctly specified* measurement model should be retained.

But what conclusions should be drawn for those who are not able, or do not wish to utilize Jöreskog's technique? The authors suggest dropping two of the items and proceeding to work with the resulting two-item scale. This is exactly wrong for two reasons. First, the two-item scale is *less* reliable than the original four-item scale. Calculating the reliabilities from the criterion of internal consistency (Nunnally, 1967, p. 193) shows the reliability of the original scale to be .68 (which agrees well with the results of the four-indicator measurement model) while the corresponding figure for the two item "scale" is .57. Thus, if the original efficacy scale can be criticized for being unreliable, the two items praised by the authors are even more so. And second, simply finding autocorrelated disturbances for an indicator is not a sufficient basis for eliminating it. While this clearly does affect the stability of the item and scale *over time,* nothing in the reported analysis has shown that it will affect the relationships of political efficacy with other constructs. It is very possible that the source of autocorrelation in these items is unique or idiosyncratic. On the basis of the analysis presented in this article, therefore, there is no reason to believe that these sources of variation in the indicators that are independent of efficacy will correlate systematically with any other constructs of interest.

For those working in this area, then, three alternative suggestions may be offered: (1) discare this scale entirely and look for better indicators of efficacy; (2) increase the reliability of the present scale by developing and testing additional items; or, for those who have no choice but to work with data already collected, (3) continue to use all four items in a summated scale and make the appropriate corrections for unreliability. The *worst* alternative would be to use only the two items as concluded by the authors.

As we said at the outset, the authors' attention to measurement error and their application of Jöreskog's estimation routine are noteworthy examples for political scientists; however, they are *not* examples that should be followed too closely in further applications of these techniques.

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JOHN E. CHUBB

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STANLEY FELDMAN

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TO THE EDITOR:

It is pleasant to have one's work read closely enough and taken seriously enough to be criticized. Therefore, we are pleased to respond to the letter from John Chubb and Stanley Feldman. We will take each point in order.

We plead guilty to using epistemic coefficients as estimates of reliability rather than the squared form of this coefficient. We did this explicitly (see n. 12 and paragraphs 3 and 4, p. 511) because the logic of path analysis provides a very clear interpretation of path coefficients relating indicators and constructs, while the "proportion of variance explained" approach is primarily useful in dividing the observed variance in an item into meaningful components, as we do in the first paragraph of p. 518.

We are rather surprised to be criticized for allowing a priori knowledge to guide our hypothesis testing. Most of the scientific literature we are aware of encourages the use of prior research; indeed, it is difficult to conceive of a science which is not guided by a priori knowledge.

As for our "misuse" of the chi-square test, many sources suggest interpreting this statistic with caution, as we do, for several good reasons. In fact, even the source cited by Chubb and Feldman cautions against interpreting too literally the significance of chi-square in large samples (Wheaton et al., p. 99). Chubb and Feldman propose the use of an alternative method to compare models as if this method possessed more statistical rigor than our procedure. However, the originators of this alternative method use such terms as "descriptive," "rough indication of fit," and "beginning to be reasonable" in describing their method (Wheaton et al., p. 99). We suggest that our ad hoc procedure is fully as justifiable statistically as any other, in the absence of convincing mathematical justification for any of them. It is also useful to point out that we devoted three footnotes and considerable space in the text discussing limitations and implications of our use of chi-square, while Chubb and Feldman fail to even hint at the ad hoc nature of their procedure.

At the core of the issue, however, is whether or not the VOTING and COMPLEX items should be combined with the NO SAY and NO CARE ITEMS to measure efficacy. Our analysis suggests that most of the relationship between VOTING (1956) and VOTING (1960) is due to some source external to the efficacy construct. In fact, using the model Chubb and Feldman believe is best (Model 4-a), we see that the amount of variance in VOTING (1960) attributable to VOTING (1956) *through the efficacy construct is less than one percent.* Indeed, according to this model, even if efficacy were perfectly stable (e = 1.0), this variance would still be less than one percent. Clearly, an item which correlates with itself over time primarily for reasons unrelated to the construct of interest is practically worthless as a measure of that construct. We make a similar argument for the COMPLEX item on p. 518. Thus, we conclude that the VOTING and COMPLEX items do not belong with the NO SAY and NO CARE items on the basis of a detailed examination of the causal structure generating variance in these items.[1]

Chubb and Feldman assert finally that the reliability of the original scale is higher than that of the two-item scale. This assertion is questionable for several reasons. First, the measure of reliability they use ignores precisely the kind of information generated by a detailed structural analysis made possible by the Jöreskog procedures. Secondly, their measure is very imprecise when there are few items involved (Nunnally, p. 189); this imprecision is a maximum for a two-item scale. Thirdly, this coefficient is ordinarily derived from the assumption of parallel items (Nunnally, p. 196; Lord and Novick, Ch. 9) which is completely unrealistic for these data: the assumption of parallel items was tested using the Jöreskog procedures (Jöreskog and Sorbom, 1976) and was rejected by a chi-square value in excess of 500 with 12 df.[2] Even the strongest of "personal preferences" should find it difficult to ignore this result.

In conclusion, we point out that it is a measurement truism that indices with more items will be more reliable, *ceteris paribus*. We believe that we show strong evidence that the VOTING and COMPLEX items are very poor items which are only minimally related to the construct represented by VOTING and COMPLEX. Even if adding these items to the scale were to increase the reliability of the scale (a point certainly not demonstrated by Chubb and Feldman), we believe that the scale itself would be contaminated by the systematic sources of error we uncovered in our analysis, to the detriment of a unidimensional measure of efficacy. The fact that *ceteris* is not *paribus* in this case in no way suggests that researchers should not continue the search for more and better items to measure efficacy. We believe that the capabilities of statistical analysis such as the Jöreskog procedures will make such a search more fruitful in the future.

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Use of Effects Coefficients

TO THE EDITOR:

In his assessment of the relative importance of socioeconomic and political variables for determining levels of welfare-education expenditures, Michael Lewis-Beck states that past conclusions "are generally distorted because of the inadequacy of the zero-order correlation, partial correlation, or multiple regression techniques."[1] Furthermore, he argues that "interpretations based on coefficients from simple correlation, partial correlation, or multiple regression are simply wrong."[2] Effects coefficients are claimed to be "superior" to these other coefficients since a comparison of effects permits a "more accurate evaluation of the relative importance of X_1 and Y_2 for Y_3 than does comparison of coefficients from the other statistical techniques considered."[3]

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[1] Our discussion bears strong kinship to a distinction bade by Heise and Bohrnstedt (1970) between validity and invalidity. In our paper we chose not to raise the issue of validity explicitly because of space limitations.

[2] Parallel items are items which bear identical relationships to a construct (i.e., the unstandardized regression coefficients between indicator and construct are equal, and which have identical error variances). A method of testing this hypothesis is outlined in Jöreskog (1971).

[1] Michael S. Lewis-Beck, "The Relative Importance of Socioeconomic and Political Variables for Public Policy," *American Political Science Review*, 71 (June, 1977), p. 566.

[2] Lewis-Beck, p. 565.

I found it interesting to observe, therefore, that for the particular causal model under consideration the effects coefficients, E_{31} and E_{32}, (where $E_{31} = p_{31} + p_{32}p_{21}$ and $E_{32} = p_{32}$), are respectively no more than a simple correlation coefficient (r_{13}) and a standardized multiple regression coefficient ($B_{32.1}$). Following procedures used by Duncan,[4] and Blalock,[5] it can be shown that E_{31} does, in fact, equal r_{13}. Using the simultaneous linear equations[6]

$$Y_2 = p_{21}X_1 + u_2 \qquad (1)$$

$$\text{and } Y_3 = p_{31}X_1 + p_{32}Y_2 + u_3 \qquad (2)$$

with the variables in standard form, the relationship between X_1 and Y_3 can be symbolized by:

$$r_{13} = \Sigma X_1 Y_3/N \qquad (3)$$

$$= \frac{1}{N}\Sigma X_1(p_{31}X_1 + p_{32}Y_2 + u_3) \qquad (4)$$

$$= p_{31} + r_{12}p_{32} \qquad (5)$$

Equation (5) is obtained by substituting equation (2) for Y_3 in equation (3), noticing that $\Sigma X_1 X_1/N = 1$ and assuming that the disturbance term u_3 is uncorrelated with its source variable. Following this same procedure for r_{12} yields:

$$r_{12} = \frac{\Sigma X_1 Y_2}{N} \qquad (6)$$

$$= \frac{1}{N}\Sigma X_1(p_{21}X_1 + u_2) \qquad (7)$$

$$= p_{21} \qquad (8)$$

Substituting p_{21} for r_{12} in equation (5) results in the effects coefficient E_{31}, where $E_{31} = p_{31} + p_{32}p_{21} = r_{13}$. Since $r_{13} = b_{31}(S_3/S_1) = B_{31}$ and $E_{32} = B_{32.1}$, the comparison of E_{31} and E_{32} reduces to a comparison of a simple regression coefficient and a multiple regression coefficient, both being standardized coefficients.

Consequently, the effects coefficients are superior in this example only if one is uncomfortable in comparing a simple regression coefficient with a multiple regression coefficient. Rather than mathematically introducing any new information, the effects coefficients here serve only a nominal purpose, incorporating both simple and multiple regression coefficients into a single concept. These effects coefficients should accordingly be viewed as a classificatory device, not a statistical innovation.

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[3] Lewis-Beck, p. 565.

[4] Otis D. Duncan, "Path Analysis: Sociological Examples," in *Causal Models in the Social Sciences*, H. M. Blalock, ed. (Chicago: Aldine, 1971), p. 121.

[5] N. Krishnan Namboodiri, Lewis F. Carter, and Hubert M. Blalock Jr., *Applied Multivariate Analysis and Experimental Designs* (New York: McGraw Hill, 1975), pp. 462–68.

[6] Lewis-Beck, p. 564.

TO THE EDITOR:

Public policy researchers frequently evaluate the relative influence of independent variables on a dependent variable by comparing, respectively, simple correlations, partial correlations, or multiple regression coefficients. Such comparisons are generally inaccurate, implying models of the policy world that the analyst would immediately reject. To illustrate this point, I adopted a simple, but popular, three-variable recursive model of welfare policy.[1] I demonstrated that assessing the effects of X_1 and Y_2 on Y_3 through comparison of r_{13} to r_{23} (or $r_{31.2}$ to $r_{32.1}$, or $\beta_{31.2}$ to $\beta_{32.1}$) seriously downgraded the total impact of X_1. However, the effects coefficients (E_{31} and E_{32}), by incorporating the indirect effects operating in the model, allowed the greater influence of X_1 to be taken into account.

One general device for calculating the effects coefficient, E_{ki}, is the basic theorem of path analysis, which would show $E_{32} = \beta_{32.1}$, and $E_{31} = r_{13}$ (which Jack Wright derives for us here). In this particular model, then, E_{31} equals a simple correlation, and E_{32} equals a multiple regression coefficient. However, an effects coefficient will *not* necessarily equal some correlation or regression coefficient among the variables. In the case at hand, $E_{31} = r_{13}$ *only* because X_1 is an exogenous variable in a just-identified recursive model. If the model were respecified to render X_1 endogenous, or to make the system overidentified, then $E_{31} \neq r_{13}$. (As I noted, these and other issues concerning the effects coefficient are thoroughly discussed in an earlier article by Lawrence B. Mohr and myself, which Wright fails to cite.)[2]

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[1] Michael S. Lewis-Beck, "The Relative Importance of Socioeconomic and Political Variables for Public Policy," *American Political Science Review*, 71 (June 1977), 563–66.

Wright concludes that effects coefficients are superfluous, and that we arrive at the same place by comparison of a simple regression coefficient, r_{13}, with a multiple regression coefficient, $\beta_{32.1}$. But this is a highly arbitrary procedure. What is to stop us from comparing r_{23} with $\beta_{31.2}$, thus reaching an opposite conclusion about the relative impact of X_1 and Y_2? In contrast, comparison of E_{31} and E_{32} is not arbitrary. Rather, effects coefficients must be systematically derived from a properly specified and estimated causal model. Such a model is the essential beginning for any sensible investigation of the relative importance of independent variables.

MICHAEL S. LEWIS-BECK

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Political Money

TO THE EDITOR:

For almost two decades, Herbert Alexander has been the most prolific writer on political finance in the United States. He has also been the stoutest advocate of full disclosure of campaign funds and of tax credits for small political contributions. Until it recently became inevitable, Alexander was a persistent doubter about government financing of campaigns. Now he favors—albeit with some reservations—government matching of private campaign gifts.

In reviewing *Political Money*, by George Agree and me (David Adamany), Alexander quarrels, not surprisingly, with our preference for a voucher system of public financing, our rejection of tax credits, and our belief that financial disclosure, while useful, has been vastly oversold.

Alexander's argument that the voucher system is administratively awkward and expensive is plausible. Agree and I concede that point in our book. But we believe those disadvantages are offset because vouchers permit every citizen equal weight in financing campaigns and because they assure that all candidates will be eligible for consideration. These advantages of equality and fairness escape Alexander's review.

Agree and I discover that tax credits and deductions proffered by Oregon, California, and the federal government are used by so slight a number of persons that they do not significantly enhance participation, if they enhance it at all. Moreover, use of these tax advantages is grossly disproportionate among the highest income groups. A specially commissioned national opinion poll strongly suggests that full public knowledge of tax incentives would not significantly increase participation or decrease the income class bias of users. Alexander finds our conclusions "excessively harsh" because we have too little data. As far as I know, neither Alexander nor other proponents of tax incentives have offered any evidence whatsoever of the effects of such legislation. Apparently there is a very high standard of proof for doubters and no requirement of evidence for proponents.

Agree and I contend in *Political Money* that disclosure is useful as an interelection check on elected officials and that it contributes important data for policy making about political finance. But we find less convincing the argument of its sponsors that it will cleanse politics by allowing voters to punish at the polls those who receive large or special interest or otherwise tainted gifts. It is difficult to sift the mass of disclosure data for such information in the short period between filing dates and election day. Financially weak challengers will not have the funds to exploit the "money issue" even if the data are available. The media are likely—as they did in 1972—to carry or bury the campaign finance issue according to their own candidate preferences. And, as our national opinion survey shows, selective perception and party identification are likely greatly to weaken the impact of disclosed information on those voters who do possess information. Alexander finds these judgments "unfair" and "too quick." Yet, despite numerous state and federal experiments with disclosure, its proponents have brought forward no evidence to show how and whether it will have the specific effects they promise for it. Again, the standard of evidence used by Alexander seems colored by his own well-known preferences.

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[2] Lewis-Beck, "Relative Importance," pp. 560, 564; and, Michael S. Lewis-Beck and Lawrence B. Mohr, "Evaluating Effects of Independent Variables," *Political Methodology*, 3 (February 1976), 27–47.

How It Was Done

TO THE EDITOR:

Like most authors, I am most grateful for any kind words proffered by a reviewer about one of my publications. Because Franz Von Sauer (*American Political Science Review*, 71 [September 1977], 1259–60) was quite generous with his praise of my book *Protest and Response in Mexico* (MIT Press, 1974) I am a little embarrassed to point out that his severest criticism, relative to my methodology, is based on the mistaken assumption that the *best*—if not the *only*—way to conduct political research is to use standardized questionnaires whose answers can be readily quantified, computerized, and reduced to "systematic results." From close acquaintance and long experience, I am convinced that the realities of Latin American political behavior are not always accessible to orthodox survey research methods, and I refuse to limit the scope of my inquiries to those subjects that can be handled adequately by such methods.

In response to Von Sauer's question, "Can Dr. Stevens attain her avowed goal . . . without having been there in the midst of things?" I must confess that I was not always an eyewitness for all the events that I describe. I was, however, a participant-observer for all but six weeks of the ten-month doctors' strikes, and a horrified spectator at the initial events of the students' strike. For the rest, I have had to rely on standard methods of historical research, supplemented by first-hand accounts of friends who were actually under fire. I am happy that, "despite these serious problems" of methodology, I have been able to convey to Sauer and, I hope, to other readers, a sense of the significance of present-day Mexican politics.

EVELYN P. STEVENS

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EDITORIAL NOTE

Articles Accepted Since Last Listing

David A. Baldwin, Dartmouth College, "Power and Social Exchange"

John R. Chamberlin and Michael D. Cohen, University of Michigan, "Toward Applicable Social Choice Theory: A Comparison of Social Choice Functions under Spatial Model Assumptions"

J. Patrick Dobel, University of Michigan–Dearborn, "The Corruption of a State"

Stephen T. Holmes, Wesleyan University, "Aristippus In and Out of Athens"

Robert W. Jackman, Michigan State University, "The Predictability of *Coups d'état:* A Model with African Data"

Richard P. Y. Li, Michigan State University, and William R. Thompson, Florida State University, "The Stochastic Process of Alliance Formation: A Time Series Analysis"

Joel A. Lieske, University of Cincinnati, "The Conditions of Racial Violence in American Cities: A Developmental Synthesis"

Delba Winthrop, University of Virginia, "Aristotle and Theories of Justice"

BOOK REVIEWS

Political Theory and Methodology

Arms Uncontrolled. Prepared by Frank Barnaby and Ronald Huisken for the Stockholm International Peace Research Institute. (Cambridge, Mass.: Harvard University Press, 1975. Pp. xiii + 232. $12.50.)

The Stockholm International Peace Research Institute has for some years been publishing compendia of data on world armament levels that, in many respects, equal or surpass in thoroughness and reliability the publications of better-known and more prestigious strategic research institutions. Most of this information has appeared in the SIPRI *Yearbooks (World Armaments and Disarmament);* from this and other SIPRI sources Barnaby and Huisken (both at SIPRI) have distilled the present volume. The book, though necessarily somewhat disjointed, brings together a wealth of information in a small scope and is potentially quite useful both as a reference and for the classroom (given students with a reasonable patience for technical material). Though focused on the nuclear arms race ("the greatest single threat to man's survival"), there is impressive coverage, with extraordinarily good statistical tables, of world military expenditure, arms trade, new tactical weapon systems (with chapters on the automated battlefield and antisubmarine warfare), chemical and biological weapons, and nuclear proliferation.

In drawing implications from this data, the SIPRI analysts take a "traditional" disarmament view, i.e., that arms are themselves a cause of conflict and high levels of armament are *ipso facto* more dangerous, theories of "stable" deterrence notwithstanding. Aside from the fact that this view is more tenable in nuclear circumstances than formerly (when arms were more clearly a *symptom* of conflict), the perspective has distinct practical advantages. Weapons systems are described in more awesome and dismaying detail than elsewhere (for example, the exhaustive list of chemical toxins and biological agents, pp. 97–107). The essential nonsignificance of SALT and other arms control agreements is more clearly perceived and more brutally stated ("for at least a decade the strategic nuclear forces of the two sides will probably be no different under SALT II than they would have been without it"). Finally, comparison of U.S. and Soviet military forces is carried out with a certain chilly "objectivity" absent elsewhere (see the concise summary of relative strengths and weaknesses, p. 50).

It is also useful to be reminded that world military expenditure equals the national income of the poorer half of mankind, that one-half of the world's scientific manpower is engaged in military research and development, and that the Non-Proliferation Treaty has probably already failed and in any event could not cope with the coming proliferation of power reactors. Some points of interpretation are, however, questionable. For example (p. 193): Has the United States indeed violated the NPT by agreeing to supply safeguarded nuclear facilities to non-nuclear-weapon states, without requiring that the recipient's previously acquired nuclear facilities be safeguarded as well? The point is doubtful at best.

SIPRI analysts tend to see technological opportunities as the major determinant of the arms race, at least in recent years (p. 23). It may be true that the dreams and visions of weapons designers are limited only by technological feasibility, but the process of getting weapons programs funded still requires considerable justificiation in terms of enemy capabilities and other such considerations. Far more weapons are dreamed of than actually deployed. There is also a tendency to emphasize internal bureaucratic and economic influences on the arms race; strategic doctrines are seen primarily as an effort to rationalize a weapons system promoted by some particular faction. There is much truth in this, but to claim (p. 157) that the "assured destruction" doctrine was articulated in 1965 in order to provide a rationale for Minuteman missiles, B-52 bombers, and missile-carrying submarines, goes too far. The idea that mutual invulnerability could provide a stable nuclearized world was developed in the 1950s, without reference to any one weapons system, and was then and is now sincerely held by intelligent theorists representing no particular interest.

The chapter on the "new" nuclear debate in the United States is perhaps most problematic. The central issue is identified sharply (p. 154, quoting W. K. H. Panofsky): "the evil dilemma [is] that the strategic forces on both sides must either be designed to kill people or else jeopardize the opponent's confidence in his deterrent." The issue, however, is at least in part whether Panofsky is correct: must one make such a choice, or is there a possibility of "stable" deterrence principally targeted on something other than civilian populations? The SIPRI researchers call attention, justly, to the fact that the United States has in fact possessed significant counterforce capability for some time. Does this necessarily create Soviet apprehensions of a U.S. first strike? Appealing though the Mutual Assured Destruction paradigm has been, is it not perhaps time to look for ways of ensuring strategic stability without holding enemy populations as "hostage"?

The SIPRI solution, however, is to stick to MAD in the short run: end qualitative advances in weaponry, reduce the number of nuclear weapons, and (implicitly) dismantle counterforce capabilities. The practical difficulties of such proposals, in arms control terms, are of course enormous—and in any event they are only interim measures, since in the SIPRI perspective any deterrence is unstable. In the long run (p. 159), "the only credible policy to reduce the probability of nuclear war may be far-reaching nuclear disarmament." The question of how this is to be achieved is stated at the outset (p. v): "if, after an objective assessment of the facts, many people judge that action must be taken to avert disaster, sufficient pressure will be brought to bear on political leaders to achieve this end." This comes perilously close to the "rationalist fallacy," i.e., the belief that correct diagnosis of a problem is equivalent to its solution. In this case the diagnosis is not beyond dispute; in any event, the "solution" is but a pious hope.

In closing, one might note that, in the two years since the last figures cited in the book, world military expenditures have jumped to an estimated $300 billion for 1975 (SIPRI estimated $200 billion for 1973, in constant 1970 prices). At least the definition of the problem remains clear.

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Socialism. By R. N. Berki. (New York: St. Martin's, 1975. Pp. 155. $10.95.)

A number of good books are easily condemned on a chapter-by-chapter basis but nevertheless contain a holistic integrity and ingenuity of thesis. If Louis Hartz's *The Liberal Tradition in America* had been published in serial form, it is likely we would have missed the overall brilliance of the work. On a less grand scale, R. N. Berki's *Socialism* is a book of this type.

After aptly criticizing current characterizations of modern "socialism," Berki presents his main thesis: "Socialism is not a single thing, but a range, an area, an open texture, a self-contradiction" (p. 16). The rest of the work explains the nature of that range, area, texture and self-contradiction. Involved in the explanation is the intellectual, social and political history of at least three centuries and five continents, tersely presented in 150 pages. Given this approach, it is obvious why Berki is easily open to criticisms of shallowness, oversimplification, reductionism, and ambiguity. But were one to stop here, the true worth of this book would be lost.

Berki contributes to our understanding of modern socialism in two ways. First, he provides an analytical framework which convincingly differentiates four normative tendencies in socialist thought and their historical representatives. Socialist "egalitarianism" is associated with the underdeveloped world; socialist "moralism" is linked to Western social democracy; socialist "rationalism" is tied to Eastern European/Soviet communism; and socialist "libertarianism" is connected to the New Left. His point is not that one value defines the essence of each variant; rather, Berki demonstrates that the primacy of one value is always in historical tension with (if it does not contradict) the other three. As a whole, this framework helps us understand the relationships between representatives of the different socialisms. Marxist humanism becomes a "moralistic" critique of Soviet bureaucratism; Soviet "rationalism" is antagonistic to Chinese "egalitarianism"; Cuban "egalitarianism" constitutes a critique of New Left "libertarianism" and so on. Berki's scheme helps us sort out and understand the intellectual bases for shifting socialist alliances and antagonisms, just as Hartz's scheme does for American liberalism.

Berki's second contribution to our understanding of modern socialism is to provide an overall sense of how socialism fits into a twentieth-century world. He skillfully demonstrates that socialist thought and practice are rooted in the complexities and contradictions of the Enlightenment which heretofore has been considered a liberal realm. He looks to those unclassifiable but influential thinkers who have done much to shape modern understanding for socialists and non-socialists alike: Rousseau (egalitarianism), St. Simon (rationalism), Owen (moralism) and Fourier (libertarianism). Furthermore, he devotes a (sometimes inadequate) chapter to Marx as a post-Enlightenment figure who affected the thought of socialists and liberals alike. The inference he draws from this analysis is that the Enlightenment essentially defined the problems of the modern world and that modern socialists (rather than liberals) capture the awareness of ambiguity and contradiction characteristic of the Enlightenment.

Enlightenment thought provoked the need to reconcile individualism and collectivism, compromise and revolution, scientific progress with human growth. While modern liberals generally shy away from facing (let alone reconciling) these antagonisms, Berki shows us that the range of modern socialism represents diverse attempts to face these questions, the questions of modernity. He has no illusion that socialists have discovered the answers—indeed, he is a good critic of each variant of socialism—but he argues that socialist heterogeneity, inspired by the Enlightenment, is healthy: "The contradictions of socialism are the contradictions of the age" (p. 20). His conclusion is that no one socialism adequately answers modern questions but the range of modern socialism constitutes a needed dialogue concerning these questions, a dialogue not characteristic of modern liberalism.

Unfortunately, Berki's book lacks a conclusion. Let me suggest a few inferences an American might draw from his analysis: (1) The "hyper-liberalism (rationalism, libertarianism) of some socialists and the "anti-liberalism" (egalitarianism, moralism) of other socialists is a useful framework for understanding the history of the American left, particularly the New Left. (2) That all socialists (and some left-liberals) share a concern with the same questions points to the existence of a normative ground for a leftist dialogue which goes beyond the sectarianism endemic in reformist/revolutionary movements. Berki demonstrates that socialism is neither a paradigm antagonistic to "liberalism" nor a number of discrete paradigms antagonistic to one another. Rather, it is an area of theory and practice which confronts the major values and dilemmas of our era. Berki's *Socialism* is an excellent source for understanding what may be the primary political perspective of the twentieth century and the justification for governance of more than half the world's people.

MARK E. KANN

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Abortion and the Sanctity of Human Life: A Philosophical View. By Baruch Brody. (Cambridge, Mass.: MIT Press, 1975. Pp. 162. $8.95.)

This book by a modern moral philosopher examines by the method of modern moral philosophy the arguments justifying abortion and finds them inadequate to justify the abortion of a fetus once the fetus is a human being. Brody fixes that point as at about the end of the sixth week of embryonic life (p. 112). After that abortion is "an act of murder" (p. 122). On his principles even abortion to save the life of the mother is immoral except in the unusual case in which without an abortion both mother and child will die (pp. 24–25). His position is the most stringent I have ever seen reached by purely philosophic considerations.

The audience he addresses are philosophers or, more broadly, academics. So great are the pressures for conformity in the narrow world of academe that he speaks to his colleagues with embarrassment. There are those, he confesses, who regard the discussion of abortion "as fruitless or even eccentric" (p. 132). The result he reached, he admits on his first page, occasioned him "considerable surprise" and his conclusion is not "popular." On page 2, he even apologizes for "the nature of the support, however, inadvertently, a position like mine is bound to attract." What testimonies to intellectual intimidation his surprise, embarrassment, and apology are! He has written a book concluding that abortion is murder. He is surely aware that abortion is practiced on an enormous scale in the United States, having increased sharply since its legalization (Alan Guttmacher Institute, *Provisional Estimates of Abortion Need and Services* [1975], p. 7); and he apologizes for his supporters! We can imagine a professor in Harvard College, circa 1850, pointing to the incivility of slavery while deploring the raucous voice of Garrison.

Moral argument, Brody rightly maintains, begins with an institution. Brody's argument for determining humanity rests on this intuition: "death is the moment at which he [a human being] stops being human and goes out of existence" (p. 100). He then shows that death occurs either when the brain ceases to function or when the heart and lungs stop. These functions, he argues, are what make a human being human (p. 107). Having determined these functions to be "the essence of humanity," he looks at the fetus to determine when they appear in the womb. He reaches the conclusion that by the end of the sixth week the essential functions are present; the fetus is then therefore human (p. 109).

The problem is that many persons would not share his primary intuition: "death is the moment he stops being human." Many world religions have held and hold that the essence of the human being survives death; Plato taught the same as philosophy. Even nonreligious, non-Platonic persons, I should suppose, speak of "human bodies" and do not treat a cadaver as dirt. That Brody should offer his intuition as a self-evident starting point, that he should see no need to prepare his readers ot explicate his intuition or relate it to the experience of other human beings can only be evidence of his addressing a narrow circle of materialists.

On one capital point, I find myself in enthusiastic agreement with the author: there is a fundamental difference between causing an act and not preventing an act that produces the same result, between killing and letting die (pp. 25, 30). Moral discourse is impossible if the two are systematically equated. Only fallacy results if they are equated ad hoc.

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The Correspondence of John Locke. 2 Vols. Edited by E. S. de Beer (Oxford: Oxford University Press, 1976, $55.00 each.)

Among Locke's unpublished writings, most of which have been hidden from general view until recent years, has been the bulk of his correspondence. Fortunately, the meticulous Locke preserved a substantial part of his enormous correspondence, including in some instances drafts of his own letters. The earliest public indication of the importance of his correspondence was afforded four years after Locke's death when some friends, including his extraordinary literary executor, Peter King, published a modest collection of letters. Though additional bits of the correspondence were published during the next two and a half centuries, they were oftentimes badly edited and in any event amounted to little more than a small fragment of the whole.

Thanks to the work of E. S. de Beer, the entire corpus of Locke's correspondence is now being published in eight volumes as part of the long-awaited Clarendon edition of Locke's complete works. Included in these eight volumes will be the texts of some 2,600 letters addressed to Locke and approximately 1,000 letters written by him. De Beer's editorial work is nothing less than a model of scholarship. He has taken extraordinary pains to present the reader with authentic texts of the letters; original spelling has been preserved, punctuation altered minimally (and only when necessary), while translations from Latin, Dutch and other languages are rendered literally. He frequently assists the reader in understanding the context in which many of the letters were written through explanatory notes and suggestions for further study. De Beer has brought to bear in this work decades of Lockean scholarship carried out in public and private collections of Locke's letters throughout the world.

Those interested in Locke, from undergraduates to the most advanced scholars, will find the *Correspondence* a most valuable tool in understanding his interests and activities as well as "the emergence of the enlightenment as witnessed by its great progenitor."

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Policy Analysis: What Governments Do, Why They Do It, and What Difference It Makes. By Thomas R. Dye. (University, Ala.: University of Alabama Press, 1976. Pp. 122. $6.50, cloth; $2.95, paper.)

Two research paradigms have emerged among political scientists who have become interested in the design, measurement and analysis of public policies. One, which grew out of the theoretical focus of systems design, studies the causes of public policies. This research focus treats policies as dependent variables and seeks to explain public policies by reference to independent social, economic, technological and political forces which are hypothesized to be determinants of policy outputs. A second paradigm, stimulated by increased governmental experimentation with social and human resource programs in the 1960s, studies the consequences of public policies, treats policies as independent variables and seeks to explain policy outcomes or the impact of governmental actions on social, economic and political conditions.

The findings of the systems paradigm probably are better known to most political scientists, both among those who assume a policy focus as well as those who research and teach political processes and institutions. The debate arising out of this research over the relative importance of political variables versus economic factors in the explanation of policy outputs has been one of the stimulating discussions in political science in the behavioral era.

The policy impact paradigm has gained recently in application largely because of its utility in applied policy making. Program evaluation, in public administration, grows out of impact theory and methodology.

Thomas R. Dye, among political scientists, is greatly responsible for the extensive application of the systems paradigm to public policy analysis. The sharpness of his conclusions—especially that political variables explain little of the variation in governmental policy outputs once socioeconomic controls are introduced—attracted a host of detractors soon after the publication of *Politics, Economics and the Public* in 1966. Using the Dye approach, others sought to improve on the measurement of political variables, conducted time-series analysis and introduced non-linear models—with the apparent expectation that methodological sophistication would result in a greater claim for the importance of politics in governmental allocations.

In his latest policy book, *Policy Analysis: What Governments Do, Why They Do It, and What Difference It Makes,* Dye has escalated the methodological debate into a theoretical argument. Political participation, electoral processes and behavior, interest group activity and partisan competition have been labeled as "pluralist variables," and Dye contends that political science suffers from a professional and an ideological myopia. As a policy science, it is professionally short-sighted in restricting the boundaries of the discipline to governmental institutions and processes. Professional tunnel vision stems from an ideological commitment to democratic pluralism. While the "pluralist variables" may have an intrinsic appeal to those interested in political values, Dye feels that policy researchers should have convinced political scientists, by now, that policy outputs are the product of social, economic, and technological forces.

If policy analysis in political science suffers a vision problem it may be in the eye of those who have applied only the input-output model, as Dye does in this thematic book.

His assumptions about political science theory and the contributions of the discipline to policy research should produce sharp reaction from social scientists who are studying policy impact. The impact paradigm raises different research questions from input-output analysis and holds more promise of explaining the effects of governmental decision making on policy allocations. Research in policy impact is concerned with effectiveness of government programs and with redistribution, or who benefits from the policies of political institutions. Among the journals which publish policy-oriented research, there is a distinct recognition of the utility of the impact paradigm. The 1976 volume of *Public Policy* alone includes studies of public school expenditure equilizations, low-income housing assistance programs and impact on racial segregation, the effectiveness of income transfer programs, consequences of campaign finance reform, urban transit costs, airport development in an age of protest, and effluent charges as a means of achieving pollution abatement.

The findings are not contradictory to input-output analysis, which in most cases has concluded that political factors do account for some variance. But more importantly, impact analysis focuses on variables that are susceptible to policy manipulation and change: how policy makers can effect institutional arrangements, improve policy delivery systems, and redesign policies to affect who benefits and increase program effectiveness and efficiency. "Pluralist variables" may account for only .2 of variance, or .1, in macro policy outputs, but they lead to policy analysis on which decision makers can act, if they so choose, whereas there is little, except over the long run, that anyone can do about the larger socioeconomic, environmental constraints.

Pluralism may be a restrictive ideology, particularly in the assumption that equity in policy outcomes is reflected in the judgment of the groups with the greatest access to the decision-making process. But it has not misled—or blinded—the discipline, as Dye believes, into asserting an importance of politics in the causes and consequences of public policies.

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Politics and Crime. By Hans Enzensberger. Selected and translated in part by Michael Roloff. (New York: Seabury Press, 1974. Pp. 215. $8.95.)

Politics and Crime is a collection of seven Hans Enzensberger essays on such topics as the theory of treason, genocide in the name of the nation-state, regicide as a last resort against tyranny, the potential for revolution in the Western industrialized states, the role of the Communist Party of Cuba, and the political implications of the ecology movement.

The first essay, "Towards a Theory of Treason" (1964), explores the ways in which the term, "traitor," can be employed with speed and severity by the modern nation-state against individuals it suspects as subversives. When such a regime feels itself threatened, Enzensberger argues, the dissident's rights—even in the so-called Western democracies—count for nothing.

"Reflections Before a Glass Cage" (1964) meditates on the Eichmann trial. Enzensberger asserts that murder is intimately intertwined with the creation and survival of all nation-states. Sovereignty involves the power to have someone killed. The present international system is a mortally dangerous one in which nations with the potential for atomic holocaust pursue their ends while considering themselves above any conceivable legal system. In such a setting, common criminals are a feeble anachronism. Their destructive potential is miniscule in contrast to that of his government. Eichmann and Auschwitz cannot be restricted to the 1940s. Nor are they part of a nightmare only for Jews and Germans. For Enzensberger they are reminders that the world's nuclear powers are using all the scientific and industrial means at their disposal to make possible the far more extensive genocide of tomorrow.

Enzensberger's next two essays, "Dreamers of the Absolute (Part I)—Pamphlets and Bombs" (1964) and "(Part II)—The Beautiful Souls of the Terror" (1964), provide an insightful account of the small bands of Russian terrorists of the late nineteenth and early twentieth centuries who sought personal and national salvation through the assassination of high-ranking officials of the czarist government. Such zealots, Enzensberger asserts with obvious satisfaction, constitute a threat to every regime founded upon its subjects' suffering. One of them, anonymous in the crowd, is enough to instill dread into "all those who hold power on this earth" (p. 102).

"Berlin Commonplaces" (1968) argues that "the western metropolises" (the United States, non-Communist Europe, and Japan) are haunted by the shadow of revolution. Externally, they face a future of inevitable murderous conflicts with the Third World. Permanent warfare between the have and have-not nations will be the reality. Internally, they cannot continue to meet their masses' insatiable demands for more and better consumer products. Their students and intellectuals have become their most bitter critics. They have been forced to silence dissidents through ever-increasing repression. There is a real possibility that their alienated groups will crystallize into class movements which will join the attack against them as they fight with their backs "to the wall at the periphery of the world" (p. 119).

But, Enzensberger maintains, a genuine socialist revolution will remain only an apparition unless the enemies of capitalism can "arouse the deepest wishes and desires of the masses" by demonstrating to them the "social possibilities" of a Marxist regime (pp. 124–25). They must, for example, find and disseminate answers to questions which are central to creating a new order in a post-industrial world: "What possibilities of self-government exist in a technically highly developed society? . . . What possibilities for the regulation of social conflict can be developed?" How can the performance of work be transformed into a learning process? (p. 124)

In "Portrait of a Party: Prehistory, Structure, and Ideology of the PCC" (1969), Enzensberger expresses his exasperation with the Partido Comunista de Cuba, which, from his perspective as a leftist theoretician, is completely un-Marxist. The PCC reflects the Cuban culture's furious animosity to theory. It is without Marxist-Leninist precepts against which to measure the development of its society. Mired in "moralizing rhetoric" and "simple-minded muddling," a mere medium for Castro's "unlimited sovereignty," the PCC lacks both historical and ideological legitimacy (p. 152).

"A Critique of Political Ecology" (1973) paints an apocalyptic picture of a world approaching ecological breakdown as a result of the excesses of capitalism. While Enzensberger believes that industrialization is far advanced in its destruction of the earth's sources of fuel, food, water, and air, he regards the "ecology movement" as nothing more than a propaganda campaign of the bourgeois and capitalist classes. The rescue of the environment, he argues, will not be attained through taxing the masses to pay for antipollution technology. Nor will it be achieved through exhortations to Third World peoples to restrict their birth rates. The abolition of the automobile, the desalinization of sea water, wars for raw materials, will prove only to be stop-gap measures. As long as capitalism survives, the quality of life can be expected to deteriorate. The world's hope, Enzensberger concludes, has to be socialist revolution. Only socialism can bring about the reconciliation of humanity with nature essential to survival.

This collection has its problems. Enzensberger's style is frequently ponderous and cryptic. His attempts to employ symbolism occasionally detract from the clarity of his arguments. Enzensberger demonstrates more intellectual force in assaults upon the ideological positions of others than in the development of his own. The title, *Politics and Crime*, implies that the anthology has more of a central theme than it does. But, each essay is stimulating. Enzensberger plays a valuable role in challenging major assumptions of Western and Communist political theory.

DENNIS D. DORIN

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Hume's Philosophical Politics. By Duncan Forbes. (Cambridge, England: Cambridge University Press, 1975. Pp. xiii + 338. $27.50.)

This book is pre-eminently a study of the Hume of history, a figure somewhat disregarded by philosophers even in the bicentennial of his death in August 1776. To those whose concern is to make Hume speak again in a context of twentieth-century thought, Forbes offers a reconstruction rigorously historical in at least two senses. In the first place, the Hume we meet is Hume the historian and commentator upon the politics of his own times, understanding of which they were to be viewed. In the second, it is Forbes's constant intention to warn us of the difficulty and near-impossibility of any consistent interpretation of the historical Hume's sustained meaning. "Hume is terrible campaign country," he says at the outset. "No interpretation ever seems to get going before it is pulled up almost immediately by some difficulty. The more of his writings one takes into account, the less confident one feels that one can ever break through. . . . And this perhaps is the ultimate mystery of Hume's 'scepticism,' which the devotees eventually attain to" (pp. vii-ix).

Forbes is telling us that the secret of Hume is that it can never be fully unravelled, and there are well-known difficulties and temptations in the way of conveying such a message. To begin with, Forbes's own text is not easy to read. One must not complain that he has failed to develop a rhetoric which masters his subject matter, since it is his purpose to show that the "terrible campaign country" cannot be reduced to the configurations of a sand-table; but there are times when one does not know whether one is reading Forbes's prose or his notes, his interpretation of his authors or his paraphrases and abridgments of their own words. He might have shown us that the ground is difficult and broken without making us stumble in loose rock cast there by himself. However, it is no part of his intention to mustify, and the matrix in which Hume is to be presented emerges in strong and bold outline once one knows what to look for.

The secret of the ideological Hume is his ambivalence; and the secret of this in turn is that his age was one of a nascent historicism, in which society was seen as growing and changing rapidly between a number of states, each of them in some degree desirable. The interpreter might then aim to fix the various pairs of poles between which the spark of Hume's thought moved, and one might possibly criticize Forbes because he is less anxious to do this than to show that Hume never came to rest at any one of them. The reader seems to follow him through a king of battle-course, in which a series of static and two-dimensional Hume-figures are riddled with fire one after the other; but when we ask whether Hume himself did not traverse the same course or a similar one, Forbes is no more merciful with us.

Forbes is determined to show that the ambivalences constitute the pattern, and that Hume's thought did not evolve according to any tidy dialectic, whether of resolution or contradiction. It is not to contravene this important conclusion to suggest that Forbes spends much of his time demonstrating that Hume did not become increasingly "Tory" or increasingly "reactionary" as he grew older, and that he may spend too much of his time on this point for the overall strength of his argument. The word "Tory," after all, does not have a single consistent meaning. If there was a late-Stuart Toryism which venerated the royal prerogative, hereditary succession and high-church principles—Hume as historian admired the first of these but not the second or third—there was also an early-Georgian Toryism, represented by Bolingbroke, which carried the country detestation of executive patronage and government finance to lengths indistinguishable from those pursued by Old Whig Commonwealthmen. Hume as political commentator was a lifelong defender of the need for patronage, and we may get from Forbes the feeling that "Court Whig" or "Walpolean Whig" is at any rate nearer fitting him than any other label (though the *History of England,* written on the Walpolean principle that the principles of the constitution were modern not ancient, appeared at a time when both oligarchic and radical Whigs had reverted to belief in the Ancient Constitution).

But if Hume accepted the need for patronage, he also declared that it would probably be the death of the constitution in the end; and Forbes seems to be having trouble with his even more constant declarations that public credit was capable of destroying the cement of society. If the letters of Hume's last years seem Stuart-Tory in their insistence that authority must be upheld against popular faction, they are Hanoverian-Tory in their insistence that Pitt's wars have increased the national debt past bearing; and the ultra-Whig Richard Price could applaud Hume's conclusion that America would be better given up on these grounds, with no indication that Price thought he was quoting Hume against himself. This convergence of ideas is not enough to make Hume a Tory; he is more like a Whig who thought the Whig system had run up against its own inherent limitations; but Forbes is absolutely right in insisting that the sense of limitation and tension is what matters, and that finding the correct party label does not.

There remains the problem whether the tension is being depicted in terms of the right set of forces. Perhaps the least satisfactory feature of Forbes's portrait is his isolation of a "vulgar Whiggism" (pp. 125–92), which Hume's thought transcended—without ever quite losing—in the form of "scientific Whiggism." Not only is "vulgar Whiggism" attributed to Bolinbroke, who was never a Whig at all; it turns out to have consisted largely in the belief that under European absolute monarchy there could be neither security of property nor liberty of culture. It is of the first importance that Hume's "scientific Whiggism"—the term was coined by Forbes long ago—was based on the premise that absolute monarchy might develop a freedom of leisure and culture unknown in free commonwealths; but "vulgar Whiggism" is far too flat and simplistic a term to tell us what Hume was rejecting. He was also rejecting the contention that the free commonwealth was necessarily a virtuous republic, pre-commercial and austere to the point of primitivism, and that the growth of culture was synonymous with the move towards corruption; and this contention was as much Tory as it was Whig, and was not vulgar at all. In rejecting it Hume was involved in one of the age's most complex debates, and if he held that the virtuous republic had never quite existed in history, he did not deny that it was in principle the best form of government; value and history were in tension.

Hume's ambivalences were those deeply felt by his age, as both Rousseau and Jefferson showed, in very different ways, by the quality of their overreaction to him. If there are points at which Forbes might have depicted the eighteenth-century debate in stronger relief and with sharper lights and shadows, we must be grateful to him for his book. This is how the historical Hume is to be found; and his skepticism was as much historicist as it was epistemological.

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Kant and the Problem of History. By William A. Galston. (Chicago: University of Chicago Press, 1975. Pp. xiv + 290. $13.50.)

This book investigates the "crisis of liberal democracy" through an inquiry into the emergence of the idea of history in the writings of Kant. Galston argues that the conflict between the various forms of contemporary politics—liberal progressivism, Marxism, and "power voluntarism"—may profitably be viewed as an "internecine struggle among ideological outgrowths of the modern historical consciousness." And the modern doctrine of history was given its first significant elaboration by Kant (pp. 3–5).

Kant took his point of departure from Rousseau, who had described the human situation, in the material kosmos of Newtonian natural science, as dualistic: a human being is simultaneously a product of subrational physical causality and rational human freedom (pp. 103–31). But Kant could not rest satisfied with this antimony. For if human beings are truly free agents, their actions must have an effect on the physical world observed by the scientist; but this is impossible if the physical world is fully accountable in terms of an ineluctable chain of natural causes and effects.

The idea of history, Galston argues, is Kant's hesitating and questionable solution. According to the suggestions in Kant's historical works, the irrational interactions of human passions in the empirical world can be thought of as leading to a rational political order, which, if not itself necessarily moral, would at least establish a political order compatible with genuine morality and inner freedom. History could then be conceived as a progress from passionate, asocial, merely physical beginnings to an ideal of perpetual peace and republican freedom—although the ideal may never be reached.

Kant's historical teaching is vulnerable to many objections. Perhaps the most crucial, in Galston's view, is that the idea of history does not, in fact, overcome the dualistic concept of mankind bequeathed by Rousseau. *Historical* progress, which proceeds according to the laws of physical causality, cannot produce *moral* progress, because moral freedom in its very being is something purely spontaneous and autonomous, untouched by material forces (pp. 240–46, 264–68). Yet Kant may be said to surpass his successors by his resolute confrontation of the central problem of modern scientific thought—whether there is a place for the human in the world of science—and by his resistance to embracing any solution blind to its own shortcomings (p. 267). Kant's tentativeness allows a better access to the peculiar strengths and difficulties of the idea of history than does the more confident, but not necessarily superior, system of Hegel (p. 5).

The breadth of Galston's book distinguishes it from the narrower approach of the usual Kant scholarship, which tends to neglect not only his place within the modern philosophical tradition, but also his quarrel with the philosophy of antiquity. Galston provides extended discussions of Kant's relation to Rousseau (his profoundest teacher) and Aristotle (his most serious opponent). However, the heart of the book is Galston's claim that in studying Kant, one discovers that the problem of history, which at first seems to be comprehensible in its own right as an issue of political philosophy, in fact points back to the problem of science as the most fundamental stratum of modernity (pp. 15, 62, 188–95). He implies that we must reexamine the roots of modern science, through a study of the physics and metaphysics of early modern thought, if we are to uncover the true origins of contemporary politics. This argument is at once the deepest level of the book and its most controversial assertion. For in my opinion, Galston has not properly addressed himself to the analyses of Heidegger and Leo Strauss—although he knows and even admires their writings. These men trace the core of modernity back to something more fundamental than science: the metaphysical doctrine of the will as the ground of the beings (Heidegger), or the essentially *political* character of all modern philosophy, and therewith also of modern science (Strauss). (See Heidegger's *Nietzsche* and Strauss's *Thoughts on Machiavelli.*) For modern thought the ground of all things is said to be found in the Cartesian will or in Machiavellian *virtú*, both being expressions for the ability to form the external matter of the world in accordance with human projects. The rejection of science by Nietzsche and the existentialists, far from being a departure from modernity, could then be understood as a radicalization of, or a return to, the original modern principle. Science is now revealed to be only one of many possible forms of the will to overpower all limits to the human will. Hence Kant's tentative replacement of Nature by History as the realm where human conduct becomes visibly intelligible, prepared the way for Nietzsche's abandonment of science (knowledge of nature) in favor of his study of the creative willfulness of great men as the deepest source of life's significance.

But such objections cannot affect the assiduity and penetration that characterize the book, which is probably the best now available on Kant's political philosophy.

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Prospects for Constitutional Democracy: Essays in Honor of R. Taylor Cole. Edited by John H. Hallowell. (Durham, N.C.: Duke University Press, 1976. Pp. xix + 197. $9.75.)

In his preface John Hallowell characterizes this volume as representing "a variety of methodological perspectives" (p. xv). However, the impression is not of variety but of homogeneity: a generally pessimistic view of the prospects for constitutional democracy, ranging from the mildly skeptical (J. A. Corry, "The Prospects for Constitutional Democracy"), to the very skeptical (Zelman Cowen, "The Way We Live Now"), to the apocalyptic (Helmut Kuhn, "Germany—Divided Once More"). Furthermore, the contributors not only express varying degrees of anguish about democracy in its "homeland" in the First World; they also see even less hope in the Third World, where efforts at institutional transfer are not only doomed to failure (J. Harris Proctor, "Communal Representation in the Republic of Malawi"), but are judged to be immoral (Ralph Braibanti, "Context, Cause, and Change"). In spite of some subsequent variations in shading, a Spenglerian mood is set by Zelman Cowen, an Australian legal scholar, who near the end of his essay turns to the well-known passage from W. B. Yeats' "The Second Coming" in which "anarchy is loosed upon the world" in a "blood-dimmed tide"—a world in which "the best lack all conviction, while the worst are full of passionate intensity" (p. 20). To this the author can only add that despite our vast technological skills "we have neither clear minds nor clear directions." It is unfortunate that such a conclusion should mar an essay in which the author draws a reasonably fair balance sheet between contending views about the rule of law.

Looked at in another perspective, the essays divide into two or possibly three groups: philosophical and empirical, with a third group of essays trying to develop an analytic or evaluative stance. Among the first two groups the empirical essays are among the best in the volume (Samuel H. Barnes, "The Dark Side of Pluralism: Italian Democracy and the Limits of Political Engineering" and Howard A. Scarrow, "Participation Through Decentralization: The Case of Britain." They contribute more to a thoughtful consideration of the varieties of democratic government than do the philosophically oriented essays (Cowen; Barry Cooper, "Culture and Anarchy: The Politics of Matthew Arnold"; Corry). Scarrow scores an important point about British democratic government essentially as "administration" rather than as local self-government. One of the most impressive contributions is Samuel Barnes' examination of the interplay of political forces in Italy and the short-to-medium-range consequences of this interplay for democratic government. His projection of the increasing institutionalization of the Italian Communist party has been most effectively borne out by the June 1976 election and events since that time. Of all the essays dealing with the shape of contemporary democratic conflicts, Barnes' essay is just about the only one that can still be read without flinching. Francis Canavan begins and ends his essay with the assertion that a "united Ireland" is the only way out of the impasse. Nothing that has happened in the last year or so gives credence to such a forecast, and even Canavan cannot avoid citing Richard Rose to the effect that there is no solution available to the Irish troubles. Even more disturbing is Helmut Kuhn's essay about contemporary West Germany. In 18 pages the author provides a thumbnail history of the postwar period, deals with the "other Germany," and then launches a partisan attack on anything that is even vaguely liberal in West Germany. His premonition of doom would do justice to a C.D.U. election pamphlet and the general reader might be justified in wondering how all this fits with the election results of October 1976 in which the conservative parties came within a whisker of winning the election—an election in which the "social-liberal" coalition advocated economic policies nearly indistinguishable from those of any conservative party program.

A third and somewhat varigated group of essays has in common an analytic or evaluative mode; this group includes essays by Braibanti; J. A. A. Lovink ("Prospects for Democratic Control"); and Emmette S. Redford ("Watergate: A Test of Constitutional Democracy"). Of these three, Redford had probably the most thankless task, i.e., to say something genuinely new about Watergate and democratic government. Given the space limitations and, even more, given the veritable flood of words in which we all have been drowned, what is there left to be said in a brief essay? Lovink examines various possible instruments for exercising democratic control over bureaucracy and public policy. Much like Theodore Lowi, whom he cites frequently, Lovink sees little hope in what has been tried in the past. But unlike Lowi he does not even offer a "remedy." In the end it is Branbanti's essay that provides an antidote to the *Gotterdämmerung* scenarios of his coauthors. He argues neither for the status quo nor against it, neither for change nor against it. Instead he tries to demonstrate that institutions and processes, including constitutional democracy, operate in a context and that we disregard this contextual element only at our peril. Though he focuses his attention on the Third World and its problems, what he has to say about the ambiguities of change and modernity apply to the First World as well. Thus Braibanti deserves our gratitude for helping us clear our vision dimmed so badly by the other essays in this volume.

A final query. As one familiar with Taylor Cole chiefly through his published work, I find the general direction and impact of this festschrift not so much an accurate "reflection of the breadth and eclecticism of Cole's work" (Hallowell, p. xv) as very much at odds with the consistently optimistic and empirical qualities of his writings. I wonder what he makes of this festschrift which so determinedly calls into doubt the ideas and institutions that constitute his life's work.

ALFRED DIAMANT

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Macaulay and the Whig Tradition. By Joseph Hamburger. (Chicago: University of Chicago Press, 1976. Pp. xiii + 274. $17.50.)

Books about Macaulay tend to be stout and ruminative. Hamburger's study is slim and ruminative. It is written for readers who already know a good deal about Victorian England and about the circumstances of Macaulay's life and writings. For such readers, it provides a thoughtful discussion of Macaulay's place within the tradition of Whig history and politics. Hamburger argues that Macaulay's most fundamental instincts in politics were those of the classical trimmer. His centrism was not so much a principled liberalism as a fear of extremism, of both the Tory and the Radical stripe. Hamburger gives a good deal of evidence of this trimming mood in Macaulay's work, and places him within a tradition of "philosopher-politicians" including Burke as well as Halifax, who stood somewhere between Macaulay's men of theory and men of business, and had affinities further back with Bacon and with Machiavelli. This study is useful for a variety of reasons. It leads one to think about the complex components of the moderate position, and the different routes one can take to arrive at the middle of the road. It sheds a good deal of light on Macaulay's own intellectual affinities and aversions, as well as the links among a group of individuals who have made similar contributions to a distinctive perspective on politics that cannot easily be placed within the familiar ideological spectra. It also reminds us that, however serene the Victorian age appears in retrospect, sober Victorians could plausibly view it as a time of alarums and conflicts, when stability and progress from Macaulay's "noiseless revolutions" in society and technology were continually threatened by the twin spectres of anarchy and tyranny. And finally, like Hamburger's earlier studies of the Mills, it provokes thought about the role of "intellectuals in politics" in this or any other age, despite the fact that one of Hamburger's achievements is to make us see that Macaulay, though bookish to a fault, had little patience with intellectual abstractions.

Hamburger has succeeded admirably in doing what he set out to do in this book. His purpose was to call attention to Macaulay's character as a principled trimmer, in order to make us aware that "the Whig label" should not be applied to him indiscriminately, and to help us see the contributions of Macaulay's trimming to the development of Whiggism. However, he raises, explicitly or by implication, a number of other interesting questions along the way, and it is perhaps a disappointing feature of the book that he was disinclined to pursue them more systematically. He uses, for example, the manuscript of Macaulay's unfinished "History of France," discovered by some clever detective work in which he was aided by his son, but gives us only glimpses of its contents. He resists any temptation to compare Macaulay with the *juste milieu* historians of contemporary French liberalism or with Tocqueville, or even to draw systematic comparisons between Macaulay on the history of England and Macaulay on the history of France. He describes Macaulay (p. 166) as "preeminently political," but he appears here much more as historian and spectator of politics than as actor. Thus the dimension of trimming-in-action that might have been added by a more sustained attention to his activities as parliamentarian and administrator is missing. Most important in terms of the intentions of the book, Hamburger provides only a shadowy account of what the "Whig tradition" had become in the nineteenth century, although surely such an account is necessary if we are to discover how Macaulay differed from his fellows and what he contributed towards the development of an ongoing pattern of thought. There is a brief discussion of this in chapter 6, in terms of the "double pedigree" of nineteenth-century Whiggism—"the liberal conviction and the prudential consideration." But the beliefs and principles of contemporary Whigs remain vague and sketchy here, and the relationship of their beliefs to earlier components of the tradition—Lockean ideas and the Commonwealth republican line of thought—is left obscure. This is surely due in part to the fluid and imprecise character of Whiggism itself, but Hamburger's description of the way Macaulay helped shape this tradition would be sharpened by a clearer idea of what he had to work with.

Nonetheless Hamburger gives us a clear and memorable sense of Macaulay's own worldview, relating it deftly to his family background and his tendency to understand contemporary events in terms of categories drawn from the past—Puritans and Jacobins reincarnated in the several types of radicals, Laud resurrected in fanatical Toryism. Hamburger also provides a sympathetic portrait of the trimmer, not as opportunist or as hack, but as a person of principle conscious of the need for a centripetal force in a centrifugal world, motivated, as Macaulay's colleague Francis Jeffrey described it in the *Edinburgh Review,* by "a desire to compromise and reconcile the claims of all the great parties in the State."

NANNERL O. KEOHANE

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Marxism and the Theory of Praxis: A Critique of Some New Versions of Old Fallacies. By John Hoffman. (New York: International Publishers, 1975. Pp. 239. Price not given.)

Marxism and the Theory of Praxis is a frontal attack on virtually all of Marxian thought which has become academically respectable in the West. Dr. Hoffman criticizes Lukacs, Korsch, Gramsci, Marcuse, A. Schmidt, Sartre, Lefebvre, Avineri, Kolakowski, Petrovic, the students of Althusser and a host of others who are categorized as the praxis writers. Hoffman believes that these thinkers who fail to comprehend historical materialism, the theory of the modes of developing interaction between humanity and nature, within a philosophy of being or dialectical materialism (as Engels and Lenin did) are forced to adopt unsatisfactory dualist or idealist theories from which flow irrationalist or "subjectivist" political practices.

Hoffman criticizes the praxis writers for contending that Marxism cannot be a philosophy, that dialectics cannot exist in nature, that consciousness cannot reflect reality, that the distinction between base and superstructure is unsatisfactory, that the indeterminacy of action is required by the concept of praxis and that *The Paris Manuscripts* are a significant part of Marxist theory.

The issue as to whether Marxism is a philosophy is not decisively resolved as it depends upon what is meant by a philosophy. The praxis writers who deny philosophic status to Marxism insist that it is not a closed system of final truths, but is subject to empirical analysis and political experimentation and is open to development in accordance with changing social practices. Hoffman does not explicitly confront the issues of falsifiability and finality; he does not state, for example, whether the primacy of the economic base to the political and cultural superstructure is an empirical generalization which is in principle falsifiable in certain situations or whether it is necessarily true in all times and places; he does not explicitly state what aspects of Marxism are final and what aspects may be changed without "revision" of what is essential to Marxist theory.

The crucial point at issue between Hoffman and the praxis writers pertains to the intelligibility of the world external to human experience. The praxis writers assert that we can know of the world only as it exists "for us" and thus the proper object of Marxist theory is the history of practical or productive interactions between humanity and the rest of the natural world; an account of nature "in itself" would be as void of content as a human psychology, stripped of relationship to things and other persons. Hoffman, on the other hand, believes the external world has an "objective rationality of its own" prior to human interaction reflected in a scientist's consciousness as true ideas. The reason why moving matter should have a rational structure such as to enable a correspondence between ideas and a world external to human experience is not explained, but such a postulate, we are assured, is essential to distinguish appearances from knowledge of reality. Hoffman points out that Marx and Engels believed that dialectical movement obtained in the natural world, although he curiously cites (p. 52) to support his point *The German Ideology* which states that the two sides of history—the natural and the human—are "inseparable."

Marxism and the Theory of Praxis provides an interesting defense of a "reflectionist" epistemology from the charge that it presupposes a passive role for consciousness in the reflection or mental recreation of objective natural laws. I think Hoffman has not demonstrated how reflection upon external reality necessarily constitutes a transformation of that reality but has justified a claim to greater attention to a correspondence epistemology amongst Marxologists.

Hoffman's case for the determinism of Marxist materialism is incomplete in that he never discusses internal determinants of human action. An account of human needs and abilities is essential to show how transformative practices (productive and political) correspond to a reflectionist epistemology. If what is reflected is merely the determinate external world (and not also determinate needs and powers reacting to, and interacting with, it) there is no reason why reflection upon externality is ipso facto its transformation. I share Hoffman's view that concepts like "creativity" or "indeterminacy" do not explain transformative practices. However I find utterly incomprehensible his belief that uniquely human attributes "can only be explained by idealism" (p. 168). Could it be that Hoffman shares with the more irrationalist of the praxis writers the view that human reality is "subjective" or "indeterminate" and thus incompatible with the objectivity of science?

The political implications of the ontological postulate of the external world rationally constituted in objective laws of natural history and the epistemological theory of correspondence or reflection justifies, according to Hoffman, the control of social processes by those possessing Marxian science. Without a science based on this ontology and epistemology, we have the relativism and skepticism of social democracy or worse, blocking the guidance of a scientific vanguard in a prerevolutionary situation and the role of scientific planners during socialist construction. Indeed there is a correlation between the "praxical" rejection of Engelsian scientism and the politics of spontaneity which Lenin and Hoffman denounce. The most interesting aspect of Hoffman's political reflections, it seems to me, are the reasons for his rejection of the new left slogan of "Workers' Control."

Hoffman's sweeping criticism of the praxis school of Marxism, while not rendering justice to all thinkers categorized in this group, does command attention to what is required by a materialist ontology and epistemology. The critical emphasis, however, impedes the constructive attempt to provide a philosophic foundation for a science of politics and society and for a cognitive ethics. *Marxism and the Theory of Praxis* claims more than it accomplishes.

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Montesquieu and the Old Regime. By Mark Hulliung. (Berkeley: University of California Press, 1976. Pp. 258. $11.50.)

The Political Theory of Montesquieu. By Melvin Richter. (New York: Cambridge University Press, 1977. Pp. 355. $17.95, cloth; $6.95, paper.)

Each of these works deserves some praise. But I prefer to begin with summary criticisms. Montesquieu begins to trouble the modern scholar much as Aristotle troubled medieval schoolmen. Because he provided sources rich in practical advice and paradigms or "ideal types," the temptation to adapt his ideas to subsequent circumstances, however distant, is well-nigh irresistible. The consequence is a profusion of seedlings withering in the shadows of mature trees in a great forest.

Each of these efforts defends its failure to present a comprehensive account of Montesquieu's teaching by denying that Montesquieu achieved a comprehensive understanding. (Richter: pp. 4, 6, 17, 64–65, and passim. Hulliung: pp. 27, 108–117, 171, 185.) Richter treats Montesquieu as eclectic (pp. 21–22). The introduction to his translations comprises roughly a third of the book. It is a critical analysis, but "critical" is not used critically in this review. Richter sidesteps the problems requiring explication in Montesquieu's texts. Those problems are glossed, ignored, or handled by the repeating of "commonplaces" of the scholarly tradition.

An early example of this sidestepping occurs when he questions the sincerity of Montesquieu's claim that "nothing he wrote could be interpreted 'to censure arrangements established anywhere in the world' " (p. 17). This is rather Richter's than Montesquieu's question. The statement from *The Spirit of the Laws* in no way limits prospects for interpretation nor even the incidental effects of Montesquieu's teaching. It addresses only his purpose in writing: "Je n'écris point pour censurer ce qui est établi dans quelque pays que ce soit." The incidental effect of censure is perfectly compatible with this stated intention. Or, again, Montesquieu's claim of a design for the work has trapped subsequent scholars in the search for unity. That implies analysis of the text as such. But Richter suggests a genealogy of ideas to resolve that claim, abandoning the assumption that Montesquieu's claim ought to be honored *solely* because he made it (p. 64). But, again, Montesquieu's conception of nature and laws of nature, philosophical or "metaphysical" in *form,* is really only a "political" or "secular" conception. He "himself said any number of times that *The Spirit of the Laws* is not a theological work" (pp. 64–69). Here, it is Montesquieu's own claim which has force.

Finally, the same process, interpreting through narrowing the sense of the text, is seen in the account of the separation of powers (pp. 90–91). Initially, the account resembles expansion rather than constriction. Richter holds that Montesquieu employs three definitions of separation of powers rather than one. But this interpretation requires a radical separation of very closely related passages. Besides taking the "second definition" out of context, Richter does not reveal that it in fact arises from the very next paragraph after the first definition in the first paragraph of only one sentence; the second paragraph is only four sentences which develops the first paragraph and, in particular, places the domestic and foreign power of enforcing law on a single footing and oppose the power of judging. The whole, then, is only the continuing definition of a new conception. When the "second" definition occurs, it is a reference to—a gloss on—the previous definition. Completing and defending the expansion, Montesquieu then states it in summary form. Thus, this is the *only* definition employed throughout Book XI, although the original version is used again in the discussion of England in Book XIX, chapter 27. The whole seems to reveal Montesquieu's consciousness of how far he could attribute his principles to England (or Locke) in fact and how far only ideally.

What may be said of Richter's translations? His own appraisal seems accurate: "I have not hesitated to depart from his sentence structure. Even had I wished to do so, I could not have produced the complex and beautifully balanced sentences he sometimes wrote." In addition, as Richter recognized, there already exist relatively adequate translations of the *Persian Letters* and the *Considerations.* Those translations remain unsurpassed. But the greater portion of Richter's work is rightly devoted to *The Spirit of the Laws.* And this is the first attempt at translation of that work in over 200 years.

With regard to the passages selected, it is curious that the interpretation of Montesquieu as a conservative political sociologist did not lead to greater emphasis on Book XX, on the laws of commerce, or on any of the final books on feudal history. More important is the omission of those troublesome passages which are central to a comprehensive analysis. An example is Book IV, Ch. 8, "An Explanation of a Paradox of the Ancients, in Relation to *Moeurs.*" Because that chapter is omitted, the work also fails to provide annotation for one of the most dense and critical series of footnotes in the work–beginning at IV, 8, and apparently ending at V, 17.

The glosses in Richter's interpretation are reflected in his translation. This leads to a paradox. Richter's translation is generally superior to the parallel passages in Nugent, because there are fewer direct mis-translations. But Richter speaks so strictly to received opinion in his own time, that this work may well fail to survive the test of time even as well as the Nugent translation, though acknowledged to be defective.

There is one great advantage in Richter's translation: He makes available in English heretofore neglected passages which deepen our understanding of Montesquieu. These are the unpublished sequel to the fable of the Troglodytes and the ninth chapter of Book XV in *The Spirit of the Laws.*

The defects of Hulliung's work are more briefly noted. He identified the attack on the ancien régime with an attack on natural law. Thus, he remained blind to the natural law basis of modern liberalism (as expressed in the Declaration of Independence) and exaggerated the radical skepticism of Lockean epistemology. Predictably, the work ends by denying to Montesquieu any genuine foundation for the prescriptive republicanism which it is the primary purpose of the book to establish. Where Montesquieu's own work would contradict this development, Hulliung treats the principles involved as anomalies to be excused (as at p. 174).

The failure to treat *The Spirit of the Laws* as a whole is symptomatic of this flaw. To defend the thesis of Montesquieu the social scientist, Hulliung systematically attacks the idea of Montesquieu the philosopher (specially, Ch. 5, 1). But at every key point in the argument, the author turns not to conclusive proof from one of the published works but to the *Pensées*. In fact, the thesis is not confirmed in any of Montesquieu's published works viewed as a whole nor in any *pensée* which can be considered as making a sustained argument.

Hulliung produces a penetrating analysis of the details of Montesquieu's work. For example, nowhere else in general publication is full appreciation of Montesquieu's understanding of the jury system shown (not at p. 213, where he follows tradition, but at p. 219). Similarly, no one else has so clearly discussed the status of justice for Montesquieu (though the content remains confused). See especially pp. 174–75.) But this penetration of details is achieved at the expense of denying necessary connections between them.

Because Hulliung can discover no organizing conception of nature in Montesquieu, he imagines him to offer "fiction" (Ch. 5) and fictionalized "history" (Ch. 6) as "surrogates for natural law." In the end, both are found inadequate, and Hulliung preserves his evident admiration for his subject only by discovering that this inadequacy is rooted in the "nature" of human things: "Social science *interprets* but does not change the world; the social scientist is one man and the politician another. Each has a type of political knowledge, but the nonconceptual 'know-how' of the political actor–ruler or revolutionary–is alone relevant" (p. 229). Understanding that no one can foresee all the unintended consequences of prudential endeavor, Hulliung assumes that all intended consequences are untenable. (Cf. p. 175.) Though he articulates the problem, he fails to see Montesquieu's anticipation, e.g., the political use of historical analysis as a means of conferring legitimacy *and* evoking salutary change.

The recursion to mystery and myth suggests the difficulty of Hulliung's analysis. He assumes Montesquieu's prudent modernity to be merely half-hearted liberalism instead of the expansion of liberal principles to include principles of prudent judgment.

A final negative comment: Hulliung defines the ancien régime–Montesquieu's understanding of it–as "aristocratic society." (See II, 2 especially.) However this may conform to common understanding, it clearly does violence to Montesquieu's labored conception of aristocracy as a form of the republic and distinct from monarchy. To discuss the books on "Monarchy," systematically substituting "aristocracy" for "monarchy" is simply unacceptable. It conforms Montesquieu to the thesis of the analysis.

Despite these criticisms, it is still possible to laud Richter's and Hulliung's participation in the rebirth of Montesquieu studies of the last 20 years or so. Their Montesquieu is incomplete, but it contributes to increasing awareness of the need for a better account. Some things they miss. Both fail to give due attention to the centrality of the question of the size of republics. Both fail (Hulliung less so) to regard the implication of Montesquieu's theory of climate from the perspective of civilization. And neither credits Montesquieu's self-understanding of his work and his chief interlocutors. To read their accounts of his vagueness, paradoxes, anomalies, and eclecticism is to imagine for a moment that they consider Montesquieu a Mao Tse-tung; they have conjured up a cherubic little form spewing forth capsules of Satanic wisdom in a Jobean lottery. Still, what they accomplish is significant.

Hulliung succeeds in restoring the question of Montesquieu's position to the plane of eighteenth-century concerns. And, in doing so, he elevates the most neglected sections of *The Spirit of the Laws* to a status level with the most interpreted sections. Richter breaks no new ground in assessing Montesquieu's position amid theories of the emerging nation-state of modern liberalism. But he does make clearer than was heretofore established the need for a faithful and literal translation of *The Spirit of the Laws.*

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Michael Bakunin: Selected Writings. Edited by Arthur Lehning. (New York: Grove, 1974. Pp. 288. $4.95.)

Bakunin on Anarchy. Edited by Sam Dolgoff. (New York: Knopf, 1972. Pp. xxvii + 412. $10.00, cloth; $2.45, paper.)

God and the State. By Michael Bakunin. (New York: Dover, 1970. Pp. xii + 89. $2.00.)

Living My Life. By Emma Goldman. (New York: Dover, 1970. 2 vols. Pp. 994 + xvi. $4.50 each.)

Anarchism and Other Essays. By Emma Goldman. (New York: Dover, 1969. Pp. xiv + 271. $3.00.)

Fields, Factories and Workshops Tomorrow. By Peter Kropotkin. Edited by Colin Ward. (New York: Harper and Row, 1975. Pp. 205. $3.45, paper.)

The last decade has been a good one for anarchism and those interested in it. In addition to the volumes listed above, Proudhon's *What is Property?* has been reprinted by Dover, and his *General Idea of the Revolution in the Nineteenth Century,* by Haskell House. A *Selected Writings of P.-J. Proudhon,* edited by Stewart Edwards and providing much never before available in English, was published by Macmillan in London, and was available as an Anchor paperback until Doubleday unfortunately chose recently to let it go out of print. And of the writings of Kropotkin, *Mutual Aid* and *The Conquest of Bread* were reprinted, edited and introduced by Paul Avrich, by Penguin in England and New York University Press here, while the collection entitled *Kropotkin's Revolutionary Pamphlets,* edited 50 years ago by Roger N. Baldwin, was reprinted by Benjamin Blom and by Dover. All told, anarchist writings seem more easily available now than ever before.

This is probably no accident. An anarchist flavor, at least, pervaded that broad movement called the "New Left" from its early and fairly impressive statement in the Port Huron document of the SDS in 1962. The apparent decline in that movement in recent years has hardly been accompanied by a revived confidence in government; the political alienation now noted has a different quality than the apathy of the fifties.

Political science, too, has changed. Anarchism seems a good deal more interesting and relevant than one might have anticipated 20 years ago. It may be in good part a dream, but it is not a nonpolitical dream, and political scientists can properly include it among their respectable theories.

That said, however, we must recognize that anarchism is not a simple theory, if it can be at all reduced to a theory, nor can it be adequately expressed in any single literary form. The volumes being considered here are very diverse in both form and content. Ranging from autobiography to fairly compact scientific or philosophical analysis, they differ not only in the theoretical perspectives they develop, but also the degree to which they develop any at all. Proudhon and Kropotkin elaborated rather substantial though very different theories, but Bakunin, once he drifted from Hegelianism, had relatively little, and Emma Goldman's anarchism seems largely a matter of common sense and decency outraged by experience. Anarchism may well be a frame of mind, only partially expressed in or supported by theory. If so, the very diversity of these writings in both style and content is a significant manifestation of the anarchist perspective, and none should be ignored.

The core of anarchism is, of course, the exaltation of human freedom and the insistence that it is the prerogative of all mankind, equally. Anarchists have generally been convinced that this is not incompatible with an at least sufficiently cooperative harmony, which would emerge "spontaneously" among the emancipated masses. They may thus be distinguished from Stirner and Nietzsche, although Goldman gave even these her blessing in the preface to her volume of essays. On the other hand, they did not follow Locke, Rousseau, Hegel, or Marx in largely identifying freedom with the proper sort of sociability, which provides justification for authority, established or revolutionary. Even Proudhon, whose theory led him to assert that in the end liberty and justice "merge," exclaimed when pressed that he had "no system," that no science can predict the actual consequences of liberty. Anarchists have been singularly willing to live with change, unpredictability, and even conflict, sometimes even making it a matter of principle, rather than resort to the usual forms of authority to establish and maintain order. Rather than justify authority, they justified rebellion, but with no idea of becoming partisans of chaos or nihilism.

Bakunin is undoubtedly the archtypical anarchist, and nowhere better than in his writings (and life) is this ambivalence of anarchism displayed. Sensitive, imaginative, knowledgeable and intelligent, he was still no systematic thinker. One must say that it is the man, not a theory, which provides the unity in his writings, and they remain powerful despite the ease with which analysis can find confusions and contradictions. Marx must have found Bakunin exceptionally frustrating, even more than he did Proudhon, whose idea of the nature and role of science was much closer to his own.

Bakunin's basic perspective is stated quite simply in his *God and the State* (p. 12; Lehning, p. 114). Human beings are animals capable of thought and rebellion. Human animality manifests itself in the social and private economy, thought in science, and rebellion in liberty. None of these capacities or conditions derives in any significant sense from any higher power or serves such. They are simply natural characteristics of matter evolved to the human level; as such they can and should be left to express themselves and develop as they will. Religious and political idealism is a perversion of nature.

A systematic thinker, given such a complex figure, would try to reduce it, to clarify the elements and identify determinate relationships among them, to obtain a coherent picture and guide. So did even Proudhon and Kropotkin, to say nothing of Marx and others, but not Bakunin. To him, reason gives rise only to abstractions, which must not be confused with realities or substituted for life. At their best, in modern science, those abstractions are "true abstractions," which can inform us of the "general causes of individual suffering" and the "general conditions necessary to the real emancipation of the individuals living in society" (pp. 60 ff.). But even such reason possesses no authority, does not determine any particular condition or individual actor. These must therefore be determined by something else, one might think—perhaps nonrational animality, or the spirit of rebellion—but Bakunin was consistent and did not press his analysis to this point.

Again, to Bakunin liberal individualism is abstract and unrealistic, and inevitably leads to the limitation of liberty by authority. He observed very nicely against the liberal acceptance of such restrictions that "liberty is indivisible; one cannot curtail a part of it without killing all of it. This little part you are curtailing is the very essence of my liberty; it is all of it" (Dolgoff, p. 129; Lehning, p. 138). Further, "man realizes his individual freedom as well as his personality only through the individuals who surround him, and thanks only to the labor and the collective power of society" (Dolgoff, p. 236; Lehning, p. 145). A few pages later, however, we learn that it is not only against the state, that artificial imposition on society, that people must rebel, but even against society itself, whose "tyranny is gentler, more insidious, more imperceptible, but not less powerful and pervasive than is the authority of the State." Now, where can the individual find the resources to rebel against a society of which one, even one's liberty, is a product? Bakunin did not say. He admitted that it is "difficult."

Many examples of such theoretical difficulties could be produced; Bakunin simply stands astride them, and it may be in this that we have most to learn from him. Political science has produced many, far neater theoretical constructions, but we are as far as ever from understanding politics. Perhaps Bakunin was right, and our conceptualizations—even those of our most empirical science—are but abstractions, strictly valid only in their own special contexts, and producing only a variety of limited insights in relation to real life. Our ultimate challenge is not in the elaboration of such—that game is fairly easy—but in using them with imaginative flexibility and political sensitivity in our effort to understand and be of some use in the political order. Henry Kariel's writings during the last decade seem one attempt to develop such a nonauthoritarian political science, if it can be called a science, but the efforts of Bakunin to do the same, consistent if not particularly self-conscious, can still be illuminating.

Both these collections of Bakunin's writings may serve that purpose well, together with their more immediate one. Both provide a good survey of Bakunin's writings from his early nationalistic idealism through the mature and reflective anarchism of the seventies. Lehning opens with a charmingly idealistic letter to Bakunin's sisters in 1836, which is hardly anarchist but displays his contempt for the commonplace that persists in his later thought. In each are both philosophical reflections and practical revolutionary messages, conveying the range of Bakunin's interests and activities. However, Dolgoff's collection is somewhat more extensive, provides more running commentary, and has an index and a more substantial bibliography.

Fields, Factories and Workshops Tomorrow is an abridgment of Kropotkin's volume published in its second edition in 1913, and with the "tomorrow" added to the title. An unabridged reprinting by Benjamin Blom is also available. The editor has cut much now obsolete detail, and has added extensive, critical but sympathetic commentaries to each chapter to bring the whole up to date, producing a very interesting book. Kropotkin argued, in essence, that both greater productivity and a better working life would result from the integration of town and country, of agricultural and industrial production, and from a more intensive agriculture, together with more local and regional self-sufficiency. He believed that the best human life in the past was lived in the free cities of the late Middle Ages, that this was destroyed by the development of the centralized state, and that we can recover that "golden age," improved through modern technology, simply by abandoning the state and embracing anarchy.

Anarchy, to Kropotkin, is communism. In *Mutual Aid* he argued that sociable and cooperative species are superior in evolutionary survival power to individualistic ones, and that the former is humanity's true nature. Association, however, must spring from felt needs and be voluntary, and he condemned the Marxist-Leninist "dictatorship of the proletariat" and other forms of state socialism, as had Proudhon and Bakunin before him. His argument in "Modern Science and Anarchism," in *Kropotkin's Revolutionary Pamphlets,* is especially good; he insisted, among other things, that bureaucratic centralization is as irreconcilable with socialism as autocracy was with capitalism (p. 185).

An unqualified advocate of "from each according to his ability, to each according to his need," Kropotkin in the *Conquest of Bread* not only envisaged the expropriation of private property, but condemned any sort of wage-system, even those accepted by other communists, socialists, and anarchists. He thought that the diverse resources human beings need in life and work could be effectively and justly managed and distributed by the common sense and consent of all workers. People would simply refuse to work with the lazy and dishonest. Unlike Proudhon, he attributed no positive significance to the development of centralized political order and ignored modern economics, so his sociology has a rather romantic if not reactionary cast. Still, his attempt to relate anarchist communism to Darwinian evolutionary theory is illuminating, and in his fascination with technological developments that could be applied in communal industry and agriculture he presaged many "radical" proposals of recent years.

Emma Goldman's *Living My Life* reveals the mentality and personality of the anarchist better than even her own essays, although these may be the best brief and simple introduction to anarchism. Intelligent, vital, driven by an intense sympathy for all victims of oppression, this immigrant girl from Russia became the best-known symbol and accomplished spokesman for anarchy in this country at the turn of the century. Less theoretical even than Bakunin, she was simply a consistent, idealistic libertarian. Highly individualistic, she insisted that anyone should be able to live his or her own life uninhibited by legal or even social restrictions, but not irresponsibly. She knew that no one is self-made, the autonomous individual of the Renaissance, or of Hobbes, and her affections ran deep, but she also struggled constantly to preserve her independence. The resulting turmoil in her own thought and feelings, and in her relations with her fellow socialists and anarchists, can easily be taken as the central theme of her autobiography, though this was probably not her intent. It shows well the nobility and the pettiness, the strength and the fragility, of the anarchist movement and the character of its members.

Goldman's life was one of continuing and consistent protest against repressive authority and defense of those who rebelled. As might be expected, occasions of peace and achievement were few, compared with those of hostility and frustration, but she persisted, undisillusioned, through 1928, when she retreated to write her story. Lesser spirits might well have abandoned hope. She stoutly defended the Bolshevik revolution until, in personal experience, she discovered its authoritarian, repressive side. This she then publicly condemned, and those responsible for it, but not its communist element. Unhappily, she then became anathema not only to conservatives but also to communists and their idealistic supporters. Those who stood by her were few. Such has been the fate of all anarchists, beginning with Proudhon in 1848. A consistent dedication to a morally responsible freedom for everyone is not a popular cause. Yet the philosophical mind should find intriguing their resolute, but endless and inconclusive effort to manage their internal conflict between liberty and egalitarian, cooperative social order. If this is, as it may be, the central dilemma of modern life, the record of their effort may help us to live with it.

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The Social Function of Social Science. By Duncan MacRae, Jr. (New Haven: Yale University Press, 1976. Pp. 352. $15.00.)

The debate about "values" in social science has been prolonged, tedious, and inconclusive. On the one side are those who, with Max Weber and George Lundberg, hold that social-scientific inquiry should be value-neutral. Social scientists' own values should not, on this view, be permitted to "contaminate" their researches; they are instead partly social engineers whose sole concern is to compute the efficiency of the available means to ends not of their own choosing. On the other side are those who, like Gunnar Myrdal, argue that inquirers' own values figure inevitably in their researches and cannot be bracketed, set aside, or ignored. All knowledge, on this view, is necessarily perspectival; what we know, we know by virtue of having assumed a certain stance or perspective which informs our choice of subject matter, the questions we pose, etc. It therefore remains only to be self-conscious, and above all *honest*, about one's value commitments and about the ways in which they affect one's inquiries. In a *confession du foi* set off in a preface or appendix, the social scientist should make an honest declaration of his own values, biases, and interests.

The differences between these two positions are real enough. But both are alike in one important respect: each views one's choice of values as a matter of taste, faith, or personal conviction, and therefore forever closed to rational assessment, criticism, and modification. *De gustibus non disputandum.* Duncan MacRae challenges this position in *The Social Function of Social Science.* MacRae's thesis is that ethical discourse and scientific communication are not mutually exclusive but, on the contrary, complement and supplement each other. The "single polar opposition: science versus valuation," is not only mistaken in its mutual exclusivity but leads us to ask the wrong questions and to mischaracterize our enterprise. It leads us to ask, for example, whether we are scientists or ideologues—the latter being viewed as pseudoscientists whose work is contaminated by their own values or biases.

Against this conventional view MacRae argues "that valuative discourse need not bring with it all the dangers that some anticipate, *if it is conducted rationally;* it need not be subjective, emotional, or ideological in the sense of commitment to predetermined conclusions" (p. 8; my italics). MacRae's claim that valuative discourse can be conducted rationally flies in the face of the still-dominant positivist view that ethical utterances are hortatory affirmations of one's own irredeemably subjective and irrational tastes or preferences. In his third chapter MacRae provides a useful overview of philosophical positivism with special reference to its vaunted fact/value distinction. Inasmuch as it takes an idealized model of science as its standard of rationality and meaningfulness, positivism has led, as MacRae rightly notes, to a "devaluation of ethics." This devaluation has carried over into the social sciences, which have for the most part uncritically appropriated the central tenets of positivism (not only as regards "values," but also as regards quantification, formalization, and explanation via covering laws). The positivist's *bête noire*—the inquirer who contaminates inquiry with idiosyncratic values—is, however, a straw beast. For the practicing scientist is, after all, a member of a scientific community with its own norms governing scientific inquiry and communication. These norms include the requirement that hypotheses be clear, consistent, and testable, that experiments be replicable, etc. MacRae maintains that these norms may be transferred to ethical discourse, thereby rendering it rational. This requires a number of moves. The first is that proponents of differing moral persuasions specify, as clearly and consistently as they can, the "ethical hypotheses" which constitute the cores of their respective value systems. The second requirement is that the proponent be allowed to defend, and the opponent criticize and "test," these ethical hypotheses. A set of ethical hypotheses is deemed defective if its constituent propositions are mutually exclusive and/or inconsistent, if their application in hypothetical test cases leads to consequences that are at odds with the system's core values, etc. The third requirement is that defective hypotheses be modified or abandoned. Although presented by MacRae as norms of modern "scientific" communication, these norms are in fact as old as political philosophy itself (*vide* Book I of Plato's *Republic*).

Besides being constrained by the norms governing rational argument and communication, social scientists' value perspectives are shaped by their theoretical perspectives. Faced with the task of explaining the "same" phenomenon, a Marxist, a rational-choice theorist, a Freudian, and a Skinnerian behaviorist will ask different questions, employ different methods, and, not surprisingly, arrive at different conclusions. Not that any or all of them distort the facts: on the contrary, "a fact," as Easton noted, "is a particular ordering of reality in terms of a theoretical interest." That being the case, MacRae believes it imperative that we formulate, criticize, and amend the "ethical hypotheses" implicit in our theories, in accordance with the norms of scientific communication he outlines in chapter 4; and in four particularly illuminating chapters (5–8), MacRae does just that. Only in this way, he believes, can there be any hope of reconciling science and ethics, expertise and democracy.

MacRae's arguments are impressive and his erudition formidable. *The Social Function of Social Science* deserves a close and careful reading by political scientists, and in particular by those who view their science as a policy science.

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Social Justice. By David Miller. (Oxford: Clarendon Press, 1976. Pp. 367. £8.50, $19.75.)

However badly justice has fared recently in the world of political practice, philosophy has been serving it—if not well—at least expansively. David Miller now joins John Rawls and Robert Nozick in offering a major study of social justice. Unlike them, he does not take explicit sides, and instead assays to provide first a philosophical analysis, then a textual-historical survey, and finally a sociological examination of what he takes to be the three major notions of social justice: distribution based on right, distribution based on desert and distribution based on need. As a consequence, his book is more useful as taxonomy, more detachedly critical as analysis and more carefully judicious as philosophy than either Rawls or Nozick. He pays a price, however: his breadth and impartiality render his conclusions skeptical and relativistic (despite his disclaimers); and his mixture of styles (philosophical, textual and sociological) and his prudence give his arguments a certain lack of conviction (despite biases—e.g., toward egalitarianism and communitarianism—that he cannot quite sequester).

Nevertheless, *Social Justice* is a very good book—an excellent preparatory text for those who wish to take on Nozick, Rawls and other justice philosophers. Miller's analysis unfolds in three somewhat disjointed phases. The first is exclusively philosophical and is concerned with distinguishing theories of distributive justice according to rights, deserts and needs—distributive justice as a generic type having already been separated from aggregative theories focusing on aggregate public welfare rather than distribution of goods to individuals. His taxonomic and definitional discussions provide some of the most illuminating sections of the book, although these are in a sense digressions en route to demonstrating a proposition which concludes the philosophical chapters and becomes the entire book's principal argument: namely, that justice based on need, on desert and on right are three "irreducible" types that are largely incommensurable. Their irreducibility becomes more apparent when they are examined in the setting of the more general political theories and philosophies of society to which they belong—which justifies the second phase of Miller's argument. In this phase the three abstract theories of social justice are rediscovered in the political thought of three traditional political theorists. Hume is constructed as a conservative philosopher of established rights, Spencer as a radical individualist philosopher of earned deserts, and Kropotkin as an egalitarian philosopher of mutualist needs. While the three chapters comprising this phase involve Miller in a good deal of provocative, often controversial, reinterpretation of standard texts, their aim is simply to reinforce the incommensurability argument advanced in phase one. By setting abstract theories of justice into systematic political philosophies, Miller believes he can show that such abstract theories represent neither "isolated moral choices" nor positions on the "correct use of shared concepts"; rather, they reflect commitments to "a much wider view of what societies are and ought to be" (p. 245).

The arguments of the first two phases thus appear to converge in a relativistic skepticism; this is the springboard for phase three, which Miller promises, will use sociological analysis to thwart such skeptical conclusions. Phase three thus subjects Miller's three types of justice to an "exercise in interpretative sociology" where it is shown that concrete (historical) cultures produce "rational" theories of justice "appropriate" to their various social structures and value priorities (p. 255). "Primitive," "hierarchical (feudal)" and "market" societies are delineated (as ideal types, not intended to exhaust the possibilities). Primitive societies are without an explicit theory of justice (because they are without theories *tout court*), but exhibit tendencies towards aggregative justice based on public welfare, and emphasize virtues like generosity and hospitality falling outside Miller's typology. Hierarchical societies (represented exclusively by feudalism here) are rooted in established rights, albeit not completely without a concern for need. Market societies in their early manifestation are based almost entirely on individualistic theories of desert, but come in their later (twentieth-century) manifestation as "organized capitalist" societies to take increasing account of need.

Miller's historical sociology leaves need with no representative home and thus, in what is probably the least satisfactory part of his analysis, he turns sociology on its head and sets out to depict the sort of society that might produce it—using the rather idiosyncratic experience of utopian communitarianism as evidence. Although justice rooted in need and the egalitarianism that attends it would appear to be Miller's own inclination, it ends up the least-advantaged theory both because of the sociological method used and because of Miller's confused efforts on its behalf.

Yet the real point of phase three, as with the first two phases, is to give sociological credibility to the case for incommensurability between the three types. One or another of them may be shown to be internally incoherent, or "inappropriate" to a given social structure, but other than this "no single conception of justice can be preferred to any other, and . . . consequently no definite prescriptions about the justice or injustice of a policy can be made" (p. 342). What this means for Miller is that, in the absence of doctrines of historical progress (e.g., Hegel) with which he has no truck, "we shall have to start by admitting that our political theory is specific to a particular time and place, and that our concepts and arguments will only be accepted within the framework of our own culture" (p. 343). This, despite his disclaimers about skepticism and relativism.

Indeed, the more Miller compares his concepts, the less he thinks they can be compared. Phase one subjects them to philosophical scrutiny only to conclude they are irreducible and depend on their general theoretical context; phase two shows these contexts to be incommensurable—dependent on fundamentally different world-views; phase three suggests these contexts are not merely theoretically incommensurable but reflections of historical times and places across which it is impossible to move. He has defined away theoretical conflict over justice, leaving these engaged in practical struggle to fare as best they can. In his prudence, he risks much less than Rawls or Nozick; but as a result, accomplishes less. By placing himself above the contest, he has made himself useful but irrelevant, lucid but unconvincing, fair-minded but finally impotent.

Nonetheless, remembering that this is a first book (a doctoral dissertation) by a young scholar, I find it exceedingly promising. We can hope that with taxonomy in order and the territory staked out, Miller may eventually decide to lay claim to one of the properties he has explored (need seems to be his instinctive choice) and defend it with the vigor of committed political theory. While his present views may mistrust that sort of theory, his gifts suggest he will be able to practice it well—not only to the benefit of his fellow theorists, but to the benefit of those for whom justice is not a concept but (literally) a matter of life or death.

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An Essay Concerning Human Understanding. By John Locke. Ed. by Peter H. Nidditch. (Oxford: Clarendon Press, 1975. Pp. liv + 867. $38.50.)

Well-deserved were the imprecations cast by John Locke upon those unfortunate printers and publishers who brought his books into the world seriously flawed by numerous imperfections. Though he persistently revised his major works (including his famous *Essay*) four or more times during his lifetime, none of his publishers came close to meeting the standards of this "fastidious author" who labored meticulously over every detail of his writing. Publishers have done little better by him in the three centuries since Locke first let his work go into print. Rather than publishing the editions on which he had labored most, they tended to republish earlier and less adequate editions. This means that the generally available texts of most of Locke's books are now *very* inadequate.

At long last this disgraceful situation is slowly being rectified. The Clarendon edition of the works of John Locke will provide not only the first *complete* edition of Locke's writings ever published, but, even more significantly, it will also afford Locke scholars and students critically edited and unmodernized texts. The Clarendon edition has been suspiciously inaugurated by the general editor of the series, Peter H. Nidditch, who has been working for many years on this definitive edition of the *Essay*. The editor has set himself the ultimate goal of establishing a complete text that is "faithful to Locke's final intentions." He appears to have succeeded admirably in this respect, and his work is enhanced by having been beautifully printed in a one-volume edition that is perfectly designed for scholarly use. Nidditch has included a generally valuable critical apparatus. He introduces the text "with an account of preceding scholarly editions, and especially of the relevant source texts that have been *collated*" (p. 811). Additional "editorial matter, interpretative or explanatory" has been added, e.g., an "explanation of bibliographical, text-critical, and typographical terminology," along with a glossary of somewhat questionable value. No college or university library should be without this indispensable volume, and it is to be hoped that its probably success will make possible future printings at a price that will enable one to use it in the classroom.

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Hobbes on Civil Association. By Michael Oakeshott. (Berkeley: University of California Press, 1975. Pp. 162. $10.00.)

Hobbes on Civil Association is not a new contribution. Instead, it is a collection of several previously published pieces on Hobbes authored by Michael Oakeshott. The publisher has, nonetheless, performed a valuable service in bringing these articles together and making them available under one cover. The principal reason for the volume, presumably, is that the lead entry, Oakeshott's justly famous introduction to the Blackwell's edition of *Leviathan,* is now out of print.

The other inclusions in this volume are an essay on "The Moral Life in the Writings of Thomas Hobbes," previously published in *Rationalism in Politics,* a 1937 review of Leo Strauss' *The Political Philosophy of Hobbes,* and a brief broadcast talk on "Leviathan: A Myth."

The introduction to *Leviathan,* slightly revised here, has more value than the "meretricious bouyancy" that Oakeshott modestly claims for it. Besides contributing a concise statement of the basic argument of *Leviathan,* Oakeshott's introduction provides illuminating insights into Hobbes' mind and work. It is particularly good on the relationship of form and content, of style and substance, of method and material in Hobbes' thought. And it also contains some compact and elegant characterizations of the mode of inquiry of political theory—for example, that political theory seeks "to establish the connections . . . between politics and eternity," and that it seeks "the revelation of the universal predicament in the local and transitory mischief."

The principal thesis of the introduction is that the coherence of Hobbes' philosophy "lies not in an architectonic structure, but in a single 'passionate thought' that pervades its parts." This 'passionate thought' is "the continuous application of a doctrine about the nature of philosophy" (p. 16). And the conception of philosophy involved is that of reasoning, or reckoning, with causes and effects.

The history of Hobbes interpretation suggests that Hobbes' writings constitute something of a philosophical Rorschach test. What people find there is in part a function of what they bring with them. Hence the neo-Marxist Macpherson sees Hobbes as a theorist of bourgeois possessive individualism; Strauss and Warrender see Hobbes as principally a moralist; others of us tend to see Hobbes as, in R. S. Peters' phrase, the great metaphysician of motion; and Oakeshott, who has pursued an illuminating concern with "rationalism in politics," finds the central theme in Hobbes to be his methodological rationalism.

None of these interpretations, I believe, is wholly without merit. Some, of course, strike much nearer the center of Hobbes' thought than do others. Some interpretations, in contrast, seem to me profoundly "ec-centric," in the literal sense, even though not positively wrong. (I would put the "Taylor-Warrender thesis" in this category, for example.)

In general, these varying interpretations are more convincing in what they affirm about Hobbes than in what they deny about him. Hence, Macpherson's interpretation illuminates the "transitory mischief" in Hobbes, but perhaps at the risk of not fully appreciating the "universal predicament." Strauss more than Warrender, in my judgment, illuminates the mind of the moralist in Hobbes; but each of them unnecessarily obscures both the genuine elements of unity in Hobbes' thought and the impact of Hobbes' profound aspirations to be "scientific."

A similar judgment might be made in the

case of Oakeshott's interpretation. He has illuminated for us better than has anyone before him the profound impact of Hobbes' methodology upon his system of ideas. This is a contribution of permanent value. If there is a weakness in Oakeshott's interpretation, it comes from an occasional tendency to insist upon a unidirectional pattern of causal influence in the dynamics of Hobbes' thought. Thus, when he insists that "the system of Hobbes' philosophy lies in his conception of the nature of philosophical knowledge, and not in any doctrine about the world" (p. 25) Oakeshott unnecessarily oversimplifies what surely is a complex dialectic between Hobbes' epistemology and his ontology. The danger in talking about Hobbes, here as elsewhere, consists in providing a one-sided view of a many-sided thinker.

Oakeshott himself exhibits an awareness of this danger in his review of Leo Strauss' book on Hobbes, included in this volume. Oakeshott is properly appreciative of Strauss' genuine contribution to our understanding of Hobbes. However, in a careful and thoughtful analysis, he clearly points out some of the weak links in Strauss' argument. For example, Oakeshott notes that the "exaggerated consistency" which Strauss attributes to Hobbes is not really warranted by the evidence. He also notes that "it is, really, one thing to prove (as Strauss proves) that there is marked change and development towards a more and more 'scientific' theory in the *Elements of Law*, *De Cive* and *Leviathan*, but it is quite another to infer from this development the existence of an 'original' theory altogether 'non-scientific,' and it is another still to infer that this 'original' theory was never really abandoned. It is indeed certain," he continues, "that, in his latest writings, Hobbes did abandon it—Dr. Strauss himself admits as much; and it is a lapse from the scrupulous attention that Dr. Strauss usually pays to the smallest movement in Hobbes' intellectual history to suppose that this abandonment was unintentional and not the real Hobbes" (p. 141).

Had he access to more recent discoveries about Hobbes' intellectual biography, Oakeshott might have had further criticisms to make—though, as he rightly notes, biographical evidence by itself should not be considered determinative. Nonetheless, his basic conclusion is, I believe, still valid today: "Hobbes's theory may not be of the *simple* 'naturalistic' character that it has often been supposed to be, but neither is it of the *simple* 'moral' character Dr. Strauss suggests" (p. 143, emphasis in original).

In his essay on "The Moral Life in the Writings of Thomas Hobbes," Oakeshott gives us a careful critical consideration of several leading views about the nature and grounds of moral obligation in Hobbes' theory. And he suggests that discrepancies in Hobbes' writings on this issue may well arise from "artful equivocation" rather than from "mere confusion" (p. 118). Hobbes, he suggests, had two doctrines of obligation, one for the initiated and one for the ordinary person who demanded a more traditional vocabulary. As Oakeshott acknowledges, this account of the internal inconsistencies in Hobbes' thoughts on obligation "in the nature of the case . . . cannot be demonstrated to be true" (p. 119). It is, nonetheless, an account which I find persuasive.

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Komputery a polityka. By Longin Pastusiak. (Warsaw: Wiedza Powszechna, 1975. Pp. 248. 32 zloty.)

Komputery a polityka (Computers and Politics) introduces the Polish reader to recent developments in both international relations theory and the use of similation and other quantitative research techniques in the study of international relations. Longin Pastusiak, the Director of the North American section of the Polish Institute of International Affairs, a subsidiary of the Ministry of Foreign Affairs, is well known among American students of international relations. During the past two decades he has spent four academic years in the United States and in recent years he has participated in the annual meetings of the International Studies Association. Pastusiak has written voluminously, primarily on U.S. diplomatic history and contemporary foreign policy, and his recent work has interested him in the evolution of international relations theory in the U.S., as well as in the use of quantitative methods for the analysis of international affairs.

Komputery a polityka consists of four articles related only by the fact that they treat the development of international relations theory in some way. In the first chapter Pastusiak argues for the development in Poland of a science of international relations founded on clearer conceptualization and a stronger empirical base. In the course of his argument he surveys the major developments in international relations theory in the West, primarily the United States. Here, as in the other chapters, he makes appropriate references to the Marxist classics and criticizes U.S. theorists for their failure to recognize the validity of Marxism as a basis upon which theories of international relations can and must be founded. In spite of his criticism of American efforts to develop theoretical models for the study of international relations, Pastusiak does present a clear and fair—and usually sympathetic—picture of the development of international relations theory in the United States.

"Computers and International Relations," the second chapter of the book, presents a detailed but elementary discussion of the use of computers in the study of international relations. It begins with an extended and largely irrelevant survey of the dominant international position of the U.S. computer industry and a description of the functions of the computer. The author then describes the uses of computers in international relations research with major emphasis on the development of data banks and information retrieval systems. Pastusiak again provides an accurate description of the use of computers in the U.S., not only by researchers, but also by various government agencies, including the Department of State, and the major television networks in their coverage of elections.

The third chapter, entitled "Simulation Methods," details the major simulations that have been developed for both teaching and research purposes in the area of international relations. The Inter-Nation Simulation, TEMPER, the World Politics Simulation, etc., are all given extensive coverage. Pastusiak concludes this chapter by noting that, although simulation as it has been developed in the United States permits experimentation with theoretical models of the international system, it suffers from the inaccurate philosophical assumptions of bourgeois social science.

In the final section of the book Pastusiak deals with Western literature on decision making in universal international organizations, which he criticizes for emphasizing too much the techniques and machinery of decision making without treating adequately the motives underlying the decision-making process.

On the whole, *Komputery a polityka* is a fair book. Pastusiak has presented an accurate survey of recent developments in U.S. international relations theory. Some of his criticisms of the theoretical models developed in international relations would be shared by a large number of American scholars—even by those who do not share Pastusiak's Marxist assumptions.

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Patriarchalism in Political Thought: The Authoritarian Family and Political Speculation and Attitudes Especially in Seventeenth-Century England. By Gordon J. Schochet. (New York: Basic Books, 1975. Pp. xii + 292. $12.95.)

The owl of Minerva, Gordon Schochet tells us, in effect, takes its flight when dusk is falling: the patriarchal theory of politics came to be elaborated only when the patriarchal society itself was under seige, and thus only a relatively short period elapsed between Locke's assertion that Filmerism was "the current divinity of the times" and 1705 when the Anglican Bishop William Fleetwood found that the whole of society (including the household) could be understood in terms of contractual and reciprocal relationships.

The history of patriarchal theory is traced by Schochet with diligence and skill, and in the process we hear about the ideas of a considerable number of historically significant but now little-known authors whose works have not been reprinted since their time of origin. Moreover, we are also provided with a better understanding not only of the intellectual context within which Hobbes and Locke undertook their masterly enterprises but also of those enterprises themselves, which Schochet treats in two illuminating chapters. All this constitutes a considerable service to historians and to other students of political philosophy, a service which the reviewer needs to recognize in spite of the author's somewhat exaggerated talk of how "an entire tradition of political expression" has hitherto suffered "suppression" (p. 4) because of the low esteem in which Sir Robert Filmer, that tradition's leading figure, has been held by some commentators.

While Sir Robert and his confrères may not have needed Schochet's kiss of life quite as badly as he supposes, the evidence which he presents clarifies and re-emphasizes the importance of patriarchalism in the early modern period of English history. Patriarchalism ranged from the vaguest analogy suggesting that the king could be considered as pater patriae, to an elaborate theory tracing a line of descent from Adam and the Patriarchs to the Stuarts and maintaining the position that contemporary kings derived their "natural" authority from those fathers who had, according to the Old Testament, in the first instance ruled over human society in an absolutist manner with God's blessing. Robert Mossom (one patriarchalist who apparently has escaped Schochet) summarized the more elaborate part of this tradition in a sermon of November 1642: "To apprehend the strict tie in which the subject is bound to his sovereign, we must know that kings are the successors of the Patriarchs.... The difference seems to be only this, that the Patriarchs were kings of their families, and kings are fathers of their countries. So that *jus Regium* commeth out of *jus Patrium....*" *(The King on his Throne,* p. 15).

Furthermore, being preached by the Anglican establishment, patriarchalism was almost certainly the *only* theory of politics encountered regularly by the lower orders. And Schochet shows how the impact of this theory must have been strengthened by the almost universal Stuart opinion that the Fifth Commandment referred as much (if not more) to "civil" fathers as to natural ones: thus could Lord Capel (quoted on p. 91), facing execution in 1649 for incorrigible devotion to the House of Stuart, claim that his predicament had been the result of "maintaining the fifth Commandment ... which enjoins ... obedience to parents ... [by which] most Divines do acknowledge that here is intended magistracy and order."

The most important implication of the patriarchal theory of politics was the rejection of what Schochet calls the "populist" theory, i.e., the theory according to which both the legitimacy of magistracy and the obligation to obey it were the consequence of the conveying or delegation of authority by "the people." For Filmer et al., populism was quite simply preposterous, for in their view the magistrate no more relied upon the suffrage of his subjects than the father relied upon the suffrage of his children. John Maxwell saw the matter thus in his 1644 *critique* of parliamentarian theories: "as the natural father ... receiveth not any paternal right, power or authority, from his posterity ... but hath this from God and the ordinance of nature ... no more can the father surrogated in the place and power of the natural father be said to receive his right, his power, his sovereignty, from the community" (*Sacro-Sancta Regum Magestas,* pp. 87–88). As the Parliamentarians of 1642 (and such of them as became regicides in 1649), the Levellers, the Exclusionists and the instigators of the Revolution of 1688 all relied heavily upon the "populist" theory to justify their several enterprises, the historical significance of its mortal antagonist becomes evident.

Given Schochet's terms of reference, whereby he takes himself to be describing in context a tradition of political thinking, and not providing a comprehensive analysis of any particular thinker (not even Filmer), there is little to take serious exception to in *Patriarchalism in Political Thought:* at the level of detail, an error concerning the date of the first collected edition of Filmer's works to contain *Patriarcha* (p. 120, n. 8) should be corrected in future editions. At the same time, one is still disappointed by the author's treatment of Filmer. Having worked in the field of patriarchalism for 20 years and having discovered new Filmer material (including the intriguing *Discourse Concerning Supreme Power and Common Right,* previously attributed to Sir John Monson), Schochet was uniquely placed to give a more rounded picture of Sir Robert (incoherencies and all) than we now possess. He has considerable justification in arguing (p. 150) that Filmer's imprecision and carelessness make "logical analysis" of limited value" (particularly puzzling is Filmer's suspiciously "populist" assertion regarding the "escheating" of kingdoms that "all ... prime heads and fathers have power to consent in the uniting or conferring of their fatherly right of sovereign authority on whom they please" [Laslett ed., p. 62]), but one would have welcomed a more sustained attempt to grapple with the difficulties involved.

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Ideology and Politics. By Martin Seliger. (New York: Free Press, 1976. Pp. 352. $14.95.)

Judged by its potential for stimulating scholarly discussion and the advancement of understanding, this is one of the best systematic English works on ideology to appear in recent years. Anyone seriously interested in the subject must read it, if only to argue, for it offers intelligent (often surprising) observations, an uncommon breadth of discussion and exhaustive research.

This is not, however, an easy volume to read. The literature and topics addressed are treated in detail, and a previous background in the subject is required of the reader. Moreover, the terminology is exasperatingly specialized and the style is prolix.

Martin Seliger criticizes the "restrictive" conception of ideology and defends an "inclusive" conception. These terms seem odd. To be successful, concepts must "restrict" the range of phenomena to which they refer and may not "include" too much. At any rate, Seliger mainly opposes criteria which portray ideologies as either being distorted, or as favoring radical change to the exclusion of pragmatic, prudential and moderate politics, or both.

Much of his argument against these conceptions, I believe, is valid and original. I question, however, his apparent extension of the "restrictive" paradigm to conceptions which view ideologies, in part, as being concerned with (but not necessarily in favor of) political change (p. 61). Seliger himself observes that "all political and social conflict centres around change" (p. 91). Moreover, the recurrent suggestion that error and inconsistency would attend any attempt to "restrict" the concept of ideology is doubtful. And, so is the assumption that by debunking certain excessive versions of the "restrictive" approaches the necessity for the "inclusive" conception has been demonstrated (e.g., p. 120).

In elaborating the "inclusive" conception (chapter 3), Seliger develops a number of distinctions. First, ideologies have a "positional" dimension, and (against some advocates of the "restrictive" approach) ideologies should therefore not be categorized by "ideational" content alone. For example, in concrete situations "conservatism" may take a "radical position concerning the restoration of the past, and "Marxism" may favor the status quo. According to "positional" criteria, therefore, any ideology may be, depending on situational response, "extremist, moderate or conservative" (p. 92). (This usage, incidentally, leaves "conservative" dangling between positional attitude and political doctrine. Why not substitute the term "preservationist" for "positional conservative"?)

Second, ideologies are applied in political action. Seliger therefore discusses the interaction of "fundamental" and "operative" ideology–of "fundamental principles, which determine the final goals and the grand vistas, . . . and . . . the principles which actually underlie policies and are invoked to justify them" (p. 109).

Third, Seliger argues that "fundamental" ideology is distinct from philosophy yet derives its basic principles from philosophy. He therefore distinguishes between "ideological" principles and "meta-ideological" principles (or "philosophical meta-fundamentals"). Following Brian Barry, Seliger holds that philosophy concerns " 'ultimate considerations' referring to justice and freedom which are so 'ultimate' that they cannot be associated with anybody in particular or with any policy; yet we can deal with them not abstractly but in the manner in which they 'weigh on people' " (p. 112). It is the point at which they "weigh on people" by being "particularized" in political policies that the "ultimate considerations" become ideological.

Fourth, there is the "core" definition of ideology. All ideologies, it is argued, have a "formal content." These are: "description," "analysis," "moral prescriptions," "technical prescriptions," "implements, i.e., the ways and means of implementation," and "rejections" or elements to which the ideology is opposed (p. 106). To make a long story short, these components might also be found in nonideological belief systems or in "merely practised belief systems" (p. 107). They are ideology because of two "overall characteristics": they "directly guide organized political action or analyze it for the sake of guiding it," and they comprise "a system of beliefs" (p. 96).

By definition, "ideology" therefore seems coterminous with "political belief system" though, apparently, "merely practised belief systems" are not "systems" in the ideological sense because they lack "rational presentation and justification or logical coherence" (p. 107). Moreover, Seliger asserts that "ideology" is coextensive with politics: "Strictly speaking, politics can become entirely unideological only if they become completely incoherent." In the absence of coherence, he adds, the politics of "party" or "polity" are unlikely to persist, and, therefore, "politics will remain ideological" (p. 104). This conception of ideology is "inclusive," no doubt about it!

One may ask, however, whether ideology is actually so widespread as Seliger's conception implies. Is there no politics of "technique" where moral evaluations (hence ideology) are absent or unselfconscious (p. 109)? Is there no nonideological politics of "drift" (p. 105)? Is there no politics of pure action (Sorel, Jerry Rubin's "Do It"!)? Is "disjointed incrementalism" not politics, or is it "jointed" in an ideological way (p. 111)? How does one account for politics by other cultural means such as myths? Consider, for example, ancient Egypt and Mesopotamia, the Trobriand Islanders, what Seliger calls "tribal societies," the mythical component in the politics of the Third Reich, in contemporary America and in all the classic ideologies. (See pp. 96–97 and n. 19 on p. 97.) Aren't the "positional" and "operative" dimensions of ideologies sometimes (not always) "attitudes" or "merely practised belief systems"? And, in these cases, can one talk about ideological politics intelligibly?

The answers to these and similar issues will pertain to how one evaluates the admirable (even if partially incorrect) comparative treatment of ideologies and ideological change in the book's later chapters. In particular, it will bear on the debatable strategy of "averaging" the "operative" and "fundamental" dimensions of ideologies for purposes of ranking and comparison.

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Capitalism, Socialism and the Environment. By Hugh Stretton. (New York: Cambridge University Press, 1976. Pp. vi + 332. $19.95, cloth; $6.95, paper.)

"Material equalities may not by themselves generate much brotherhood, but they are a necessary condition of it. They may soon be a condition of government by consent: the only workable basis left for social arrangements between political equals" (p. 313). The last sentence in Hugh Stretton's book thus draws what one may take as an optimistic or an agonizing conclusion about the future of advanced industrial societies: either a significant equalization of material goods and life chances will be demanded, and produced, by forces generated out of the process of political democratization itself, or the resistance to greater social equality will be so great as to destroy political democracy where it has come to exist. Both solutions are quite possible, Stretton tells us, and his book is a new social democratic primer on how to achieve the former rather than the latter.

Stretton's thesis is that "conflicts about inflation, inequality and scarce resources are parts of one central problem of democratic distribution," that the present troubles are neither a crisis of capitalism nor a crisis of democracy as such, but rather "a confrontation (at long last) between democratic distributions of power and capitalist distributions of goods." We are living a "sea-change in the capacities of democracy itself," having entered an epoch of "new freedom to choose between diverse social systems and futures" (p. 1), if electorates would only become aware of it.

The book is divided into three parts. The first is a set of three scenarios—supposing conservative, liberal and social democratic values—which forecast the major likely patterns of society to issue from the growing conflicts mentioned above. Those saturated with the often vacuous futurologies of recent years will be impressed by the range and depth of Stretton's constructions, built on his earlier books, *The Political Sciences* (1969), *Ideas for Australian Cities* (1970), and *Housing and Government* (1974). Part 2 discusses the relative merits of different directions of change, focusing on the complex relations between various equalities and various liberties, and ending with an important discussion of the home and the "domestic economy" in general, considered as a productive sector coequal with the "institutional economy" (the production of public goods and services within private, public and mixed forms of ownership). Echoing a central theme of Michael Young and Peter Wilmott's *The Symmetrical Family* (1973), Stretton concludes: "The distinguishing purpose of socialism should not be to reduce private and increase public ownership. It should be to tame institutional and increase domestic ownership: to attack inequality and alienation by giving to every household (as far as scarcity and environmental prudence allow) the fullest control of the most versatile resources it can use" (p. 206). In part 3, a brilliant exposition of certain major principles of a social democratic program, Stretton therefore debates services versus direct money grants, shares of space (housing and land markets) with special reference to cities, shares of wealth and income generally, and the problems of inflation, wage-price policies and overabundant group bargaining leverage in all of this. Finally, since "an equalizing society has to be productive," he attempts to show how such a society could "organize and motivate production as efficiently as we do now" (p. 281).

In contrast to our too often rigidified or simply inane conceptions of Right and Left, especially in Europe, Hugh Stretton's book is impressive in its disengagement from stereotypes and sacred cows. For example, "I assume that the Left need no longer bother to abolish capitalism. Capitalists, and the mechanisms of inflation, are doing that. . ." (p. 135). "'Democratic bureaucracy' is inevitable" (p. 135); i.e., "public control is inevitable. But its methods and purposes are wide open to choice" (p. 1). "Communist socialism may prompt some thoughts about democratic socialism, but it is still the biggest single hindrance to achieving democratic socialism" (p. 158). And, in explanation of the book's title, "there are equal and unequal ways of conserving resources. There are also equal and unequal ways of squandering them. So there is no inevitable general relationship between the Left and the environmental movement. Instead there is a tangle of common, overlapping and conflicting interests, and plenty of opportunity for negotiation" (p. 10).

Adopting a common-sense usage of "Right" and "Left" as terms indicating not particular forms of government but rather attitudes toward equality (Right wants existing or greater inequalities, Left wants to reduce them), Stretton argues that equalization is essentially a political rather than a technical problem, and that the Left has so far failed because "it doesn't yet know what to do about the problems and too many voters know that it doesn't" (p. 137). Thus, historically "the Right has deserved its victories and the Left its defeats. But the suffering citizens haven't really deserved either. Wrong options face them at election time" (p. 205), most importantly the left-wing error which has identified "socialism or equality, in a doctrinaire way, with exclusive public ownership" (p. 277).

The recent political experience of several countries in western Europe—above all the debate within and about the French Left—attests to the power of Stretton's arguments. His conclusions imply a necessarily heterogeneous and shifting set of alliances among groups seeking an organic transition to a regime of greater equality. Stretton's central concern, however, is something beyond equality for its own sake: "The purpose of equality is not to abolish differentiation—of pay or anything else—but to free people from its anxious and punishing and corrupting effects so that they can enjoy its liberating, individualist and sociable possibilities" (p. 280). The book's combination of moral commitment, political pragmatism and technical competence merits a wide audience. Students of public policy, of advanced industrial politics, and even politicians themselves, have much to learn here.

RONALD TIERSKY

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Friedrich Nietzsche and the Politics of Transfiguration. By Tracy B. Strong. (Berkeley: University of California Press, 1975. Pp. xiii + 357. Price not given.)

Tracy Strong's book on Nietzsche attempts to cover the central themes in Nietzsche's work, beginning with a discussion of truth and nihilism, proceeding through a discussion of morality, the Greeks and politics, and culminating in a discussion of will to power and the doctrine of eternal return. Because Strong distrusts both the usual periodizations of Nietzsche's works and the efficacy of chronological textual analysis, he roams back and forth among passages selected from Nietzsche's writings. And because he knows that Nietzsche has no ordinary "system," he attempts to make manifest a whole in Nietzsche similar to the coherence which comprises a particular city in a particular time and place. The effect of this approach is to make the book appear to be a series of false starts, with little development even in the discussion of passages cited two or three times. It is difficult to say whether the fault for this appearance lies with the author, the reader, or Nietzsche himself, because Strong's interpretative principles make it difficult to separate the matter being analyzed from the one who analyzes it.

But one might say that this difficulty emerges in Nietzsche's own writings. Indeed, one of the strengths of Strong's book is his attempt to take Nietzsche seriously, and this requires that one give credence to Nietzsche's claims about what differentiates him from previous thinkers, even his most extravagant claims. Consequently, Strong's effort to illuminate both Nietzsche and his own interpretative principles through comparisons with Wittgenstein and other twentieth-century figures is occasionally stimulating. The second strength of the book also stems from Strong's attempt to take Nietzsche seriously. This is his effort to deal with the highest themes in Nietzsche, paying little attention to issues which are important only because of the past or current state of intellectual history, political science, or academic philosophy. This keeps the book on a high level even when Strong's substantive remarks and stylistic mannerisms fall short. The third strength of the book comes from the several useful points Strong makes about particular topics, among them the following: Nietzsche's understanding of the Greeks, Nietzsche's understanding of Jesus, Nietzsche's understanding of genealogy. Though Strong does not fully illuminate these topics, leaving the precise relation of 'genealogical' and 'ontological' principles in the dark, for example, and though his best remarks often come when, against himself, he succumbs to the lure of following an individual text closely, his understanding of these important issues is far more sophisticated than that in almost all the secondary literature.

The main specific problems with the book are these. First, although Strong speaks of taking Nietzsche's harshest remarks at face value, he does not do so in fact. The importance of race is noted, but a full account of Nietzsche on race is not given. More, Nietzsche's notorious remarks on women are ignored, and Strong will often speak of men and women where Nietzsche would speak only of men. If Strong has a coherent account of Nietzsche's views on women which would save them from vulgar misrepresentation, he should offer it. Further, though Strong discusses Nietzsche's unmasking of democracy, his own account of Nietzsche's understanding of the possibilities for future "humanity" is too democratic. That is, the place of rank order in this future, the place of tyranny in this future, and the rarity of "philosophers of the future" are neither stressed nor developed; some of these topics are hardly discussed. Second, Strong's interpretations of power and eternal return are inadequate, though sometimes insightful. He dismisses the importance of overcoming and self overcoming too hastily, fails to elaborate the concrete dynamics of power in the self and commonwealth, ignores the connection between power and affirmation, fails to stress the connection between power and eternal return in "love of fate," and discusses eternal return's historical meaning too curtly. Third, Strong's discussion of the will to truth does not resolve, nor even satisfactorily elaborate, the issue of the permanent, or nonrelativistic status of Nietzsche's own claims to truth. More, he fails to connect the will to truth and questioning with will to power and eternal return, fails to clarify the relation between truth and nature, and fails to illuminate Nietzsche's problem of the "value" of truth for life.

These problems do not eradicate the finer points in Strong's book; nor does his penchant for phrases and examples which he senses, or should sense to be inappropriate detract from its finer points. Whatever its flaws, the book can be read with enjoyment by all those interested in political philosophy, and if Strong has detractors, this book is not as bad as they might have wished. But it is also not as good as his admirers might have hoped.

MARK BLITZ

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John Stuart Mill and Representative Government. By Dennis F. Thompson. (Princeton, N.J.: Princeton University Press, 1976. Pp. vii + 241. $13.50.)

Focusing primarily on *Considerations on Representative Government,* Dennis Thompson explores one of the major tensions of John Stuart Mill's more general theory of democratic government. The tension, simply put, comes to this: Mill was an ardent advocate of those basic values which we have come to identify with democratic government. His system is calculated to cultivate wide-scale citizen participation in governmental affairs, particularly in political associations and the operations of local governments; the representation and articulation of diverse interests in the highest councils of decision making; and, *inter alia,* an active, informed, and self-reliant citizenry capable of exercising and defending its liberties in an effective and rational manner. Yet, despite his spirited defense of popular participation, Mill saw a compelling need to incorporate into his system at various points a respect for a high degree of competence, defined in both intellectual and moral terms, to guard against "general ignorance and incapacity" and rule by interests inimical to the "general welfare." Thus, Mill sought to structure a representative assembly to insure that a decisive balance would be held by those "who are governed by higher considerations." But, for Mill, even this was not enough: the proper functions of the representative assembly were not those of administration or even law-making—functions, he believed, best handled by those more competent—but rather, debate and deliberation, principally on proposals submitted to the assembly for approval or rejection by its intellectual and moral superiors.

This, in essence, is the background of an ongoing intellectual dispute about whether Mill should properly be regarded as an elitist (albeit with certain democratic trappings) or as a democrat.

Thompson approaches this issue by analyzing four major aspects of Mill's system, the principles of participation and competence and theories of government and development. In this endeavor, he treats Mill's *Representative Government* as a "connected exposition" of democratic theory that is the outgrowth of two general criteria for good government set forth by Mill: namely, the degree to which government is able to utilize and augment the "good qualities" of the citizenry.

In Thompson's view, there are "protective" and "educative" arguments associated with both the principles of participation and competence. As far as participation is concerned, it is protective in that each major interest is intended to "have a voice in the political process" (p. 176), and educative because it helps to cultivate a citizenry with a civic morality necessary to maintain and improve the regime. Competence is designed to allow qualified leaders to protect the government and public against "instrumental and moral incompetence" which could easily prove disastrous. But the role of such leaders goes far beyond this; in Mill's scheme, they are also to be "public educators" who should "strive to strengthen the civic capacities of all citizens" (p. 176). In this important respect, then, a symbiotic relationship exists between the principles of participation and competence.

In structuring his political institutions, according to Thompson, Mill intended "to give the principles [participation and competence] equivalent scope for being realized" (p. 176), thereby allowing wide latitude for their application and fulfillment in practice. To be sure, Mill's theory of development stresses egalitarian trends which led him to emphasize the principle of competence in order to "counteract the levelling of intelligence and the growth of conformity in mass society" (p. 177). But, as Thompson also notes, this trend within the context of Mill's theory could just as well serve to increase "the quality and extent of citizen activity" and, simultaneously, advance the principle of competence "by increasing the number of citizens who choose to develop their higher faculties" (p. 177). Viewed in this perspective, Mill's "theory of government seeks an accommodation between the principles" of participation and competence, while his theory of development "points the way toward a gradual attenuation of the tensions between" them (p. 177).

In the last analysis, Thompson concludes, Mill provides us with no clear-cut answer concerning which of these principles should be accorded priority. However, the logic and thrust of his theory would strongly suggest that participation should be given precedence. In Rawlsian terms, Thompson formulates a priority rule which seems thoroughly consonant with Mill's approach: "greater competence in leadership is acceptable to the extent that it also tends to increase opportunities for participation" (pp. 198–99).

Thompson notes throughout the more recent studies which have a bearing upon Mill's theory of democracy, as well as developments which represent significant deviations from its prescriptions such as greater centralization and the structure and practices of modern political parties. Although Mill's *Representative Government* is certainly not free of major difficulties, not the least of which is its theory of development, modern students of democratic government, Thompson contends, would do well to take its teachings to heart: "Mill not only anticipates many of the recent criticisms of contemporary democratic theory but, more importantly, creates a theoretical structure that comprehends those criticisms as well as crucial values that the critics themselves overlook" (p. 201).

In this assessment, Thompson is quite correct. We can only hope that his work will lead to a renewed interest on the part of modern theorists in the critical issues posed by *Representative Government.*

GEORGE W. CAREY

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Political Terrorism. By Paul Wilkinson. (New York: Halstead Press, 1975. Pp. 160. $11.95.)

"Terrorism" exerts peculiar fascinations; we seem not so much interested in it as compelled by it. Rarely is it studied until it erupts. Studies rarely have done more than catalogue its macabre episodes: assassination and kidnapping (Max Nomad), street slaughter and incindiarism (C. D. Burns), insults, rotten eggs, broken windows, beatings (G. Brennan), political judges (B. Adams), pure intimidation (J. Joesten), intimidation of the word (Selznick), sheer fear (Ferrero), open violence (Pye), anarchic violence (L. Proal), nihilistic killing (R. Payne). And the few grand analyses (Merleau-Ponty, Camus, Fanon, Arendt) often seem far too grandiose. For students of politics, terrorism is no one thing. Indeed, it provokes conflicting intimations—social morbidity and superabundant energy; deep forces and ephemeral byplay; Weber's "peculiar means" of politics, at the very boundaries of the political.

Paul Wilkinson's book is also prompted by events. (Wilkinson is a British academic, preoccupied by the long Irish agony, and he candidly tells us one of his aims is identifying countermeasures to terrorist attacks on liberal democracies.) Over half of his book is devoted to specific episodes: IRA, EOKA, FLN, Mau Mau, Tupamaros; Czarist Russia, Macedonia, Third Republic France, Malaya, Viet Nam, Palestine then and now. And multiplication of cases brings multiplication of problems: some blurred categories, and some lacunae. (Why are the Jacobins treated under "righteous assassins," rather than under opponents to indigenous autocracy? Why are the Continental Jacobins chosen as the prototype of terrorism, without a word about the very English Civil War? Is Northern Ireland really an example of "liberal democracy"? Bienan's 1968 survey, *Violence and Social Change,* Yablonsky's *The Violent Gang,* and Camus' *Rebel,* are not mentioned. Wilkinson's comment on p. 133 linking Camus with Sartre suggests he simply misses Camus' contribution; and much the same seems true of Merleau-Ponty, Sorel, and Arendt's *On Violence,* all invoked but not explored.)

But Wilkinson's study does attempt a real conceptual grasp. Wilkinson builds on the three best previous treatments, by Brian Crozier (1960), Thomas Thronton (1964), and E. V. Walter (1969), to note the inadequacy of simple dichotomies of rulers/ruled, or elimination/propaganda. He draws on the "older tradition of statecraft and wisdom" (p. 125), from medieval tyrannicide through Machiavelli and Hobbes, to distinguish terrorism from civil violence generally. He aptly identifies terror*ism* as sustained, programmatic, ideological, and organized. He focuses on revolutionary terrorism, distinguishing it from "subrevolutionary," repressive, criminal, war, and purely psychic terrorism. Above all, he focuses on a fatal ambiguity (so clear in Brinton's 1938 classic, *The Anatomy of Revolution*, which Wilkinson oddly omits): in a "reign of terror," what really reigns–political leaders, or terrorism itself? That terrorism is a systematic effort to instil terror, that terror is a subjective attitude which varies with individuals' states and situations–this, Wilkinson concludes, "makes nonsense of the claims . . . that terrorism can be a rational, selective, discriminate political weapon of real precision" (p. 12).

Unhappily, matters are not so simple. Wilkinson's own modest list of liberal democratic countermeasures suggests this. Don't be seen to give in; convince the population it can be protected; combine security with social reform; above all, isolate the terrorists, don't alienate the population by the countermeasures used–this is admittedly "a strategy fraught with risks," demanding great skill and patience, "an almost super-human task" (p. 138). What Wilkinson misses here is the bearing on terrorism. He stresses that terrorism must "appear entirely unpredictable and arbitrary to the society," that it is "only 'selective' and 'predictable' according to the . . . [terrorists'] own idiosyncratic code. . ." (p. 14). But he ignores the central dilemma of terrorism, the dilemma of the target: if it is (or appears) wholly indiscriminate, it risks alienating potential supporters; if it is clearly discriminating, it risks losing impact and impetus. Like bizarre mirrors, government and terrorists reflect each others' plight. Unpredictability accelerates and polity itself wavers between two poles. Rational, selective, discriminate political weapons are hard to find anywhere.

Here the so-called "Document on Terror" of 1952, whatever its provenance, offers some critical supplementary themes. So does the recent literature on the meaning of *civil* disobedience. But at root, the problems are endemic to the subject. Wilkinson's is a useful contribution, probably the best introduction we now have. And as with all good introductions, it serves to suggest some further connections to be made.

R. L. NICHOLS

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Bargaining: Formal Theories of Negotiation. Edited by Oran R. Young. (Urbana: University of Illinois Press, 1975. Pp. viii + 413. $15.00.)

In this book Young examines formal models ("deductive theories") of bargaining, defined as "a means by which two or more purposive actors arrive at specific outcomes in situations in which: (1) the choices of the actors will determine the allocation of some value(s), (2) the outcome for each participant is a function of the behavior of the other(s), and (3) the outcome is achieved through negotiations between or among the players" (p. 5). While much of this type of work has often been interesting only to specialists, *Bargaining* is of much wider appeal and usefulness. To be sure, specialists will appreciate finding the major works in the field collected in a single volume and will be stimulated by Young's insightful and provocative contributions. Other readers will appreciate the book for providing both an enlightening overview of this field and rich conceptualizations useful for anyone concerned with problems of strategic interaction.

In part this wider appeal stems from Young's orientation. He examines these models not as purely logical constructs nor as prescriptive theories. Instead, he is concerned with their potential "as 'descriptive' or 'positive' theory," with their utility for helping "with problems of explanation and prediction" (p. v). The insights to be obtained from these models, whatever their authors' original foci of concern, are thereby made accessible to researchers with widely different substantive interests.

From this perspective Young set out to take stock of the work in this field, to clarify several conceptual problems, and to evaluate prospects for further theoretical work. In each task he has succeeded admirably. With respect to taking stock and evaluating prospects, he concludes that "there is a considerable gap between the existing models of bargaining and reality in many real-world situations," and that therefore "the principal value of these models lies in the insights and conceptual stimulation which they unquestionably generate rather than in the specific predictions and explanations that can be derived from them" (p. 408). Generally, however, he seems fairly optimistic that future work on such models can produce fruitful "positive theory."

The book's outstanding merit, however, is the conceptual clarification Young provides. A reader perusing this literature cannot have been unimpressed with the complex diversity of formal bargaining theories. Young's introductory chapter lucidly describes the source of this diversity as being alternative efforts to resolve a set of conceptual problems that confront any effort to analyze bargaining. This analysis provides the basis for classifying bargaining models in terms of three approaches: game-theoretic, economic, and manipulative. Selections in the book are organized according to these categories (plus one dealing with the prospects for a unified theory), and represent the major work of each type. Included is work by Bishop, Coddington, Cross, Ellsberg, Harsanyi, Nash, Nicholson, Pen, Schelling, Wagner, and Zeuthen.

Such a conceptually based scheme of organization makes this an unusually coherent work. The reader can readily compare alternative approaches as well as different specific models. Young facilitates this comparison with lucid introductions to each section in which he provides a concise analysis of each approach's usefulness and limitations with respect to bargaining problems in their overall complexity.

My criticism of this book is grossly unfair: Young did not go beyond fulfilling the ambitious tasks that he set out to accomplish. His conceptualization is so rich, however, and his critiques so pointed, that one wishes he had done so. In particular, he demonstrates in various ways that all of these analyses have had to devise means of restricting the possibilities of strategic interaction. However, "strategic interaction is a central feature of all situations involving bargaining, no matter how the phenomenon of bargaining is conceptualized in detail" (p. 5). An additional chapter placing these bargaining models in the context of other approaches to strategic interaction problems would have been very useful.

Again, this criticism is unfair. Young's conceptual contributions are already highly generative, and the book fulfills an important task quite well. Its contents overall are readily accessible to readers without advanced mathematical skills, although knowledge of basic microeconomic and game-theoretic techniques will facilitate a reading of some of the selections.

ROBERT LYLE BUTTERWORTH

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American Politics

Who Pays the Property Tax; A New View. By Henry J. Aaron. (Washington, D.C.: The Brookings Institution, 1975. Pp. 110. $5.95, cloth; $2.50, paper.)

The usually accepted view of the property tax as a regressive excise tax, necessarily burdensome for low income groups, is questioned by the theory and analysis of a growing number of economists. Henry J. Aaron provides a finely tuned analysis of why the property tax should be recognized as a tax on capital and concludes the tax is generally progressive in its burden. "Since the ownership of capital, or net worth, is progressively distributed with respect to income, the property tax on balance is a progressive, not a regressive, tax" (p. 93). He asserts, further, that the questionable analytical practice of relating home property taxes to *annual* household income reinforces the false impression of its regressive character. (He adds that almost any tax may be made to appear regressive if income for any single year is the measure.)

Aaron recognizes that the general analytical concepts on which he bases his theory of the progressiveness of the property tax are eroded with the present widespread property exemptions, fragmented administration, and the recent tendency of state legislatures to enact circuit breakers, etc., for elderly and low-income persons. The property tax will show an increasingly progressive net burden, according to Aaron, as we remove exemptions, increase the size of our assessing and taxing districts, eliminate property tax deductions from progressive federal and state personal income taxes, and discontinue state "circuit-breakers" and other rebates or refunds. With these changes, and shifting to some type of income averaging from our present annual one for analytical purposes, would bring theory and empirical studies into closer alignment.

The author seeks not just to save the property tax—which continues to be the easiest general tax for local governments to administer—but to make our whole tax system progressive in its overall effects. The growing importance of the regressive payroll tax and the lessening importance of progressive corporation income taxes at both national and state levels, he believes, makes it increasingly important that the present progressive elements of the property tax be strengthened.

Economic critics of theoretical analyses such as Aaron's, emphasize that the property tax today, and increasingly with every legislative session, is riddled with so many exceptions and its administration so fragmented that only an empirical study of specific states and their communities can tell us who pays the property tax. Since Aaron would improve the property tax in many of the ways with which critics such as Dick Netzer (see "The Incidence of the Property Tax Revisited," *National Tax Journal,* 26 [December 1973]) find fault, much of the economic theory disagreement dissolves into a difference in evaluation of political realities.

Governors and legislators in most states, at least since the late 1960s, have cooperated or competed to give property taxpayers relief through (1) increasing the types and amount of property exempt from the tax; (2) providing property tax refunds, rebates, relief for elderly and low income persons ("low income" in this context goes up to $15,000 in Oregon); or otherwise (3) shifting the revenue emphasis away from the property tax with little concern for who will bear the burden under the changed system. "Any tax is better than the property tax" is the view of many.

The Advisory Commission on Intergovernmental Relations, after urging reduced dependence on the property tax, has regularly reported and praised these efforts; and, hence, pushed the political realities even more in Dick Netzer's corner. The one reform that would aid Raymond Aaron's vision of the property tax is a growing emphasis on larger assessment districts—the county rather than the town, village, or city.

If today's economists have reached substantial agreement on the progressive incidence and burden of a general property tax, how can they and we stop the undermining of a potentially valuable progressive tax? The lag between publication of Keynes' general theory and enactment of the Full Employment Act of 1946 was slightly more than a decade. Economists have already debated property tax incidence for many more years, and yet the national and state political frameworks for discussion of the property tax as essentially a regressive tax remain almost unchallenged. Why? Do supporters of progressive taxation and opponents of the property tax subscribe to different tax incidence theories? Or is the agreement merely a matter of some immediate convenience or issue? Can we distinguish the political and social characteristics of leaders or of the states and nation in 1946 when Keynesian economics was readily written into law from the situation today when leaders, supportive of the preogressive tax principle, have failed even to retard the erosion of the property tax in recent decades?

CLARA PENNIMAN

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Generational Change in American Politics. By Paul R. Abramson. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. xvi + 140. $13.50.)

In the 1960s it seemed as if American society came to the rather sudden realization that generational differences might sometimes be as politically significant as economic, regional or racial differences. Social scientists lagged only slightly behind in this realization; Paul Abramson's book is one result of this relatively recent concern.

The book's major points have to do with the substantial contribution of postwar generations to the decline of class voting and to the erosion of party loyalties within the American public. As such, much of the book revolves around the concepts of "class" and "generation," and it is in examining these concepts, at both the theoretical and operational levels, that we see most clearly the weaknesses of Abramson's book.

"Class" is first mentioned on p. 2, "class voting" on p. 7; neither is ever defined. There are, of course, numerous possible answers to the question, what is a social class? It might be defined in terms of relationship to the means of production (Marx); occupation of a superordinate or subordinate authority position (Dahrendorf); unequal distribution of "life chances" (Weber). Unfortunately, this book indicates limited awareness of the richness of political sociology with regard to class analysis. This superficial treatment is repeated when it comes to using the still-undefined notion of "classes": farmers and the unemployed are eliminated altogether, and everyone else is classified (according to the occupation of the "head of household," a technique which is not sufficiently justified here) into two large groups, the middle class (essentially nonmanual occupations) and the working class (manual occupations). If one is advancing a theory about the relationship of occupation *per se* to vote choice or party identification, this might be perfectly reasonable; to assume that this method results in a satisfactory definition of "class" is not.

I would similarly criticize Abramson's treatment of the concept of "generations": though he does offer a minimal definition ("a group of persons who have undergone the same basic historical experiences during their formative years"), he goes no further. Most recent writers on generations, or those who use some type of cohort analysis, ritualistically mention Mannheim's "The Problem of Generations." It seems to me that some of these analyses could profit from a serious consideration of Mannheim's article. For one thing, he describes generational differences not in terms of fixed attitudes but in terms of different way of "experiencing life and the world." This more fruitful way of conceptualizing generational differences is put to good use by Paul Beck, for example, in his recent work on generations and partisan realignment (in *The Politics of Future Citizens,* ed. Richard Niemi, pp. 199–219). Another valuable part of Mannheim's work is his delineation of the notion of generational "units," the idea that people who experience during their youth the same historical events and problems respond—because of their different social locations—in different, even opposite, ways. Abramson's discussion in his sixth chapter about the polarization of attitudes of the college-educated young toward urban and campus unrest and aid to minority groups is interesting but would benefit from a more definite theoretical explication (similar to Mannheim's, perhaps) of the sources of the within-generation differences he finds.

On the operational level the treatment of generations is also unsatisfactory. "To maximize comparability," Abramson notes, "the arbitrarily defined 10-year cohorts employed in the 1948 survey were used" (p. 30). Comparability is gained at the expense of meaningful generational groupings, however. The whole concept of generations is based on the idea that the people comprising them experienced certain defined events at a particular period in their lives, and that dividing up the adult population into 10-year chunks can tap those common experiences only accidentally. One example should suffice. Abramson refers to the "Depression cohort" born between 1904 and 1913, when actually these people became eligible to vote during the years 1925 to 1934. The youngest of them, one could certainly argue, formed their political views in the very Republican and not particularly class-oriented twenties, while many who came of age during the height of the Depression would not be included in this cohort. Pooling the SRC data might in some instances solve the problem of too few cases which results from more finely-drawn cohorts; but in the absence of this, defining even a few large cohorts in concrete historical terms, as Butler and Stokes (in *Political Change in Britain*) do in their analysis of generational change in Britain, is essential.

The most interesting part of the book is the discussion in chapter 6 of generational differences in attitudes toward groups such as the military, civil rights leaders, the police, and black militants, and of generational differences in the relationships between these attitudes and support for McGovern in 1972. It raises the possibility that one way to successfully define a generation, and within that, generational units, is to find issues which were particularly important during the formative years of a generation, and to see whether those issues are more salient to the members of that generation and whether their attitudes on those issues are more closely related to their political behavior, e.g., their vote, than is true for citizens in other age groups.

It has been amply documented that there are political differences between age groups and that at least some of these differences, for example in strength of party attachment and perhaps in certain attitudes, are due to generational rather than life-cycle effects. What is needed now is a much more theoretically sophisticated and careful discussion of the sources and implications of generational differences than this book provides.

KRISTI ANDERSEN

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The Social Control of Drugs. By Philip Bean. (New York: John Wiley, 1974. Pp. 198. $11.95.)

Drug Abuse Control. Edited by Richard L. Rachin and Eugene H. Czajkoski. (Lexington, Mass.: Lexington Books, D. C. Heath, 1975. Pp. 181. Price not given.)

Sociologists of deviance have made significant contributions to our general understanding of social behavior. We now understand that societies seem to need deviants to maintain their own coherence, and that both deviants and conformists derive some satisfaction from being in opposition. We also know something about how behavior comes to be labeled as deviant, and whose interests are served by such action. Usually it is small, weak groups who are suddenly discovered to have some characteristic judged to be deviant. Usually social control agencies who have the responsibility for "controlling" the deviance (perhaps assisted by moral crusaders exploiting some dramatic events) apply the pressure for strong action against the deviancy. As a result of these efforts, laws are passed, regulations announced, budgets appropriated, and the whole clumsy mechanism of social controls is mobilized to influence the behavior of a small, weak, exposed group. Sketched from this lofty theoretical perspective, these events seem both ridiculous and inevitable.

An area where this kind of analysis has extraordinary power is drug abuse. Indeed, the only problem with the analysis is that it is a little too easy. Many sticking points in the conventional analysis can be got over quickly. An easy case can be made that current drug abuse policies are ineffective and inconsistent; consequently, it is easy to presume that their explicit objectives cannot be the *real* explanation for their existence. The social control agencies whose interests are served by current policies are too easy to dislike for their complacency, self-interest, and viciousness. The general public's knowledge of the problem is apparently too shallow, confused, and intolerant, and the victims of the social control efforts are too obviously the poor, degraded, and powerless.

Both *The Social Control of Drugs* by Philip Bean and *Drug Abuse Control* by Richard L. Rachin and Eugene Czajkoski adopt these general perspectives. Bean notes in his preface: "There did seem some point in trying to show that the drug takers' behavior and the workings of the legal system were inextricably linked and that sickness, after all, depends on who defines it as such." Rachin and Czajkoski note: "If any single thought is provoked by these articles, if has to do with the complex and symbiotic relationship between the existence of social problems and organizational responses ostensibly designed for their cure."

The book by Philip Bean is a gem. Though apparently exhaustively researched, it is quite short and nicely argued. Part I presents a history of drug control action in Great Britain since 1900. (An unexpected bonus is a short history of international control efforts as well.) Part II seeks to explain the twists and turns of that policy on the basis of international treaty obligations, the orientation and position of the medical profession in England, and the size and characteristics of the drug using populations. The roles of various moral entrepreneurs and the police are described less distinctly.

While it is bad form to give away the plot, it is hard to resist sketching the outline of Bean's analysis. From 1900–1960, the major impetus for British control efforts came from international treaty obligations. The medical profession monopolized the policy, and established a system of general exhortations to physicians about prescribing opiates. While provision was made for enforcing the prescribing regulation under "Medical Tribunals," only a few tribunals were established, and virtually no one was charged. The system succeeded because it posed a "solution to a problem when there was no problem in the first place."

By the 1960s, a rapidly growing epidemic of heroin use among young males had appeared. The new population of drug users liked publicity; they "showed off their spiritual sores like medieval beggars." Moreover, they were "unrepetant." This set of conditions invited control. Consequently, with only a modest effort by a few moral entrepreneurs, England acted to control drug use. Predictably, the reaction was harsh to the unrepetentant deviants, neglected the more serious problem of barbiturate use, treated the medical profession rather "gingerly" and created new police powers and organizations. The pattern is familiar and well traced.

The Rachin-Czajkowski book is a collection of essays of uneven quality and various concerns. Indeed, I am puzzled by the inclusion of the last three essays, whose topics are not obviously related to the other essays, and are not of high quality. The first four essays are worth some discussion.

"Drug Control: An Agenda for Repression" emphasizes the ineffectiveness of enforcement efforts in controlling drug abuse, and the enormous social costs of equipping the police with the special authority they need to enforce narcotics laws. The essay is a chest-thumper—full of the indignation one can muster about an apparently ineffective policy that imposes substantial costs on the powerless, and creates grave risks for the rest of society. Unfortunately, the analysis of the effects of drug enforcement is casual, and it is hard to discern the motivating force behind the enforcement policy.

The Dickson essay ("Narcotics and Marijuana Laws: Two Case Studies in Bureaucratic Growth and Survival") and the McDonald essay ("Choice in the Enforcement of Drug Laws") introduce one of the possible motivating forces—narcotics enforcement organizations. The Dickson essay argues that new regulations and laws were advocated by the Federal Bureau of Narcotics to insure their continued survival and growth. While the analysis is a little uneven (e.g., no real explanation that made addicts vulnerable to enforcement action; and the variables defining the bureaucratic "health" of the Federal Bureau of Narcotics change occasionally to fit the evidence), there is enough evidence to persuade a partly convinced reader that at least some of the reason for these policies was the RBN's bureaucratic self-interest. The McDonald essay presents some interesting information about the narcotics enforcement procedures of an urban police department. Unfortunately, he has not given himself enough space to examine this information, and has devoted too much space to an unnecessary rediscovery of police discretion.

Thus, these books provide an overview of drug control efforts, and some occasional glimpses of the details of both the actions of the various agencies and the social outcomes. The problem with the books is that it is not clear what we should take away from them. We can treat the analyses as data points supporting a general theory of deviance and social institutions. But these perspectives are far from being novel and uncertain; they have always seemed to me rather obvious and well known. How else would one expect a human society to behave?

What I suspect the authors really want to do is to influence policy. But in what direction and on what basis? There is a hankering for liberalizing the use of heroin, an argument made on the basis of some casual analyses of the impact of drug control efforts, the identification of some undeniable bad side-effects, and the discovery that some of the motivations for the policy may be self-interested. But none of these arguments is fully developed in a way that forces the authors to take explicit responsibility for the predictions implied by a policy preference. The only author who takes explicit responsibility for a policy position is Brantingham, and his observations are acute:

> The decriminalization movement makes considerable sense, but is marked by two points of concern. First, it exhibits an exhaustive preoccupation with two end points of the law changing process. The broad middle range of the ... process which involves the actual change of legislation, case law, administrative regulation, treaty and beyond that, the actual change of criminal justice agency behavior is rarely addressed. The problems involved ... are sufficiently difficult and delicate to warrant more than academic hand waving. Second, to the extent that the hope of beneficial result from decriminalization is pinned on the description of the British system advanced by Lindesmith ... and Shur, it has been undercut by subsequent events in England and by subsequent English scholarship.

The analyses of these books offer little in the way of help in deciding what to do about the drug abuse problem. They merely remind us that in deciding what to do, we must not only solve all the detailed empirical estimation and policy design problems, but we must contend with huge, unwieldy social machinery. A pity that the authors do not lend more of a hand.

MARK MOORE

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The Political Mind and the Political Environment: An Investigation of Public Opinion and Political Consciousness. By W. Lance Bennett. (Lexington, Mass.: Lexington Books, D. C. Heath, 1975. Pp. 207. $16.00.)

This book attempts a resynthesis of our understanding of public opinion and its role in a democracy. It is an intriguing blend of reconceptualizations of old data, reports from a range of new empirical studies, and a guiding set of normative concerns about the quality of public participation in politics. Although I found the book uneven in some respects, it contains many worthwhile nuggets and makes what strikes me as a useful contribution to the literature.

The author begins with a review of earlier interpretations of public opinion data, including as centrally as any, my own paper on "... Belief Systems in Mass Publics," and finds them wanting in a number of regards. Armed with results from several recent and largely unpublished studies in which he has participated, Bennett attempts to probe beneath the surface of such simple parameters of public opinion as "attitude stability" and "attitude constraint" to inquire as to the underlying developmental processes that produce variations in these manifest "indicators." He introduces considerations of cognitive style and, in one of the most impressive passages (chapters 3 and 4), dissects the significance of symbol manipulation at the level of political rhetoric for opinion formation in the public. Since the empirical results, while illuminating, are far from encouraging normatively, the author devotes the balance of the book to a further examination of available theory and data, with an eye to discerning the most effective means of improving the quality of public opinion formation. This quest takes him back through the literature on cognitive development and political socialization, and leads in the long run to the primary conclusion that the stimulation of broader grassroots political activism would be the most promising prescription.

I found the book uneven because of the author's penchant for ignoring sources and findings highly relevant to his contentions, in favor of detailed elaboration given to less relevant materials. I am not sure that this defect is a mortal one, but it does steal enrichment from a treatise which has rather ambitious synthetic yearnings.

Some of these blind spots occur in connection with the very extended review of my own work. In the early chapters, Bennett is concerned to argue that constraint and stability in mass public attitudes about political issues is much greater than my "Belief Systems" paper represented, mainly because these attributes must be sought out in more homogeneous subgroups of the society, on more concrete issues. More particularly still, he contends that such signs of muscle tone in mass attitudes can arise, as it were, indigenously: they are not dependent exclusively on a "trickle-down effect" whereby the elite defines certain connections which become progressively more blurred and psychologically ineffectual as one proceeds into less politically attentive strata of the populace. Bennett does not deny that such elite definitions and tricklings-down are one part of the public opinion picture: indeed, he is dependent upon such mechanisms in his later treatment of the elite manipulation of political symbols. But he does wish to argue that mechanisms of this kind are not the sole source of such constraint and stability.

Now in the "Belief Systems" paper the data obliged me to think in terms of more homogeneous "issue publics" for whom certain very concrete issue domains were highly salient and others were of minimal consequence. And in a last section of the paper I asked whether membership in these various more homogeneous issue publics seemed cumulative or hierarchical, as a pure "trickle-down" model could well imply, or whether these publics (within which attitude stability ran higher) were dispersed helter-skelter from one issue domain to another, suggesting a more indigenous origin. The data turned out to be clearly non-hierarchical, just as Bennett would wish. Rather than use these findings, however, he prefers to argue that I ignored the possibility of anything but a hierarchical model, and never even mentions the most relevant section, despite copious citations of less relevant parts of the paper.

Similarly, much of chapter 2 is given over to an examination of the relationship between cognitive style and attitude stability. He imputes to me a prediction that attitude stability should increase linearly with cognitive complexity. He counterpredicts, on the basis of psychological theory and considerations of information in take as an intervening variable, that the relationship should be curvilinear. He shows that it is, and uses my alleged misprediction to call my conclusions on a wide variety of other matters into question. Actually, a dozen years ago I devoted an entire widely cited paper ("Information Flow and the Stability of Partisan Attitudes," *Public Opinion Quarterly,* 26 [Winter, 1962]) to the curvilinear prediction which is exactly Bennett's form, involving information intake and attitude stability (with cognitive sophistication lurking in the background as well), and displayed some sketchy data which seemed to confirm the prediction. This work is never mentioned, although it would have straightened out his guesswork and provided him with confirming data, to boot.

While these odd renderings are easiest to spot for my own work, they scarcely stop at that boundary. To my mind the most exciting developments in this area spring from changes in the parameters of public attitudes over the past decade as publicized by Pomper (most notably, "From Confusion to Clarity: Issues and American Voters, 1956–1968," *The American Political Science Review,* 66: 415–28) and many others, and culminating in Nie's "Mass Belief Systems Revisited." Indeed, the leading edge of discussion currently involves what illuminations and conclusions can be drawn from all of this observed change. Since one focus of Bennett's book is upon how exactly these same parameters of public opinion could conceivably be changed, it is disconcerting to discover that the naive reader of his book would never know that such change had in fact occurred, much less that it had been subjected to intense scholarly discussion. These rich sources are simply not mentioned.

I would not argue that what is being learned about such change would necessarily embarrass the author. I think at most points it would not. But the lack of enrichment mars the book. A number of findings in chapter 4 concerning circumstances under which attitude constraint can rapidly increase need not be left at a hypothetical level: they occurred *en masse* five and ten years ago, and have been discussed in terms compatible for the author. Similarly, the author's view that "there is no *a priori* explanation" (p. 24) for his finding that the stability of responses to abstract political statements is less than for concrete issues is astonishing: it seems to me it was predicted ages ago, and that what is being learned from the 1960s helps considerably to flesh out the dynamics involved. Finally, there is other published evidence, again not cited, leading directly to the author's most emphasized conclusion, linking heightened attitude stability and constraint to political activism beyond the isolated ritual of voting. (See, for example, evidence summarized in my "Public Opinion and Voting Behavior," in F. Greenstein and N. Polsby, *Handbook of Political Science,* Vol. 4 [Reading, Mass.: Addison-Wesley, 1975].)

Despite all of its puzzling lacunae, the book retains a good deal of interest. While I personally profited most from the attention given the impact of elite symbol manipulation, numerous other passages, including discussions of cognitive styles and socialization processes, may well repay the reading.

PHILIP CONVERSE

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The Political South in the Twentieth Century. By Monroe Lee Billington. (New York: Charles Scribner's Sons, 1975. Pp. xiii + 205. $8.95.)

The Persistent Tradition in New South Politics. By George Brown Tindall. (Baton Rouge: Louisiana State University Press, 1975. Pp. xiii + 72. $4.95.)

George Tindall is a leading historian who has here made another felicitous contribution to the understanding of the nation's politics. The extraordinarily deep and rapid changes occurring in the American South have brought us to an era that George Tindall calls "a post-New South which does not yet have a name" (p. 2). These two volumes trace and discuss the way we, the United States, got there.

The Persistent Tradition in New South Politics is the published version of the Walter Lynwood Fleming Lectures in Southern History given at Louisiana State University. The slender volume embraces three essays, one each on Bourbons, Populists, and Progressives. It is gracefully written by an historian's historian, reflecting knowledgeably on changing interpretations and reinterpretations by historians of the region. It is instructive not only of the South, but also of the obstacles to clear understandings of social change and of the role that imperfect understandings can themselves play in shaping attitudes and actions.

Tindall finds a persistent tradition of "community" in the South. It persisted in the Bourbons, who "had achieved the ultimate success of all durable conservative movements, the reconciliation of tradition with innovation" (p. xii). The Populists challenged the vision of the traditional organic community, so the Bourbons thought, and thus had to be put down. The Progressives advocated a more active government, but at bottom were the heirs of the Bourbons. The author searches for an accurate definition of each of his three political types, acknowledging with realism that in our search for order and understanding we sometimes seek realities to match established labels, rather than devising labels appropriate to newly perceived realities.

A useful service of the book is its emphasis on the unevenness, raggedness, and variety in human behavior. Tindall emphasizes the diversity and cleavages within the Bourbon South, to which political period greater homogeneity used to be attributed than the facts warranted. He reminds us that the fall in electoral participation after 1900 was among southern whites as well as blacks, and in Democratic primaries as well as in general elections. In the current period of declining party orthodoxy, it is relevant to be reminded of the bitterness of partisan controversy eight decades ago. The issues for which persons fought and suffered then, and occasionally died, were *political party* issues. More recently, important issues about which persons have felt no less deeply, e.g., civil rights, Vietnam, have been fought largely inside and outside the parties rather than between them.

Monroe Billington states the purpose of *The Political South in the Twentieth Century* clearly at the outset: "a brief, general, up-to-date, interpretive account of southern politics in the twentieth century" (p. ix). He also gives his grand summary at the outset: "One hundred years after Reconstruction the South is far along the road back to the mainstream of biracial, two-party American national politics" (p. xiii).

The book is divided into seven chapters, dealing successively with Progressive politics, the impact of World War I, the Depression, white politicians and black civil rights, the black revolution, presidential Republicanism, and the new politics of the South. Each chapter is divided into sections. The treatment is chronological, often with topics developed through state-by-state descriptions within the sections. The book is packed with narrative details. It may thus be of greater interest to the newcomer to the subject than to students seeking a more explicitly conceptualized general interpretation of southern politics since 1900. There is a useful bibliographical essay at the end, indicating the extensive secondary sources on which the author has drawn. For the immediate past, he relied heavily on newspapers and magazines. The book is useful as a compact survey and as a reference work. It develops interest by presenting political issues in the terms of their own time, while still emphasizing the dynamic influences that shaped change. It is good for readers in the 1970s to know the terms in which controversies were debated in prior decades, for example, social security legislation during the New Deal and Jim Crow legislation for the nation's capital during the New Freedom.

The central political significance of blacks in the southern population is kept before the reader. Billington steadily stresses the diversity in southern political divisions. The transition to a nationalized southern politics during World War I and thereafter is fittingly emphasized. One gains a sense of the massive and inevitable impact of major wars on domestic political issues, organization, and competition. The author clearly identifies the impact of the New Deal on blacks—not Mrs. Roosevelt's alleged meddling in the southern way of life, but the impact of social and economic change resulting from new federal social and economic legislation and the concept of equal citizenship. Billington traces for their political significance changes in the region between as well as during these periods of social trauma: for example, the Progressive tenets of industrial and economic development, the development of state government services following World War I, the pre-Depression agricultural problems of the 1920s, and most important of all, the opening of voting booths to blacks in the 1960s and 1970s.

Both these books are short, both address southern political change, both are informed by the literature of the subject, and both are offered in the tradition of literary history. Tindall's lectures are more engagingly written but with a less comprehensive objective. It is implicit in both books that to understand fully the next stages of regional political evolution will require interpretation of alterations that have been occurring in the national party system and that seem destined to increase.

ALEXANDER HEARD

Vanderbilt University

Citizens and the Environment: Case Studies in Popular Action. By Lynton K. Caldwell, Lynton R. Hayes, and Isabel M. MacWhirter. (Bloomington: Indiana University Press, 1976. Pp. xxx + 449. $17.50.)

A National Policy for the Environment: NEPA and Its Aftermath. By Richard A. Liroff. (Bloomington: Indiana University Press, 1976. Pp. xi + 273. $10.00.)

After a first generation of environmental policy books in which consciousness raising was the goal, and a second in which analysts advocated prescriptions for reform, it is appropriate that we now have the next logical development: studies which describe what actually happens in this important policy area. Both these volumes use case studies to describe the status of particular policies. The first, *Citizens and the Environment,* is a compilation of 68 cases ranging in significance from such familiar national issues as the Reserve Mining, Alaska Pipeline and SST cases to such local issues as preserving open space in Jefferson County, Colorado and a 100-acre salt marsh on Long Island. Accordingly, the space devoted to them varies from less than two pages to more than ten for the Alaska Pipeline. This effort to embrace every environmental issue from land-use patterns to health impacts of air pollution has resulted, perhaps inevitably, in uneven coverage of events, truncated narratives, and an encyclopedic collection of acronyms.

As the title of the book indicates, the message is citizen participation. It is an outgrowth of the recent philosophy of the senior editor, Lynton K. Caldwell of Indiana University, although in fact Lynton R. Hayes and Isabel M. MacWhirter wrote all the case studies. These studies represent examples of conflict between environmentalists and growth-oriented policy makers which are all too familiar to students of environmental policy. If the purpose of this volume is to produce an instructive book of cases, it would have been more useful to include a smaller number of cases with greater depth of coverage of each. More recent material in greater detail about most of the incidents reported here is available in such sources as the Bureau of National Affairs' *Environmental Reporter: Current Developments.*

The authors have avoided the greatest dangers for case studies: generalizing from a few cases to the universe of policy making. On the other hand, no descriptive generalizations are made from the 68 cases. No inferences are drawn or theories expounded on the relative empathy of federal decision makers or state and local ones toward environmental causes. Nor is any comparison made between the effectiveness of ad hoc local public-interest groups and nationally organized ones. In some instances the groups involved were successful in preserving (temporarily at least) their piece of nature. In others, the highway, dam, pipeline, nuclear reactor or condominium went through, on schedule or off. But no trends, temporal or geographical, ever emerge. Even the generalization that there can be no generalizations about environmental policy is not made explicitly.

The second volume takes a different approach; its author, Richard Liroff of the Environmental Law Institute, investigates in great detail one piece of legislation—the National Environmental Policy Act (NEPA)—from its legislative history to subsequent judicial interpretation and suggested changes. In a sense this book can also be credited by Lynton Caldwell, as most writers about NEPA acknowledge his input to the Congress at the time the law was being discussed.

Liroff concludes that NEPA was passed more through lack of understanding of its implications than because of careful work by proponents. One might question whether this conclusion does justice either to its proponents or opponents, considering the major compromise he describes: the elimination of any power to stop environmentally damaging projects by an agency external to the one responsible for administering the program. Examples of testimony given by environmentalists in support of the rejected clause, however, indicate that they were clear on what they wanted. The fact that it was killed in committee indicates that both sides were not totally ignorant of the import of what they were about (p. 23).

Because no power to stop projects was given either to the Council on Environmental Quality or to the Environmental Protection Agency, the most important aspect of the law became its potential for modifying the attitudes and behavior patterns of actors inside agencies with the responsibility for writing environmental impact statements and carrying out the projects involved. The central portion of the book, where the author addresses the question of NEPA's impact or lack of impact on these attitudes and behavior, is the best part of the study, for it is here that he elaborates on some useful examples of the tendency of bureaucracies to cling to instrumental goals which prevent them from incorporating values from groups external to their immediate clientele into their decision making. This idea has been well developed by organizational theorists, which lends credence to the idea that NEPA, far from being accidental, was one of the most theoretically informed attempts to alter bureaucratic behavior yet made by the U.S. Congress.

The effect of NEPA as a remedy for the problems inherent in incrementalism is a difficult phenomenon to measure, as Liroff rightly points out. Consequently, he focuses upon questions which logically precede the substantive evaluation of individual impact statements—questions concerning personnel changes within agencies, guidelines for drafting statements, as well as numbers and complexity of statements issued by various agencies. Some surrogate measures for substantive evaluations of impact statements are included: the Environmental Protection Agency's ranking of other federal agencies' statements and the number of lawsuits filed against agencies by public interest groups. Therefore, it is possible to compare the responsiveness of agencies to NEPA, and here some surprises are revealed, e.g., the high ranking given the Corps of Engineers, traditionally an organization not environmentally oriented. Generally, however, his findings are that real changes in response to NEPA have been slow, grudging, and small, and this can be explained by the forces which lead to inertia within most bureaucracies.

One is left with the impression that the cynics may be correct; the largest measurable impacts of impact statements may now be reductions in the size of our national forests and in the unemployment level among Ph.D. holders. However, as Liroff points out, his is only a preliminary study. He makes no real attempt here to systematically analyze the kinds of projects which were undertaken in even one agency before and after NEPA, nor to analyze the differences between draft statements and final statements after public and governmental comments have been added. Even more important, the author has failed to ask the logical next question: have we any evidence that environmental impact statements are accurate?

A wealth of information has now been created for researchers interested in determining the accuracy of our predictions of ecological and social impacts. Sufficient numbers of completed projects now exist whose real impact on the physical environment and human communities can be measured and compared with the impacts predicted for them by natural and social scientists.

Like the authors of *Citizens and the Environment*, Liroff attributes much of whatever impact NEPA has had to public interest groups and their willingness to litigate and to comment on draft impact statements. Thus this book ends on the same theme that the first began: an implicit faith in external private groups' capacity to force governmental agencies to become more alert to the implications which their programs have for the environment. This theme in turn reflects a change in emphasis in the thinking of such analysts as Caldwell, from a belief in the re-educability of technocrats through such action-forcing programs as environmental impact statements, to a reaffirmation of the need for public watchdogs external to the system.

One might accept this prescription on purely procedural grounds without trying to evaluate the impact of citizens groups on environmental policy. "Power to the people" is an appropriate slogan for a democratic system. The hard questions, starting with the basic: who are the people? remain unasked. There are victories recorded among the 68 case studies both for those who favor unfettered growth and for those who favor environmental protection. We are being asked to take on faith the notion that involvement of the populace will be helpful to the environmental cause. Both books have generated some empirical evidence for this idea. We know that popular opinion has proved useful in some places at certain times utilizing certain techniques. However, environmental strategists are given little help in discerning what these other variables ought to look like, and policy analysts are left wondering when and under what circumstances the public becomes sufficiently concerned to have an impact on environmental policy.

LETTIE MCSPADDEN WENNER

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Measuring Business's Social Performance: The Corporate Social Audit. By John J. Corson and George A. Steiner. (New York: Committee for Economic Development, 1974. Pp. 74. $14.00, paper.)

Social Responsibility and the Business Predicament: Studies in the Regulation of Economic Activity. Edited by James W. McKie. (Washington, D.C.: The Brookings Institution, 1975. Pp. 361. $12.50, cloth; $4.95, paper.)

These books represent recent contributions to a scholarly and practical theme of continuing importance—the issue of business behavior in a presumably pluralistic society. Corporate social responsibility has been a major arena for debate in modern form at least since the publication of Brandeis' *Other People's Money* in 1914, but until recent years has been noted more for its ideological content than for its development of criteria or implementation.

It is in respect to implementation that *Measuring Business's Social Performance: The Corporate Social Audit* is significant. The slim paperback volume was prepared for the Committee for Economic Development, an independent research-educational organization, mostly business-supported and oriented, by George A. Steiner, Professor at the Graduate School of Management, UCLA, and John J. Corson, businessman and educator, with the collaboration of Robert C. Meehan, a management consultant with the accounting firm of Peat, Marwick, & Mitchell & Co.

Business firms have almost always supported the notion of corporate social responsibility, which one would expect, since it costs nothing and the opposite would be tantamount to opposing God and motherhood. Detractors in abundance have either dismissed the claim as mere hypocrisy, delusion, or toothless ideology. Sutton, et al., in *The American Business Creed* (1955) analyzed the business claim of corporate social responsibility as a mere strain reaction to criticism of business behavior. Some ten years later, Earl F. Cheit in *The Business Establishment* saw the phenomenon as a real change in business attitude and as a response to criticism.

Now comes the CED publication, characterizing as "a new dimension of corporate accountability" the emerging corporate practice of offering a social report, or as they borrow the term from accounting, a social "audit." The report acknowledges the growing "social" demands on business, as well as the lack of business consensus on how they ought to respond; however, it has a generally optimistic flavor. Reviewing the pressures on business for greater disclosure, the Steiner book then reports the results of a survey of 254 corporations on how and what they have been reporting in their social audit. Businesses have been recording their participation, as one would expect, in contributions to economic growth and efficiency, education, employment and training, civil rights and equal opportunity, pollution abatement and urban renewal, as well as culture and the arts. Contributions to conservation and recreational activities appear to be small, but surprisingly, a substantial number of corporations report involvement in medical care activities. The concluding chapters of the CED book consider the scope and the value of the social audit, and endorse the practice. From an outside viewpoint, one aspect of the business social audit is at once a logical outgrowth of business practice and yet an apparently questionable execution, viz., that the professional group apparently given most responsibility for the social audit, which essentially involves sociological and political matters, are the accountants. This fact alone ought to arouse the interest of political scientists, and *Measuring Business's Social Performance* is a good place in which to start an examination of the social audit, since it is very well done and offers considerable food for thought.

Social Responsibility and the Business Predicament, edited by James W. McKie, has been put out by the Brookings Institution as one of its studies in their series in the Regulation of Economic Activity. The book's purpose is vastly different from that of the CED document. The Brookings Institution is interested in social responsibility to determine "what is sensible and workable in the demands for social responsibility, and to make specific recommendations on how much and what kinds of responsibility business enterprise can usefully be expected to assume" (p. vii).

Eleven authors have contributed essays to the collection, and the set offers some outstanding intellectual contributions. The authors are generally pessimistic regarding the likelihood that corporations can achieve any meaningful measure of social responsibility without significant outside pressures upon them, but they offer some solid suggestions for increasing corporate output of social goods.

McKie outlines the development of variations in the corporate attitudes toward social responsibility, and Marvin A. Chirelstein argues that changes in corporate law could only have a slight effect on business social behavior. Thomas C. Schelling's chapter has some provocative thoughts on the assignment and identification of responsibility among individuals within a corporation; Roland N. McKean's application of the "free rider" problem to the question of social responsibility is very interesting. James A. Wilson considers costs and benefits to consumers, vis-à-vis regulation, while Martin Bronfenbrenner concentrates on the vulnerability of consumers. Jerome Rothenberg discusses the deterioration of the physical environment; John F. Kain looks at urban problems and Benjamin Chinitz at regional development; Raymond Vernon is concerned with corporate foreign operations; and Charles A. Myers examines employer-employee relations.

The McKie book covers many of the pressing issues presently under consideration by those interested in the relationship between business and society. I found the contributions by McKean and Bronfenbrenner most innovative and interesting, but those with different interests will find challenging material in the other articles. There are some problems with the book; as with most collections of this nature, the contributions tend to lack focus. Some important issues are hardly considered, if at all, e.g., the question of executive criminal activity, political corruption, and whether business executives have any legitimate mandate to engage in social activities outside of their narrow business sphere. Nevertheless, there is a good deal of interesting material in the McKie book, and it makes an excellent companion piece to the Steiner volume.

ROBERT CHATOV

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The Politics of Exclusion. By Michael N. Danielson. (New York: Columbia University Press, 1976. Pp. ix + 443. $17.50, cloth; $6.95, paper.)

The deplorable conditions in parts of our large core cities and the fragmented political structures which govern our metropolitan areas are unparalleled in the advanced industrialized and urbanized nations of the world. Are the deplorable conditions of our cities the result, to a significant degree, of fragmented political arrangements? This is one of the most important questions facing this nation. Yet any proposal which is based on the assumption that the two sets of phenomena are related runs into the most intransigent and determined opposition to be found anywhere in our contemporary political life. Danielson's book is centrally concerned both with the question and the political responses to it.

Danielson systematically and comprehensively details the political decisions and structures which affect the residential circumstances of various groups, especially low-income and black populations. He treats the familiar subjects of zoning, planning, public housing and local finance with a wealth of illustrations drawn from a great variety of sources. His major contribution, however, lies in his orderly analysis of interest groups, the courts, Washington bureaucracies, metropolitan councils and state governments. The comprehensiveness of his treatments results in something more than a compilation of data. His central message is that for those who wish to "open up the suburbs" there is scant hope for success, regardless of which political arena is chosen for the fight.

If there is a possibility to alter the politics of exclusion, Danielson sees it in the fact that many fears which fuel the controversies are unfounded: the "parade of the horribles" tactics may become less effective if a few cities, like Dayton, for example, show that by sharing responsibilities among all municipalities, housing for low-income groups can be handled without destroying existing neighborhoods.

This volume does not break any methodological ground. Danielson proceeds to make his points by the sheer mass of evidence; indeed, if the book has a fault, it is in a tendency toward redundancy. For some, the book will contain more detailed information on urban politics than they want. But for teachers in the subject area, it should become a standard and could serve well as one of the basic sources in courses on American urban politics. For the would-be political activist concerned with housing and metropolitan politics, it is essential reading, as it imparts the kind of realism that offers sound counsel to those who wish to embark on new schemes for change.

OLIVER P. WILLIAMS

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Who's Running America? Institutional Leadership in the United States. By Thomas R. Dye. (Englewood Cliffs, N.J.: Prentice Hall, 1976. Pp. 222. $8.95, cloth; $5.95, paper.)

The early protagonists of elite theory consciously designed their theoretical framework as an answer to Marxism. While Marxists sought to discover laws of the capitalist political economy which exist irrespective of the characteristics of institutional officeholders, and analyzed political-economic processes and policies in terms of these laws and structures, elite theory focused upon the character of ruling elites. Classical elite theory was, however, not only descriptive theory but a prescriptive one as well, either justifying "elite" control over masses (Gentile), or despondently arguing its inevitability, regardless of political-economic system (pre-First World War Michels). In contrast, Marxism argued that a breakdown into an elite and mass was attributable ultimately to the class nature of all existing societies and would disappear when class society disappeared.

In part because of elite theory's association with fascism during the interwar period (Rocco, Gentile) and, in part, because of the post-World War II "Celebration of America" embodied in the newly revived doctrine of pluralism, empirical studies based upon elite theory lay dormant in America until 1956 with the publication of *The Power Elite*. With that book, Mills instituted a novel departure in elite theory—its use as a radical critique of American society. But, even then, such Marxist or neo-Marxist critics as Paul Sweezy and Robert Lynd warned that elite theory led to: (1) a voluntarism in which decision makers' ranges of choice are divorced from the laws and structure of the social formation, (2) ahistoricism in which social change not explicitly directed by elites cannot be readily explained, and (3) an inability to make normative judgments about the "elite" except on the most simplistic "gut reaction" grounds.

Who's Running America? assembles an impressive amount of information, much of it new, on America's institutional leaders. Yet, Dye's unquestioned acceptance of the theory's premises and his obvious unawareness of the recent epistemological controversy over the utility of elite theory—especially the debate among Laclau, Poulantzos and Miliband—led, unhappily, to a book containing some not very useful time-bound information. Moreover, the information developed is employed to bolster Dye's conservative inclinations—notwithstanding a disclaimer that he is doing nothing more than providing "interesting data on national institutional elites" (p. 217) just as G. William Domhoff has employed similar information to support his radical inclinations. Let us see why elite theory leads to: (1) voluntarism, (2) ahistoricism and (3) a disjunction between empirical data and evaluative principles.

The first major charge against elite theory of which Dye's work is an important example is its voluntarism. If Dye had asked not "who's running America?" but rather "how is America run—and in whose interests?" his research would have led in an entirely different and more fruitful direction. He might have, for example, directed his attention to the problem of whether the capitalist system imposes certain constraints upon policy makers by virtue of its structure and laws, irrespective of who occupies office at a particular time. For example, a British Labour government containing virtually no businessmen—indeed, composed of persons nominally hostile to business—has cut social welfare appropriations just as forcefully as a Conservative government largely composed of big businessmen or business-related politicians would have. The Labour Government did so, not because of their "elite characteristics," but clearly because of certain objective laws governing the United Kingdom's particular social formation in the context of the contemporary world capitalist system.

Dye, while aware of businessmen, is thus unaware of capitalism, and is led to accumulate data of minor utility. For example, what major difference does it make whether the secretary of state is, like Henry Kissinger, of lower-middle-class Jewish background, or of the very different backgrounds of John Foster Dulles or Dean Rusk? What principally matters is, quite obviously, not such background data, but the laws regarding institutional structures in such a capitalist social formation as the United States which promote certain "acceptable" persons to high position, and as Paul Sweezy put it, "the range of choice which is set by the nature and laws of the social structure under which they live." Only after making such an inquiry can one reach the problem of whether *specific* background data leads to specific policy preferences within that range of choice.

Voluntarism is but the first of the major theoretical errors inherent in the elite theory to which Dye subscribes. The second major error is its ahistoricism. Dye collected his data from 1970–71 sources, but assures us that the time lag between then and 1976 does not disturb "any major findings" except for "the recent addition of a small number of women and blacks to top institutional positions" (p. x). But what this rather bashful statement glosses over is that there have been changes in the composition of the "elite," and that these changes reflect the movement of social forces in American society. Elite theory simply provides no explanatory mechanism which would allow us to understand why the "elite" of 1970–71 is different in composition, policy preference, or even style from the "elite" of, say, 1930–31 or 1976–77. Thus, all of the substantive data developed by Dye and his associates is of no scientific value whatsoever, and, even if new data were provided in annual editions of this book we would have no understanding of America's dynamics. The books' findings are, then, trivial at best.

But there is still another sense in which Dye's work (and elite theory, generally) is static and ahistorical. Elite theory urges us to look for one coherent elite in which some one group or another which may be dominant at a particular time. It cannot provide explanations for the frequent dissonance within the "elite" or why, over time, some interests appear or disappear within the "elite." Thus, the high military may have been members of the "elite" when Mills wrote, but were no longer members of this select group when Dye wrote (p. 68). But the theoretical conceptions of elite theory cannot explain why different organs of the state appartus vary in importance over time and why contradictions and conflicts (e.g., welfare versus military) frequently develop between state organs and branches. Space, of course, precludes the presentation of a conceptual scheme which can embrace the various conflicts and changes in importance between state organs within the framework of capitalism, but Nicos Poulantzos' concept that the state structure is divided and that its various organs and branches reproduce and reflect divisions and clashes between classes and fractions of classes within the *general* political interest of the capitalist class (e.g., private property) is far more useful than elite theory in understanding changes in state policies, and contradictions between policies. Very simply, Dye's static elite theory analysis cannot tell us anything about the changing relationships of state organs, nor can it link the diverse policy outputs of the state into an integrated dynamic framework.

Given the theoretical deficiencies of elite theory, it is not surprising that the elite theorists' evaluative judgments vary considerably. But in Dye's case there is no attempt to do anything but pontificate about his rather conservative biases—even to the point of ignoring contrary evidence. To take but a single theme running throughout the book, Dye refers to major corporate executives as "concerned with the public interest" (p. 42), "advocates of corporate responsibility" (p. 44), and "public regarding" (p. 169). But, while corporate officials may feel that way in their heart-of-hearts, it is extremely difficult to square their conduct—manufacturing dangerous cars, processing food with unnecessary additives, causing irresponsible levels of water and air pollution, etc., etc.—with such sentiments. The reason is not that big businessmen are inherently venal, but rather, as Neil Chamberlain, among others, has shown, capitalists are constrained by the laws of capitalism, especially the centrality of the profit motive in the capitalist system.

In summary, then, *Who's Running America?* not only suffers from the grave theoretical deficiencies inherent in elite theory, but, in addition, it is not a particularly distinguished example of its genre. Its biases are presented in a very assertive manner without even considering plausible arguments in conflict with Dye's views. The enormous effort expended on data collection is but another example of an elephant laboring mightily and giving birth to a mouse.

ALAN STONE

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The Rise of Guardian Democracy: The Supreme Court's Role in Voting Rights Disputes, 1845–1969. By Ward E. Y. Elliott. (Cambridge, Mass.: Harvard University Press, 1974. Pp. xiv + 391. $15.00.)

Ward Elliott's thesis is that there exists in the United States a group which he calls the

"Guardians," composed at least partly of liberal political scientists, which has influenced the Supreme Court to be "interventionist" especially during the Warren years and especially in the reapportionment cases. The Guardians are essentially but not deliberately elitist, in the sense that they believe so deeply in democracy that they are willing to force their version of it upon an unwilling populace in disregard of clear constitutional principles. He sees two disastrous results: one, the public becomes used to "guided democracy" and becomes alienated and unwilling to participate; two, the reapportionment decisions focus upon "formal" rather than "essential" democracy, which consists primarily not of forms but of consensual attitudes.

The book takes the form of an extended polemic, well written except that it is tediously repetitious, against the Warren Court. Many of its points are well taken: the court did often produce poorly reasoned opinions; its zeal for reform did sometimes outrun its obedience to constitutional command; and the reapportionment decisions did not produce the results for which some of their more enthusiastic supporters hoped. Nevertheless, these are points which have often been made by other writers, and it is doubtful that the author would wish to rest content with having made them.

Elliott would no doubt prefer the book to be judged on its underlying thesis (described above). But it is at this point that the book is weakest. It depends upon a conspiracy theory of politics—although the author is too sophisticated ever actually to claim that there *was* a conspiracy. The political scientists he quotes would be happy to think that they had as much influence as he attributes to them. He has a hard time arguing that the reapportionment decisions were "thrust" on the American people in view of the fact, which he admits, that the decisions were received enthusiastically. His argument that there is no constitutional basis for the decisions is weak: many outstanding constitutional law experts disagree. And there is no obvious connection between the constitutionality or the wisdom of the decisions and their results.

To expand upon one of these points: it has long been obvious that the results of reapportionment in many states have not been what some of the reformers hoped for. But this in no way proves that the decisions should not have been made. Elliott makes much of the fact, for instance, that instead of helping the central cities, reapportionment has actually aided the suburbs. Granting that this has often been the case, how does it bear on whether the decisions should have been made? Underlying the apportionment decisions was the idea that legislative districts should reflect the distribution of voters. If voters are in the suburbs, the meaning of the decisions is that the suburbs should gain greater representation. Whether this fact would make the original sponsors of reform happy is really beside the point.

One may even agree that there has been at times something which one could dignify by calling it the Guardian Ethic. That it has directly influenced Supreme Court decisions is much more doubtful: it is much more likely that, coming from the same groups, the judges were actually "members" of the Guardians. And this reflected the fact that most of them were appointed by presidents who were themselves Guardians and who were popularly elected. The fact seems to be that the Guardians were influential because they represented something in which many of the populace believed: there was not merely a series of reforms thrust upon an unwilling public by an elite.

Elliott believes in powerful legislatures, and one of his concerns is that the Guardian Ethic has succeeded in undermining the legislative branches. Legislatures are not—no one would argue the point—as powerful as they were in the nineteenth century. But Elliott skips too lightly over the deficiencies of American legislatures: their lack of leadership, their inability to make policy, their frequent venality—and, often, their lack of representativeness. To a great extent, the expanded powers of executives and even of courts grew out of the incapacity of legislatures to meet modern problems.

Then too, Elliott assumes (like Justice Frankfurter) that apportionment is properly a legislative task. In fact, the legislature is probably the very worst place in which to lodge this power, for quite obvious reasons. Apportionment is a *constituent* function: the only problem is to figure out how to carry it out as such. One may agree that the Supreme Court has not solved this problem; but one cannot really argue that it ought to be handed back to the legislatures. In fact, one might even argue that the legislatures even now retain too much control of apportionment, which results in the continuation of egregious gerrymandering. Elliott is probably correct in arguing that the courts were wise to stay out of the gerrymandering question: but he does not argue that gerrymandering is a good thing. On his evidence, we the people (not through the courts) ought to be seeking a better method of accomplishing apportionment; he does not believe that British-style boundaries commissions would work, but they haven't really been tried.

Enough has been said, I think, to indicate that this is a stimulating, at times perceptive, but basically flawed, piece of work. Political scientists should read it, because we need this kind of self-criticism. But we need not, needless to say, accept its particular criticisms.

Loren P. Beth

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The Growth of American Government: A Morphology of the Welfare State. By Roger Freeman. (Stanford, Calif.: Hoover Institution Press, 1975. Pp. 228. $8.95, cloth; $5.95, paper.)

With this book Roger A. Freeman, Senior Fellow at the Hoover Institution, moves into the vanguard of the neoconservative counterattack on the welfare state. Freeman is no stranger in conservative ranks. He served as assistant to Presidents Eisenhower and Nixon, has twice won the George Washington Honor Medal of the Freedom Foundation at Valley Forge, and now seeks to recruit a wider audience.

This volume, the author tells us, is an updated and expanded version of the earlier work for which he won Freedom Foundation acclaim. It contains an analysis of the development of governmental employment, revenues and expenditures for the period 1952–1972 along with polemical development of the thesis that "the steady weakening of the nation's global and military power, and of its domestic tranquility, cohesion and progress must to a large extent be attributed to the welfare state" (p. 35).

For the analytical portion of his work Freeman utilizes data from a wide variety of governmental sources including OMB, SESA, the Census Bureau, etc. A "Technical Note" discusses troublesome problems of data comparability. In his more restrained moments the author is prepared to admit not only the limitations of his data but the monumental difficulties involved in devising satisfactory measures for such things as productivity of governmental employees and the benefits of governmental programs. Freeman has some interesting and significant things to say; but the manner in which he presents his data and the extravagant rhetoric of his argumentation will appeal only to the true believer. There is much repetition in this book. We are informed on pp. 3, 20, 124 and 179, for example, that "well over 500" federal categorical grant and loan authorizations are now in operation.

Mr. Freeman's central concern is with the "dramatic shift" from defense expenditures to expenditures for domestic purposes. In the period 1952–1972, he tells us, defense expenditures as a percentage of all governmental spending dropped from 50 percent to 21 percent while the share of domestic services went from 37 percent to 69 percent of the total. He notes that social welfare (education, income maintenance, health, etc.) received 48 percent of all public expenditures in 1972 and holds that the cost of most such programs far outweighs any possible benefits. The product of our educational system, for example, does not reflect the fact that expenditures for public education increased eight times between 1952 and 1972. Citing the Coleman Report and other studies, Freeman is, to put it mildly, very dubious about finding positive correlations between educational expenditures and educational quality. He warns, as others have warned, against throwing dollars after problems.

Apparently the same reasoning cannot be applied to defense expenditures. Here, it would seem, bigger is better and no elaborate proof is needed. Nor does Freeman require much proof that governmental "investment" in highway construction, one of the few domestic programs which meets his approval, has yielded rich dividends. The primary gain, it would seem, has been in highway safety. Due primarily to the construction of modern roads "in 1972 36,300 lives were saved which would have been lost had 1952 fatality rates continued" (p. 23). In addition good roads save time which, as we all know, is money. The environmental impact of the highway program, its effect on energy use, etc., escape attention; but then the author has warned us that cost-benefit analysis often depends heavily on human judgment and political ideology.

Insofar as appears, Freeman's ideological commitments are uncompromising. Frequent appeal is made to the "free market" without any suggestion that even minor imperfections may exist. What are sometimes popularly characterized as "tax loopholes" for the wealthy appear under Freeman's searching scrutiny not as loopholes at all but as carefully considered "incentives" which benefit us all (p. 90). One must conclude that the "free market" for which Freeman speaks operates within a context shaped by a very extensive set of these governmentally provided "incentives." The author has no doubt that his version of the "free market" rewards individuals in accordance with their contributions to society. People are worth the wages they can earn—in non-governmental employment—and those who challenge the "free market" dispensation seek merely to "redistribute" income from "those who earn it" to "those who yearn it" (p. 200).

The argument is, of course, a familiar one. For obvious reasons it has not commanded widespread support since the 1920s. Nor is it likely to acquire new stature as a result of this restatement.

LLOYD M. WELLS

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The Politics of Neglect: Urban Aid From Model Cities to Revenue Sharing. By Bernard J. Frieden and Marshall Kaplan. (Cambridge, Mass.: The MIT Press, 1975. Pp. 281. $14.95.)

The Frieden and Kaplan volume should interest anyone concerned with urban policy, poverty policy, and policy analysis in general. In addition, as a case study of the federal management of the Model Cities Program during the Johnson and Nixon administrations, it illustrates the gap between presidential initiative and implementation of domestic policy in a manner that is also instructive for those concerned with public administration or the American presidency.

The authors were among those who participated directly in the development of national efforts to remedy urban ills. *The Politics of Neglect* is based on their experience, interviews with leading actors in the process, and a reading of relevant materials on urban aid during the past decade. It is the authors' contention that the central contemporary domestic problem remains "the capacity of the federal government to respond to the needs of the poor and minorities in the cities" (p. 1). Because that capacity was put to an extreme test in the Model Cities Program the authors have developed a well-documented, clearly written analytic history of the program as a case study through which the nature and limits of federal performance may be seen. Through this careful reading of the federal implementation effort, it is their intention "to define a future role for the federal government in reducing poverty and inequality, drawing on the experience of the 1960s but without repeating the overly optimistic assumptions and mistakes of that decade" (p. 4).

From the establishment in 1965 of the Department of Housing and Urban Development and a presidentially appointed task force to advise on organization and programs for the new department, to the emasculation and demise of Model Cities under President Nixon, the authors conclude that reformers were unable to understand or appreciate the limits our decentralized, pluralistic political system places on centralized coordination of major social programs. It is the basic view of the authors that

> the underlying problem ... was not in the technicalities of program design.... Members of the task force, in common with many of their contemporaries, had a naive belief in the power of the President to reshape not only the federal government but the world outside Washington. They counted on a sustained White House commitment to social reform and placed great faith in the ability of rational planning to find solutions to the problems of the cities. They also accepted White House assurances that substantial new funds would be available for domestic programs, despite the competing claims of the Vietnam War.... The stubborn facts of interest group politics, limited budgets, bureaucratic resistance to change, and the extreme pluralism of American society faded away before the image of a well-conceived federal blueprint for the slums, backed by a President with enormous power.
>
> This view of the world could not be sustained very long. Even the course of Congressional debate and enactment of the legislation began to reveal the limits of the power of the Chief Executive (p. 36).

Model Cities was an innovation in response to the consequences of decentralization and fragmentation of federal aid programs. Thus, the authors greatly exaggerate the naiveté of the task force, which knew that it was involved in a complex task, but thought it was worth trying to respond to the challenge. The task force was charged with setting goals to achieve what are fundamentally impossible ends. They acquitted themselves well under the circumstances. The subsequent emasculation of their program was inevitable. No less "naive" an approach could have achieved the intended result either (as the authors tacitly admit in their own inability to find a viable alternative). The distasteful paradox that faces all reformers is that the problems of the poor and minorities in the cities are integrally related to the American economic, social, political and basic value systems, and that without major restructuring of these systems only sporadic and incremental change in the conditions of the poor and urban minority groups is possible.

While Frieden and Kaplan ignore the impact of economic, social, and value systems on urban policy (a widespread weakness in the work of political scientists), they recognize the limitations imposed by the political system. However, they do not realize that they are presenting antithetical suggestions for future policy. They recommend improvement of the system of cash transfers to the poor, either through a guaranteed annual income or through a rationalization and extension of the present series of cash and in-kind programs. This would certainly benefit the poor, and modest improvements will probably occur here. However, the major thrust of their reform proposal is of doubtful value.

To benefit low-income groups and poverty neighborhoods the authors would place primary reliance on various aspects of revenue sharing. Yet the history of revenue sharing indicates that programs designed and administered by federal agencies are far more likely to be responsive to the authors' intended constituency (urban poor and minorities) than are those that depend more heavily on state or local agencies. It was, in fact, President Nixon's revenue-sharing efforts (both general and special) that put the final coup de grace on several Johnson poverty-related programs. The Nixon/Ford programs of revenue sharing have shifted resources away from inner-city poor and minorities to middle-class groups. A substantial body of research indicates that any other outcome of general or special revenue sharing has been, and is likely to continue to be, improbable.

DOROTHY BUCKTON JAMES

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The Twilight of Capitalism. By Michael Harrington. (New York: Simon and Schuster, 1976. Pp. 446. $10.95.)

Business Civilization in Decline. By Robert L. Heilbroner. (New York: Norton, 1976. Pp. 127. $6.95.)

The Cultural Contradictions of Capitalism. By Daniel Bell. (New York: Basic Books, 1976. Pp. xvi + 301. $12.95.)

Anyone who has ever toiled in that vineyard we call "Introduction to American Government" knows that the first class is always the hardest. The challenge is to present a plausible context for all the material that will follow. My own formulation runs something like this: the United States is a capitalist society. (Not a "pure" specimen, to be sure; but did anyone ever claim that it was?) This being the case, the governmental structure should be seen as carrying out various functions which are useful for this sort of economic system. We will be spending the semester studying the politics of a capitalist democracy. I add that "capitalist" should be seen not as an adjective of opprobrium, but rather a realistic description of our social and cultural setting.

Not everyone will find favor with this rendering. Indeed, most political scientists I know prefer to call ours a mixed economy, with a panoply of welfare programs. Thus large parts of many introductory courses will be devoted to demonstrating how government regulates business, oversees the economy, and promulgates social goals. Perhaps in the past we had some kind of capitalism. What we have today needs something more than a caption.

This is a thoroughly respectable position, so much so in fact that it can claim immunity from any ideology. After all, to see ours as a welfare state, a mixed economy, or a pluralist society is not really "left" or "right," "radical" or "reactionary," in any stereotyped sense. Certainly most of us feel comfortable if we can assert that we are not ideologically beholden. People who purchase their philosophies in packages are obviously not thinking for themselves.

All this said, one advantage of orienting a course to its capitalist context is that it can use almost any available textbook. Regard the issues we are asked to argue. Ought abortions to be outlawed, regulated, or erased from the statutes altogether? Do we want deposits on our beverage bottles, fluoride in our drinking water, or reinstatement of capital punishment? Should we settle for the current defense budget, or try to wring out $7 billion worth of water? Needless to say, inflation and unemployment are also issues. And well they might be, as conditions endemic to capitalism. So are poverty, race, and crime. While such problems predate capitalism, the system has a lot to do with ensuring their persistence. (Especially as it lacks the capacity to provide jobs for major segments of the citizenry.)

The capitalist focus does *not* mean detailing how this corporation or that industry obtains favored treatment from officialdom. Such an approach elaborates what is altogether obvious, and encourages intellectual indolence. The point, rather, is that the political process simply assumes the centrality of business. For the nation to function effectively, we want General Motors to be prosperous and Exxon to enjoy good health. What other options have we got?

Government exists to run the errands which business decides it can delegate, like fighting fires, operating prisons, and delivering junk mail to rural box-holders. Ours is a "capitalist government" in that its chief role is to work at cleaning up messes created in the wake of private enterprise. Still, it must not do its job too efficiently. After all, official ineptitude works to the benefit of business. It shows we had best not give the state any tasks that are really important.

All this is by way of introduction for these three books on American capitalism. Together they make up an excellent reference shelf on our contemporary political economy. (Sadly, we no longer call our subject by that name; with the result that we know much less than we ought about the workings of economic power.) All three authors are prepared to admit that capitalism is very much alive, even if not in the finest of moral fettle. It is also worth noting that while the three occupy academic chairs–in economics, sociology, and political science. They prefer to address lay audiences, usually in journals of some circulation. Thus all of the books contain material which appeared in earlier incarnations as articles.

A further point deserves emphasis. Harrington, Heilbroner, and Bell are men of superior intellectual attainment. By this I mean they are smarter than the average run of political scientists, including those endowed with professional reputations. They are equally at home in philosophy, history, and literature; in their writings you watch informed intellects at work. Their approach is not so much inter- or multi- or even supra-disciplinary, as one which eschews campus categories and pits one man's mind against the world. They tend to be the sort of people we invite over for visiting lectures; any longer stay on the premises might show how limited the rest of us are. In short, they have better brains than most readers of this review. Any critical comments which follow should be read in this context of respect.

Harrington's current book is primarily an homage to Marx. It is a rereading of the master, showing a sympathy that manages to avoid sycophancy. His purpose is to find modern meaning in Marx, acknowledging that anyone who has been dead for 94 years may need a little help from his friends. Harrington begins with the laudable premise that Marx was neither blind nor obtuse. To those prone to intone that "Marx failed to see" something apparent to just about everyone else, Harrington shows that he was perfectly well aware of whatever it was he is supposed to have overlooked. What we end up with may be less Marx himself than a disciple's interpretation of his mentor. But that is what we will always have to settle for, at least until texts can interpret themselves. Harrington's achievement is to retain Marx as an instrument for understanding contemporary American capitalism. We may need other tools as well. But the Marxian foundation has fewer fissures than many think.

I wish that Harrington had extended his exegesis one step further, to the interfaces where technology and psychology meet. The advance of industry, Marx said, would reveal a revolutionary contradiction: a conflict between the technical rationality of our machines and the moral irrationality of private ownership. Is such a realization occurring right now, rousing some variant of a working-class consciousness? A case can be made that this is happening, although here, too, Marx will need amending to fit the ways our minds work now. If we are to reach the socialism Harrington hopes for, then we must know how today's capitalist culture is schooling us for a new form of citizenship.

While a harsh enough critic of capitalism, Heilbroner seems happier when he can dissociate himself from Marx. Not the least reason is his belief that politics can develop a motor power of its own, and thus transform major contours of the economy. Heilbroner's stress is on achieving meaningful economic planning, a goal Marxists see as the stale half of a very small loaf. Indeed, he admits the truth of that position. "The next phase of capitalism," he says, will see a "drift towards a business-government state" (p. 34). In fact, "corporations will have a major influence in the determination of any national plan" (p. 36). So we are moving towards a planned capitalism, with capitalism doing most of the planning.

But even this won't last. Our business civilization has run out of steam, and shows no signs of regenerating a renewal. Heilbroner prophesies gradual decline and decay, but is unwilling to guess what comes next. His strength lies in dissecting the power of corporate capital, including its overseas satellites. Can even a politically mobilized populace rein in so sprawling a megalith? Here, too, Heilbroner seems less than sure.

Anything written by Daniel Bell, an established authority of our time, warrants an attentive reading. As one of the coiners of "end of ideology," he represents an important ideological school: that nexus of liberalism and conservatism so pervasive along the banks of the Charles. Bell's learning reaches polymathic proportions. It often seems as if an average page contains at least one statistical table, an allusion to a minor work of Hegel, a quotation from Virginia Woolf, and a hypothesis of quantum physics. The mind grows groggy with citations; to Wilhelm Dilthey and Susan Sontag, Anne Sexton and Georges Sorel, Mark Rothko and

Huey Newton. Alongside Bell, we are all illiterates.

Bell too calls what we have capitalism. More than that, his title implies a dialectic still busily at work. However we are now in the clutches of a new, "post-industrial," contradiction. Capitalist technology calls for rational intellects: the willingness and ability to think systematically and in theoretical terms. However the culture we have created is increasingly emotional, sensual, self-indulgent. This is indeed a contradiction, if not quite so epochal as the ones Marx had in mind.

Bell makes no bones about where he stands in this stand-off. He is on the side of system, theory, rationality; and he seems to see any opposing tendencies as way-stations on the road to collapse. Attractions to astrology and the occult could well weaken the structure Bell and others have so labored to build. Yet at one point (p. 72) he indicates that "in its products and its advertisements, the corporation promotes pleasure, instant joy, relaxing, and letting go." So it does. The expansion of leisure time, discretionary income, and the range of personal experiences—all mean it is in capitalism's interest to encourage new spheres of enjoyment. Fly now, pay later.

So a Marxist might argue that it is the rationality of capitalist production which spurs the hedonism Bell finds so deplorable. We have streetcorner loungers, unneeded by contemporary technology; a student stratum, kept unproductive until well into adulthood; even corporate managers and engineers, eager to go through the gestures of swinging. Here are human products, byproducts, and rejects of modern society's substructure. Whether their indulgences will undermine the edifice is itself an intriguing question. But the step Bell cannot take is to say that a culture of fads and fashions arises from the structure he so esteems. To admit that, would be to discover that the seedlings he so disdains were sown in his own backyard.

So let me end where I began: I particularly recommend these three books to anyone looking for a framework for Political Science 101. Most of the others we have been using refer to a Rip Van Winkle world. Try calling it "capitalism" and see what happens.

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The Sex Education Controversy: A Study of Politics, Education, and Morality. By James Hottois and Neal A. Milner. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. 136. $14.00.)

The debate over sex education in the public schools has been a lively one. Because of this, James Hottois and Neal A. Milner saw the issue as a means to test community power theories. The book, then, does not focus on why individuals support or oppose sex education nor on the content of sex-education programs. Instead the authors investigate innovation, professionalism, policy-making techniques, and the presence of, and opposition to, sex-education programs, looking toward the means of discontent management.

In a reflexive style, the authors outline the development of their framework toward conflicts arising over sex education. They came to ponder who should make authoritative decisions regarding sex education and how universal these decisions should be, scrutinizing tolerance on the part of both proponents and opponents. They also found themselves concerned about the process of decision making, about the possibility and meaning of participation in the face of professionalism, about the impact of wide-scale participation on innovation and change. In the course of their study, their focus moved from a major emphasis on how an intense minority can be prohibited from imposing its will over a less intense majority to an equal concern with how empathically policy makers can deal with such an intense minority. The discussion in advance of the presentation of empirical findings is dense, not in the sense of hard to understand but rather in the sense of many ideas closely woven. In these respects, as well as in the authors' insight into how their own values of participation, innovation, and tolerance clash in regard to sex-education policy, the book is provocative and worthwhile. The data and discussion thereof are less satisfactory.

To answer the questions raised about conflict mitigation, innovation, and social control, the authors gathered data from a mail questionnaire sent to school superintendents of all districts with a minimum 5,000 student enrollment in 1968 and from two case studies. Of the 1,708 questionnaires mailed, 540 were returned, a response rate of 31.6 percent. The authors, in the appendix, argue that the low response rate, and its skewedness in terms of geographical distribution and wealth of responding districts, enhanced the probability that sex education had been considered in the districts responding, but had no independent impact upon presence or absence of community conflict. This assertion is arguable. Another potential limitation of the questionnaire findings arises from the use of a single informant per district. No attempt at independent verification of sex education program consideration or opposition was undertaken. Since the single informant, the district superintendent, had an institutional position to protect, s/he may have responded so as to enhance her/his handling of the situation; further, the tendency to have done so may vary systematically with professionalism-cosmopolitanism, considered as independent variables on the introduction and handling of sex education qua innovation. The case studies, however, usefully illuminate some of the claims derived from questionnaire data.

Other methodological problems in *The Sex Education Controversy* relate to presentation of information and construction of measures. For example, in attempting to discover how a perceived need for this innovation, sex education, developed, Hottois and Milner content-analyze professional journal articles on sex education. They state that approximately one-quarter (or 88) of the articles were used; no basis for selecting these articles from the population of articles is given. More troubling, since this affects fundamentally the conclusions drawn, is the construction of measures. For example, six educational innovations, for which, again, no selection criteria are given, constitute an "innovation index." The "index" is additive with each innovation counting one. In later analysis, a two-value variable, innovativeness, is used, constructed by finding the mean number of innovations for all districts and using this mean to dichotomize districts into "high" and "low" innovativeness.

As a result of their investigation of the sex education controversy, Hottois and Milner conclude that the issue is moral and the imposition of values by both sides ought to be reduced. Sex education, in their view, ought thus to be voluntary. They see no "compelling state interest" which would require such a program to be compulsory. They fault proponents who fail to recognize that they have value commitments, yet characterize opponents as trying to perpetrate their morality on others. While the reevaluation of the value positions involved in this controversy is useful, it remains that any policy, voluntary or mandatory, represents a value position and has consequences on the allocation of rewards and deprivations. Given their recommendation, students, like many direct clientele groups, would not represent their interests in any systematic fashion. Further, the liberal voluntarist "solution" places little weight on the personal and social costs of unwanted pregnancies, venereal disease, and other situations at least partially associated with sexual ignorance.

PATRICIA BAYER RICHARD

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The Effective President. Edited by John C. Hoy and Melvin H. Bernstein. (Pacific Palisades, Calif.: Palisades Publishers, 1976. Pp. xi + 183. $8.95, cloth; $4.95, paper.)

The 14 essays in this book grew out of a symposium on the presidency held at the University of California (Irvine) early in 1976. Together they constitute a good introduction for undergraduates to many of the basic problems facing the presidency and to some of the contradictory ideas about how to solve them.

The editors declare the theme of the essays to be "that our institutions . . . have served the nation well . . . and should not be tampered with" (p. 16). Their introduction is nonetheless followed by George Reedy's assertion that "there are obviously structural changes that could or should be made" in the presidential office (p. 20). Still, Reedy identifies the basic problem as lying "within our society itself" (p. 22) rather than in its institutions. This idea is similarly to be found in the essays by Mervin Field and George Gallup, who identify the quality the public wants most in a president as honesty, rather than leadership or ability to assess ideas, and in that by Grant Dillman, vice-president of UPI, who views our real problem as the electorate's "insistence on instant solutions where in fact there is none" (p. 128).

There thus appears to be general superficial agreement that something is amiss in the body politic. Accepting that as creating regrettable but unchangeable limitations, the authors address themselves to the specifics of institutional tinkering. John Anderson claims to perceive a recent congressional resurrection; Dillman labels it evanescent. Dillman suggests ways of restructuring of the bureaucracy and complementary congressional committees, and the establishment of a "clearing house" to provide legislators with information; Stephen Horn suggests that a party career ladder be created, by which younger members would begin as administrative assistants in various bureaucracies and rise to assistant secretaries in later administrations; Stephen Hess advocates a smaller White House staff; Thomas Cronin prefers long-range planning to "adhocery" (p. 150); Hubert Humphrey proposes a Council of Governors which would work directly with the president. Stronger parties are recurrently advocated, yet there is little indication of how to achieve them, nor is there any discussion of Karl Lamb's assertion that "the media . . . are taking over the traditional functions of the party in naming the candidate" (p. 91) by deciding what percentage of a primary vote is necessary to prove that a candidate has "won" and by drying up a prospective candidate's financial sources by reporting polls that show the impossibility of victory.

The book, in short, reproduces many of the milder suggestions and disagreements that surfaced during the 1976 campaign. Unfortunately, it lights upon but does not confront the larger sociological questions, nor does it deal with the broader reforms that have been proposed elsewhere. Reedy, for example, asserts that in American society "people no longer identify themselves on the basis of a series of economic interests" (p. 26) and that our system, which now needs human beings only as consumers, includes "very large numbers of people who no longer can really feel that what they are doing is making a difference" (p. 29). Surely this has implications for the political process, ranging from popular perception of presidential actions to the kinds of party appeals most likely to succeed. The basic problem that our political institutions were created for a society which has undergone many changes is not addressed here. The possibility of a symbiotic relationship between institutions and electorate is not considered; no one asks: to what extent are the electorate's perceptions and expectations affected by its knowledge of what its political institutions are likely to produce? Similarly, no one mentions the provocative suggestions raised in such works as Charles Hardin's *Presidential Power and Accountability* (such as installing the defeated presidential candidate as leader of the opposition in Congress or the nomination of presidential candidates by congressional candidates). Other good suggestions not mentioned are found in Louis Fisher's *Presidential Spending Power*, or even Jimmy Carter's *Why Not the Best?* and Walter Mondale's *The Accountability of Power*, the last two advocating that cabinet members be subjected to public questioning by Congress. Neither is there reference to Representative Henry Reuss' proposed constitutional amendments for creation of a separate office of chief of state and for a vote of no confidence, nor of the *George Washington Law Review* symposium on Reuss' proposals (January, 1975). Some of these suggestions seem more feasible than others, but all represent the kind of creative thinking that can serve as the stimulus for further such thought. These are the kinds of ideas with which undergraduates, as well as their teachers, should be grappling. As a primer to prepare one for such ideas, however, *The Effective President* should serve students well.

PHILIPPA STRUM

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Are Government Organizations Immortal? By Herbert Kaufman. (Washington, D.C.: The Brookings Institution, 1976. Pp. viii + 79. $2.50, paper.)

In this slim volume, Herbert Kaufman explores the mysterious question of organizational births and deaths in the federal government. He compares the list of agencies in existence in ten major departments and the Executive Office of the President in 1923 and 1973, leaving out the Department of Defense and the Post Office. While admittedly leaving out much—independent agencies of all types, field offices, sub-bureau units—the number of agencies encompassed nevertheless reach the formidable total of 421. The methodological questions are formidable also, such as how to treat an agency like AID that has had as many incarnations as national administrations. Kaufman strives to emphasize continuity rather than change because of his incentive to discover change and turnover and thereby overturn conventional wisdom about the seeming immortality of government agencies.

Kaufman has discovered that government agencies are indeed durable and persistent. Of the 175 organizations in the 1923 sample, only 27 or 15 percent have disappeared. At the same time vast increases have occurred in the number of organizations within the departments existing in 1923 and in those created subsequently—a total of 246—leading to a total of 394 agencies surviving in 1973.

The author examines a number of interesting subsidiary issues. He finds that staff agencies increased more rapidly than line agencies; that older agencies had as good a chance of survival as new ones, if not better; that the existence of a statute as the basis for organizational protection provides no more security than does an executive order or departmental action; that there has been considerable variation in the mortality of organizational units from department to department (Agriculture was highest with 34 percent); and that organizational growth has not occurred evenly but in spurts which appear largely to reflect changes in political and economic conditions and not presidential idiosyncracies.

Kaufman first outlines the possible reasons both for organizational deaths and organizational survival. These reasons include both agency responsiveness to its external environment as well as agency or government-wide strategy to ensure an agency's survival or its demise. The lists on both sides seem complete and reasonable but, on balance, suggest that organizational existence might be perilous. Kaufman discovered, however, that six of the seven hazards to organizational existence accounts for the demise of the agencies (competition, change of leadership and policy, obsolescence, completion of mission, statutory authority to change the structure) but that chance appears to play as great a role as any of the definable hazards. The data do not suggest why certain agencies have not survived when others similarly situated and beset by enemies and changing forces in society have survived. Age itself appears linked (albeit weakly) to organizational survival, i.e., the older the agency, the greater the likelihood of its persistence. But Kaufman can only conclude on the evidence that some agencies find a stable niche and survive within it while others are flexible enough to respond to rapidly changing social conditions.

Kaufman draws out the implications of his findings in terms of what the future holds for organizational growth. Extrapolating existing trends leads to projections of exponential growth. While appropriately suspicious of such projections, Kaufman nevertheless foresees an awesome task of control of agency births, facilitation of agency deaths, or management of a vastly more complicated structure. He is not sanguine about any of the possible means of control of growth. He is particularly critical of time-limited authorizations which threaten an organization's existence every stated number of years. The evidence with respect to annual authorizations of agency programs at the federal level would appear to support his argument. On the other hand, state efforts to impose such controls would bear watching for verification of his position.

This is a useful first-cut of understanding of the bureaucratic "problem" as defined by both conservatives and liberals today: the tendency of bureaucracy to grow on the basis of some internal logic or metabolism that may or may not correspond to social need. It is a necessary cut, although a limited one, because it did not address important ancillary questions. Structure and function do not necessarily coincide (as Kaufman explicitly states) so that the death of an agency may not mean a reduction of the "problem"—the function may be simply transferred to another unit or some lower level of the organization. Whether governmental efficiency is thereby improved or society better served remains an open question. Secondly, it does not deal with functional utility of federal agencies that are moribund and yet persist for various reasons: symbolic (their existence means something to someone); friendships or political ties (old cronies have positions in these agencies). Anyone familiar with the Washington scene can identify such agencies still in existence despite the malodorous examples of the Subversive Activities Control Board and the Federal Metal and Non-Metallic Mine Safety Board of Review. Moreover, it does not deal with agencies in which the diligence in pursuit of social goals is less than all-consuming, thus ensuring that the functions of those agencies will persist beyond the retirement times for existing commissioners or board members. It may be argued that some agencies have a half-life comparable to that of plutonium wastes.

Kaufman defines the research and social problem more narrowly in terms of the development of organizational theory and social controls. By further efforts to understand births and deaths in the bureaucracy, we may advance our understanding of organizational change as well as the problems and needs of organizational control mechanisms. These are worthwhile goals and we hope that, in addition to others such as those indicated above, they will be pursued despite their complexity and difficulty of execution.

DEAN E. MANN

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The Joint Chiefs of Staff: The First Twenty-five Years. By Lawrence J. Korb. (Bloomington & London: Indiana University Press, 1976. Pp. xiii + 269. $10.95.)

The U.S. Joint Chiefs of Staff (JCS) in its first 25 years emerged as one of the most controversial and least understood elements of the American political system. Neither political nor apolitical, neither so strong that it controls the country nor so weak and divided that it is never consulted, the JCS reflects largely the strengths and weaknesses of any bureaucratic structure set up by elected officials. The 25-year experience has been analyzed crisply by

Lawrence Korb—who approaches the JCS as individuals and as a composite, and examines their political and operational roles.

Legally, the JCS are the principal military advisors to the president, to the National Security Council (NSC), and to the secretary of defense and the Congress; second, they assist the president and secretary of defense in their responsibilities as commander-in-chief and deputy; third, they prepare strategic and logistic plans which provide guidance in developing the defense budget, military aid programs, industrial mobilization plans, research and development programs and contingency plans of the combat commands. Fourth, they review plans, programs and requirements of the army, navy, air force and the unified theater commanders.

Korb gives workmanlike descriptions of the first two functions, the complex and frequently misunderstood advisory roles. He judges the third and fourth functions, preparing and reviewing plans, to be "routine and in need of no further elaboration" (p. 7). This statement seems inconsistent with his conclusion that "the chiefs have shown themselves to be poor planners" and "not innovators in the policy process, even in military areas: . . . even when a situation cries out for change the JCS remains addicted to the status quo" (pp. 180–81).

It is precisely because the JCS sees its role in preparing and reviewing plans as "routine" that its strategic planning function is unsatisfactory. The 1958 Amendments to the National Security Act were Eisenhower's attempt to correct JCS planning weaknesses which he had learned at first hand. Recognizing that separate sea, land and air warfare were gone forever, the law removed the services from major planning responsibilities, placing the task in the JCS and unified commanders. Combat forces were mission-oriented; the army, navy, and air force retained responsibility only to "provide, train and equip" forces for the unified commands. Eisenhower emphasized the point in his special message to Congress, 3 April 1958:

> complete unity in our strategic planning and operational direction [is vital]. It is therefore mandatory that the initiative for this planning and direction rest not with the separate services but directly with the Secretary of Defense and his operational advisors, the Joint Chiefs of Staff. . . .
>
> No military task is of greater importance than the development of strategic plans. . . . Genuine unity is indispensible at this starting point. No amount of subsequent coordination can eliminate duplication or doctrinal conflicts which are intruded into the first shaping of military programs.

The responsibility for "the first shaping" of strategic planning and doctrinal development by the JCS—repeated three times in the Eisenhower message—nevertheless was delegated by the JCS to the services where large and prestigious staffs continue to dominate planning considerations. On the navy staff today, for example, are a dozen flag officers whose primary role involves long-range strategic planning and objectives.

Joint doctrinal guidance under the circumstances shows neither coordination nor consistency, underplaying joint roles and propagandizing service values. The outsider cannot conceive, for example, that despite seven years of costly war in Vietnam, involving the loss of thousands of combat and support aircraft, pilots, and crews, no approved joint doctrine has yet evolved for tactical air support or for control and employment of certain types of fixed and rotary wing aircraft.

The Eisenhower era reforms in the national security structure were not implemented because the members of the JCS give primary loyalty to the services which they head. As service chiefs they will not surrender the necessary power to the JCS. As members of the JCS they limit their advice to the president, NSC and secretary of defense to the purely military aspects of a problem. When that advice is unresponsive to political, economic or domestic reality, the organization itself is immediately suspect. What is necessary in both the advisory and planning roles is less obvious than the manner of achievement. Is the JCS willing to reorganize itself to the extent necessary to perform effectively in its statutory planning responsibilities? Korb points the way; the real issue is yet to be faced.

PAUL R. SCHRATZ

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The Participatory Bureaucracy: Women and Minorities in a More Representative Public Service. By Harry Kranz. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. xxii + 244. $17.50.)

"Since the body and mind of government is lodged in its nonelected fourth branch, the bureaucracy must be vested with the heart and soul of representative government by making it a true replica of the nation's polity in which all groups are included and participate." Harry Kranz finds that bureaucracy in America has already achieved hegemony, finds that attempts to control it by elected officials, Congress and courts are inadequate and that suggestions such as those of Friedrich and Finer for control are ineffective, and regards the power of bureaucracy as irreversible and desirable. In fact, he describes as demagogic diarrhea the proposals of politicians to reduce the extent of the bureaucratic estate. Since the bureaucracy, according to the author, is here to stay, is all-powerful and cannot be controlled from without, it must be controlled from within. "A participatory democracy would promote self-rule, the soul of U.S. democracy." The way to this happy state is to make the bureaucracy by agency and grade at all levels of government an adequate statistical reflection of the country's population, leaving out, one supposes, children, idiots, morons, and some classes of criminals. For starters, statistical adequacy with respect to blacks, Spanish-speaking, Indians, Asians, and women would do the trick.

According to the author, "as an accurate mirror and echo of the nation's sociological and geographic diversity, its [the bureaucracy's] *authority* to govern for all would be unquestioned." Further, "The bureaucracy would be legitimated because it would be legitimate—the true genuine representative institution." Many who might share the author's views as to the desirability and the feasibility of upgrading the participation of blacks, Spanish-speaking, Indians, Asians, and women in public employment will doubt his estimate of the power of the bureaucracy and the potential of descriptive representation to control it in a manner as significantly beneficial a manner as he envisages. The justifiable criticism the author levels at the empirically unverified assertions of some of his predecessors in the field has not served to restrain him from the same temptation.

However, whatever one's doubts as to the book's more grandiose pretentions, it has the very significant merit of contributing much information hitherto not readily available about the levels of employment by agency and grade for blacks, Spanish-speaking, Indians, Asians and women in federal, state, local and special district employment. In this array he finds the federal government to be by no means the model employer one might expect. The Civil Service Commission comes in for some harsh and seemingly well-deserved criticism for its failure to lead, and even for its sabotage of efforts to improve the nation's employment record. The "myth" of the merit system receives a pretty convincing debunking. The author shows rather persuasively that a true merit system in accordance with the Griggs decision is feasible, and outlines methods that seem to hold promise for its attainment. While the author's larger claims for his version of participatory bureaucracy may seem unconvincing and unsupported by any evidence, the justice and desirability of equitable access to all public employment for those who have been so blatantly discriminated against in the past and even the present are well argued and amply documented.

NORTON E. LONG

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Political Power in Poor Neighborhoods. By Curt Lamb. (New York: Halsted, 1975. Pp. xxii + 315. $19.50, cloth; $9.50, paper.)

The central question in this book is the extent and impact of political revitalization among the American poor. In particular, Lamb attempts to understand the role played by militancy, political culture, ethnic and racial awakening, various types of community groups, and the status of women in catalyzing political mobilization and institutional change. His major conclusion, hardly novel but worth continued restatement, is that militant political action is the most effective strategy for bringing about change in poor neighborhoods. However, it is the way in which he comes to and elaborates this finding that makes the book well worth reading for students of the powerless.

Lamb explores these issues with the Local Change Study, a systematic survey of over 8000 residents and community leaders in 100 poor neighborhoods, a study developed and employed by Barss Reitzel and Associates, Inc. in the late 1960s for the Office of Economic Opportunity. The wealth of empirical data provided by this survey enables Lamb to test his hypotheses against hard facts. And it is from these facts that his conclusions about the prerequisites of political revitalization emerge.

The data leads him to discover that there has been relatively little institutional change in the 100 neighborhoods under investigation. Where change has occurred it has generally been in communities whose political culture is "pragmatic militant," combining a high degree of protest with more conventional tactics, coordination among groups, broadly based organizations, and committed leadership. "Defiant militant" communities, lacking commitment to conventional tactics or coordination among groups, rank second. The other types of communities, "reformist," "passive center," and "idle radical," experience significantly less institutional change.

Lamb next proceeds to determine the sources of political mobilization among the people themselves. Looking first at their attitudes and values, he finds only two possible ideological sources of political energy: (1) a nascent radicalism based on alienation from local government; and (2) among blacks, a "pragmatic racism" that enables then to perceive racial antagonism but also to recognize and accept racial good will. Lamb then looks at neighborhood leadership and refutes the oft-made assumption that poor neighborhoods lack the necessary leadership resources to sustain organizational activity.

It is on the question of group membership, however, that Lamb makes his greatest contribution. On the one hand, he finds that group life among the poor is relatively impoverished and that their traditional community organizations, the churches and the political parties, have become nearly irrelevant to the enterprise of community development. Nonetheless, there is mobilization based on membership in activist groups and particularly in the new and increasingly numerous "militant ethnic groups." In comparison with members of civil rights, CAP-sponsored, or other social action community groups, the militant ethnics, white as well as black or brown, have the strongest political commitment. Their educational achievement is the highest of all the action groups and their leaders the only ones whose incomes approximate their constituents'. Thus, says Lamb, "Ethnic militants, with high education, low income, and dedication to class interests, seem more deeply committed to the needs of the poor" (p. 131). Lamb forces us to see that the source of change in the lives of poor people rests on a combination of "pragmatic" radicalism and cultural (ethnic or racial) solidarity.

Finally, Lamb explores the political status and orientation of poor women. He finds that poor women are even less mobilized than poor men; they are less likely to be angry, radical, activist, or politically involved. Moreover, there is little evidence that radicalism in poor neighborhoods has promoted the equality of liberation of women there (his "radical egalitarianism" hypothesis); a high degree of issue and task specialization continues to differentiate the sexes. Nor has the women's movement had much effect on either the status or political consciousness of poor women. Only among black women, and particularly black women leaders, have sex roles begun to break down, but even there women have a far road to go.

The chapter on women best illustrates both the strengths and weaknesses of Lamb's approach. He has tackled the subject, in this case one seldom attempted by analysts of the poor, and through the Local Change Study tested his hypotheses. However, not all of his hypotheses are well founded; the one concerning "radical egalitarianism" is problematic at best. Moreover, the data are so attitudinal that he cannot document socioeconomic forces, events, or specificities that might actually demonstrate the extent to which the status of poor women has or has not changed. Finally, in this chapter especially, the discussion of the literature is repetitious and often meaningless. Nonetheless, he tried.

Other problems that plague this book include the definitions of the subject of inquiry and the style of its presentation. What exactly is the meaning of "political revitalization"? Does it equal "mobilization for effective political action"? And what, then, is "effective political action"? The style of argumentation, like the definitions, is also incomplete. Lamb has a tendency to taxonomy, useful at times but as often trivial or even obscurantist. More importantly, by focusing only on the poor in his investigation, he neglects the very great constraints imposed from outside their own communities. He does this in a spirit of demonstrating their strengths and of arguing for the involvement, once again, of the federal government in building militant community groups. However, the result is the appearance of misunderstanding of the class and governmental forces opposing the poor.

Despite his failings, Lamb deserves some plaudits on several counts. Few enough scholars have even made the effort to analyze empirically important value questions, and fewer still have drawn from such an evaluation the strategic implications for those interested in change. Lamb attempts to do both. Moreover, he has made a contribution to the growing documentation of conflict theory. Once more we are forced to recognize that, in this society at least, confrontation, protest, and militant demands, not compliance or patience, are the sources of change. Finally, and not least, Lamb makes the reader acknowledge the role of militant ethnics, black, white, and brown, in providing the base for the types of action groups that are most likely to bring about change. What follows from his findings is that poor people can be mobilized politically and can effect institutional change. The requirements include: a pragmatic radicalism (that is, some underlying belief in the system and its responsiveness); ethnic and racial awakening; the availability of information on local issues; committed leadership; coordination of activist organizations; *and* militant direct action.

MARGARET LEVI

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The Persistent Poppy: A Computer-Aided Search for Heroin Policy. By Gilbert Levin, Edward B. Roberts, and Gary B. Hirsch. (Cambridge, Mass.: Ballinger, 1975. Pp. xvi + 229. Price not given.)

System dynamics, pioneered by Jay Forrester and made famous through *The Limits to Growth,* is extended in this book to a behavioral problem—control of heroin addiction and the crime it entails under American laws. The authors, a community psychiatrist and two systems specialists, use the computer modeling method to generate alternative 25-year futures predicted by their assumptions under various policies. They believe the results of their electronic experiments justify some confident, but undogmatic recommendations.

The substantial, but intellectually manageable complexity of the heroin system would seem to warrant the simulation approach. Levin, Roberts, and Hirsch use 324 equations to gauge the effects of seven policy variables on three major outcomes (crime rate, number of addicts, and cost of programs). Between policies and outcomes intervene sometimes conflicting mechanisms involving such varied factors as heroin purity and price, community definitions of the problem, migration in and out of the community, and the attractiveness of the addict lifestyle.

Generally, however, the policy implications are what one would expect given the authors' premises. For example, they assume that at most 40 percent of addicts can be attracted to drug-free therapeutic communities and that, of these, 30 percent or fewer can be rehabilitated. For methadone maintenance, their corresponding figures are 60 percent and 60 percent. Not surprisingly, methadone-oriented policy results in dramatically less crime at lower cost with only moderately more addicts. The parameters used are consistent with available evidence; but the skeptical may ask, given the initial numbers, why the need for fancy computer work?

One might defend the effort by pointing to occasional counter-intuitive results. In the simulations, the conventionally virtuous policy of cracking down on pushers rather than on ordinary addicts actually increases crime, because of higher drug prices and the fact that many former addict-pushers resort to thievery (p. 99). The chief value of the technique, however, is that it forces one to be absolutely explicit about variables, mechanisms, and numbers. You get out what you put in, but at least you know what you're putting in. Or, as Levin et al., put it in their admirable introductory chapter, "its unique advantage is that all questionable assumptions are explicit and therefore open to the influence of debate and further investigations" (p. 16).

There is room for criticism here. Drug statistics are notoriously unreliable, even concerning such basic matters as the number of addicts in a community. Consequently, many of the functions and parameters are only casually supported—sometimes necessarily so, as when the authors dare to include and quantity such variables as the "futility-despair level." But those who question the study's conclusions should take the trouble to study its assumptions and show which are least reasonable.

What are those policy conclusions? Essentially, the authors urge, first, steady, but moderate police efforts aimed at both suppliers and addicts, and, second, strong reliance on methadone maintenance. They expect little from drug education (except education to change community attitudes toward a medical definition of the problem), narcotic antagonists, or drug-free rehabilitation. They reject heroin maintenance, preferring more crime (with methadone) to more addicts (with government-dispensed heroin), though they obscure the nature of this choice with auxiliary arguments not derived from the model.

These results should be a valuable guide to policy, provided that we regard them not as definitive conclusions, but as interim results from an iterative process. In that spirit, I would suggest several modifications that might be considered in future runs: (a) Instead of assuming the youthful proportion of the population constant, examine the effects of birth bulges like the one that has just passed through the drug-susceptible age. (b) Incorporate more precisely into the equations our knowledge of the epidemiology of heroin addiction, a topic about which relatively advanced theory exists. (c) Include as a policy variable the unemployment rate, to try to gauge how responsive addiction might be to national economic policies as well as to local drug-focused efforts.

Other readers should be able to propose additional refinements, joining the authors in their "series of conversations with the evolving model." Specialists in drug and crime policy who might be frightened off by computers and equations should not be deterred. The text of *The Persistent Poppy* explains the study's assumptions and results in clear, nontechnical prose, with the aid of diagrams and tables. Equations and more detailed explanations appear in an 86-page appendix, but this also should be accessible to general readers willing to devote some time and effort.

JACK H. NAGEL

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The Promise of Greatness: The Social Programs of the Last Decade and Their Major Achievements. By Sar A. Levitan and Robert Taggart. (Cambridge, Mass.: Harvard University Press, 1976. Pp. ix + 316. $15.00.)

The Great Society's social programs are currently the subject of numerous revisionist interpretations. Levitan and Taggart's *The Promise of Greatness*, one of the best of these efforts, provides an important rebuttal to Edward Banfield's *Unheavenly City*. Using studies done primarily for the national government, Levitan and Taggart evaluate the results of individual social programs (income support, health care, low income housing, compensatory education, manpower, civil rights, and community organization), and their combined effects on poverty, blacks, and the economy.

The purpose of this overview is to make a case for renewed governmental activism, a message well timed for the Jimmy Carter administration. The authors assert that negative assessments of Great Society programs have contributed to the "social slowdown" of the mid-1970s and they think a more balanced assessment of the programs will contribute to new initiatives. In their opinion the evidence shows that the programs had a "massive, overwhelmingly beneficial impact" (p. viii). Their argument is that the Great Society's goals for improving the nation were realistic, the programs had positive results and were reasonably efficient, the critics overstated their negative impacts and underrated their benefits, and finally, despite the Nixon administration's efforts to check the growth of these social programs, the improvements have continued (pp. 7–10).

A subsidiary theme is the limitation of policy analysis. Levitan and Taggart suggest that the spate of policy evaluations alleging programs had failed were used as "proof" by policy makers and the public who ignored the limitations of the research. The authors admit that they too executed such studies on the assumption that the only question was how the programs could be improved. But their criticisms were often used to provide a negative answer to a different question, the question of whether programs should be continued. The authors emphasize the importance of null hypothesis selection in policy analysis. They argue that policy analysts should "assume success until failure is reasonably demonstrated" (p. 276) but the critics of the Great Society chose instead to assume failure until success was unequivocally proved. Levitan and Taggart, as economists, also provide valuable critiques of evaluation techniques (typically used by economists) which lead to negative program assessments and of major negative reports such as HUD's National Housing Policy Review and Labor's studies of institutional training under the Manpower Development and Training Act.

The first major section of the book lucidly assesses the criticisms of individual social programs by reviewing program results. The chapter summarizing the various types of income supports emphasizes that the controversial Aid to Families with Dependent Children should be viewed in the perspective of other income-support programs such as social security, Supplemental Security Income, unemployment compensation, food stamps, and veterans' pensions and compensation. The authors acknowledge the problems of benefit inequities and work disincentives but argue that many reform proposals create more problems than they solve. They conclude that social security remains secure, the growth of AFDC has slowed, and the support system is functioning reasonably well because developments in the last decade have assured "at least a poverty threshold standard of living" (p. 260).

The chapters on health and housing programs include interesting reviews of the arguments against both in-kind aid and cash transfers. The following chapters on human resource programs, namely education and manpower, contrast nicely with each other. The authors think that evidence on the value of education programs is weakest and on the value of manpower is strongest. They conclude that in primary and secondary education there is no clear evidence whether the improvements resulting from programs are enough to warrant the cost. But in manpower programs where the outputs are quantifiable, the authors think the results show conclusively that improved wages and job stability outweigh the program costs.

The second major section of the book synthesizes diverse indicators of the overall impact of the programs. The chapters on the effect of the programs on poverty and blacks argue that government efforts have reduced the number of poor people and improved the socioeconomic status of blacks, though progress for blacks has slowed as government pressure has eased. The chapter on economic impact denies that middle- or lower-middle-income families paid for the modest redistribution which did occur and says that any redistribution was from the affluent to the poor. In discussing impact on economic growth, the authors maintain that economic growth is compatible with full employment and income redistribution and they point to the favorable economic conditions in the 1960s as evidence. They argue that the deleterious economic effects of the Great Society have been exaggerated and imply that Nixon administration economic policies and associated economic theories made major contributions to the unemployment, inflation, and economic slowdown which followed the Great Society. But the authors do not fully explore the relative contribution of these various factors. They concentrate on countering the case against the Great Society rather than on formulating an alternative explanation of economic trends.

Levitan and Taggart are well aware of the limitations associated with their general policy overview. Systematic assessment of underlying problems suffers. The authors' political and economic theories are left implicit. What patterns are there across policies? What do these patterns reveal about the power of the players in the political system and the resulting distribution of benefits and institutional change? How much difference does the type of policy make in formulation, implementation, and outcome? Similarly, what do these patterns reveal about the operation of the economic system? The book raises a great many important questions which it does not address. A related limitation is the minimal use and citation of academic studies. For example, no reference is made to Banfield even though the authors seem to be directing their arguments in his direction. A wider range of policy-related studies may have contributed to the climate described in the book and they should be considered. This wider scope would help the reader assess whether policy studies have had the great influence which Levitan and Taggart attribute to them (and thus whether revised assessments can change the climate). Another limitation is that Levitan's and Taggart's reform proposals are extremely general. They advocate modest, positive, constructive steps which build on rather than repudiate the social programs of the Great Society. But the authors do not specify the outlines of these policies.

The general character of the book is both its strength and its weakness. The purpose of this revisionist interpretation is not to provide a map but simply to provide a directional sign and in this it has admirably succeeded. Like Banfield, the authors call for lowered expectations but they mean lowered expectations about program results so that satisfaction will rise along with a willingness to continue governmental action to create a better society.

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Presidential Power and the Constitution: Essays by Edward S. Corwin. Edited with an introduction by Richard Loss. (Ithaca, N.Y.: Cornell University Press, 1976. Pp. xx + 185. $12.50.)

This collection "forms a bridge" to Corwin's *The President: Office and Power* (p. x). The editor's splendid introduction, 12 essays on the presidency from Wilson to Truman, and addenda to the published bibliography of Corwin's writings complete the volume.

The "natural law background" to the Constitution justified limited government, according to Corwin. Constitutional law, a system of restraint on government, was based on dual federalism, separation of powers, due process, judicial review, and the "notion that government exists for the purpose of supplementing and reinforcing the non-political control of society, and especially those which rest on social superiority and economic power" (p. 155). Only the maintenance of constitutional principles within which precedents must be squared to be valid, gave the assurance of liberty.

Limited government was impaired, claimed Corwin, with the end of dual federalism, the growth of presidential initiative in legislation, congressional delegation of legislative powers, the creation of the IRC's, and judicial review becoming an instrument of policy based on "will and power rather than a doctrine based on knowledge" (p. 150).A theory emerged holding that the purpose of government was to "correct the non-political control of society from the point of view of the theory of equality of man. Fusion of government functions translates . . . into a policy of social leveling" (p. 155). This gives the assurance of security, but results in a constitutional theory of executive power. Separation of powers is "shaky if not altogether defunct" (p. 149).

Since the Constitution delineated the structure and power of the national government, Corwin maintained presidential power must be based on constitutional provisions or customary practice squared with constitutional principles.

The provision that the "executive be vested in a President" was no grant of power, but was "simply to settle the question whether the executive should be plural or single and to give the executive a title" (p. 121). The president's duty is "to create an administration and maintain one for the faithful execution of the laws." But he is not responsible for the "executive department and control of all subordinate executive officers" (p. 87). Presidential authority to conduct foreign relations did not require the "Senate's consenting to any unnecessary abdication of its participation in the shaping of our diplomatic policies" (p. 31). The precedents for executive leadership in legislation and presidential dictatorship in time of war were established by Wilson. In war it may "be necessary to suspend the Constitution" (p. 167).

Corwin was confident that separation of powers safeguarded against executive usurpation even though the framers had conferred on the president "all the prerogatives of monarchy in connection with war-making" except declaring war and creating armed forces (p. 23). The Commander in Chief clause is "the elastic block in the closed circle of constitutionalism; in the heat of war the powers it confers are capable of expanding tremendously, but upon the restoration of normal conditions they shrink with equal rapidity" (p. 23). By 1943, Corwin recognized the "elastic block" was incapable of returning to its initial form, because "emergency war powers are assimilated to peacetime powers of government" (p. 120). War, crisis, emergency, and presidential personality moulded the Constitution.

Inter alia, the solution to executive dominance was "timely action by Congress to see that no power vacuum occurs," and a "new Cabinet to control, support, but neutralize the presidency politically" (p. 175). It seems not to have occurred to Corwin that the president would neutralize Congress and the "new Cabinet politically."

The Constitution delineates the structure of *political* authority, and not the structure and power of the national government, as Corwin maintained. Some presidential power is personal, and a great deal comes from the selection process. The president's position at the intersection, gaps, or overlaps in the system of political authority allows him to make policy where the authoritative basis for action is ambiguous. Consequently, the Constitution changes, and the Supreme Court has the authority to determine its meaning. This produces a widening gap between the constitutional principles and the content of the law, as its content becomes more and more the product of the justices. The rationalization of the law is a political, not purely legal, process.

Behind constitutional questions are political questions, and a constitutional-legal approach to presidential power helps to understand the questions necessary to comprehend the development of the political order. Corwin's "elastic block in the closed circle of constitutionalism" turned out to be a "royal garter" fitting any presidential leg, however deformed. Nixon not only snapped the "garter," he all but cracked the "elastic block" when, announcing the invasion of Cambodia, he said, "I shall meet my responsibility as Commander in Chief." By stretching their imaginations and the "elastic block," modern presidents claimed "prerogative" supported the turning of law enforcement agencies into law violators and using illegal violence in the name of national security.

MYRON Q. HALE

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Many Sovereign States: A Case for Strengthening State Government—An Insider's Account. By Dan W. Lufkin. (New York: David McKay, 1975. Pp. 253. $7.95.)

Dan W. Lufkin is the kind of successful citizen turned temporary bureaucrat that has been prominent in Washington but less so in state capitals and city halls. He went from a partnership in a Wall Street investment banking firm with a part-time interest in ecology to a two-year stint as Connecticut's first Commissioner of Environmental Protection. In this book he describes his personal route into and out of state government. He offers a wealth of anecdotes about his contacts with citizens and other government officials during the formative years of his new agency. As the title suggests, he also expresses strong views about the proper roles of state governments in the federal system.

Lufkin is at his best in illuminating the human problems and opportunities of policy makers in state governments. As the first head of a new department, he had a great opportunity to witness the important years of program and organizational formation. For those who want a feel for how activities move from legislation to implementation, this book has much good material. There are insights into the design of regulatory standards to fit both local needs and the requirements of national legislation; the pursuit of the all-important first budget in an incremental system that depends on what was spent last year; plus the host of problems created by the petty routines that control the flow of a government's resources. On these traits Lufkin's book is a state-level equivalent of Marver Bernstein's *The Job of the Federal Executive.* It portrays the state administrator at work amid conditions peculiar to his level of government.

Lufler feels that states deserve a strong role in the formulation and implementation of policy, especially in areas that "impinge on human experience on a direct, daily basis" (p. 185). By these he means education, highways, public health, police protection, welfare, parks and recreation facilities, and pollution control. He would leave the "big decisions . . . which can have devastating long-range impact on the society" to the federal government. Yet the impact of education can be devastating in the long run. Lufkin also has trouble in handling both the priority of national standards and state discretion in the welfare field.

Reading this book, one senses the influence of Lufkin's background. He was a successful businessman outraged about environmental deprivations, but with a concern to be moderate in the fact of conflicting pressures. He expresses little patience with single-minded reformers—Ralph Nader in particular—who fail to consider the disadvantages that may flow from their primary goals. It is difficult for any self-professed moderate to express his own goals and recommendations in general terms, insofar as everything depends so heavily on circumstances. Lufkin is not skillful in his handling of abstract issues, and often fails to recognize the problems that lie beneath his own prescriptions. In two successive pages he argues for general federal policy guidelines to be administered by states and cities, and for the right of states to make broad policy decisions (pp. 116–17). Elsewhere he casually tosses off sweeping judgments about programs and personalities. A serious student of state government should approach this book with caution. There is much good description and occasional insight about the problems of state officials and the role of states in the larger system. A generous reader should focus on these points, without being distracted by the author's lapses into casual analysis.

IRA SHARKANSKY

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Policy Analysis in Public Policymaking. By Thomas D. Lynch. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. x + 117. $12.50.)

The objective of this short study is to "clarify what can be reasonably expected of policy analysis as it relates to governmental decision making" (p. ix). Specifically, the focus is the utilization of PPB in three policy areas in the Department of Transportation. The work has an inadequately developed research design, but does offer some useful observations about the use of formal policy analysis in policy making. The study's conclusion is that in only one policy area—the aids to navigation program in the Coast Guard—is formal policy analysis used in making policy. He finds that in this area, the PPB process was followed in the decision making and was used as an integral aspect of policy making.

What does it mean to say that PPB is used as an "integral" part of the policy-making process? One difficulty in answering this question is that throughout the book the terms "PPB" and "policy analysis" are used interchangeably. It is generally accepted that the special feature of the PPB idea is the grouping of expenditures into a program structure. But as the author states, "no case study involved the use of the program structure as a part of an analysis or as part of decision making" (p. 72). Void of program structure-based analysis, PPB becomes a general label for an analysis process of setting objectives, constructing reasoned relationships between ends and means, and costing out the proposed alternatives.

To what extent, then, is policy analysis PPB found in the case examples contained in this volume? Before the Coast Guard decided which of several radio navigation aids to adopt, outside contractors performed extensive technical studies of the performance capabilities of the different radio systems. The full format of PPB submissions were reportedly prepared for this policy issue, but there is no discussion of how the broad Coast Guard PPB objectives of waterborne traffic facilitation and safety were incorporated into the technical analysis as a PPB analysis would require.

The second Coast Guard example concerns a series of decisions about the design and maintenance of offshore buoys. One part, dealing with buoy tenders, posited three alternatives involving various mixes of renovation or construction of new offshore tenders. What evidence do we have that PPB played a role in making this policy? These alternatives were presented in prescribed PPB form. Undoubtedly, this case example shows that PPB stimulated analytic thinking and caused the relevant information to be presented in prescribed logical PPB form. But whether this constitutes an "integral" role for PPB in policy making in terms of influencing the outcome or improving the quality of decisions is not shown in this study.

Why were analytic studies used at all in these case examples? As the author points out in the summary, this policy-making process was suitable for formal analytic input because it was dealing with highly technical issues, because it was not highly political, and was done in an agency generally supportive of policy analysis. As the remaining case examples show, in the absence of these conditions it is difficult for formal analytic input to policy making.

Two case studies involved subsidy programs for both system development and operation of urban mass transportation. In concluding that PPB was used as an aid in making critical decisions on these programs the author simply means that the PPB staff supplied important information, special analytical studies, and general input to the policy debate. Clearly this does not constitute policy making through PPB. Probably the more interesting question is why rigorous analysis in general played such a limited role in these decisions. The conclusion here is that "... the key decision makers involved did not feel the necessity for such analysis in both cases. The preconceived attitude toward the usefulness of policy analysis by those who influence and make policy is apparently significant in explaining the role of policy analysis in public policymaking" (p. 47).

The two remaining case examples examined in this study involved research programs on urban transportation, one of which was initially conducted in HUD prior to the creation of the Department of Transportation. The New Systems program to develop newer forms of urban transportation entailed no PPB role whatsoever. This non-use of PPB is explained as a predictable result of the strong political mandate for doing something for urban transportation, as well as the general non-use of PPB in HUD. The tracked Air Cushion Vehicle program also involved no PPB role, attributable to the strong personal interest in the program by the secretary of DOT and the frequent and rapid changes in direction of the program, leaving no time for analysis.

JOHN N. COLLINS

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Slavery, Race and the American Revolution. By Duncan J. MacLeod. (New York: Cambridge University Press, 1975. Pp. 249. $15.95, cloth; $5.95, paper.)

Duncan MacLeod is Lecturer in American History and Fellow of St. Catherine's College, Oxford. And he knows his American history thoroughly, especially the primary and secondary sources for this book.

Slavery, Race and the American Revolution treats a large and important topic, the reciprocal effects of the American Revolution and Negro slavery on one another:

> The Revolutionary period and its immediate aftermath was a watershed in the development of a multiracial America. On one side of the watershed slavery was a racial institution, but not avowedly so; on the other, its racial nature was openly asserted. On one side of the watershed racial discrimination was largely implicit and very largely defined by slavery; on the other, it was explicit and existed independently of slavery. Between the 1750s and the 1820s the main lines of the sectional conflict which was to erupt in civil war were laid down (p. 12).

David Brion Davis and Winthrop D. Jordan have heretofore studied this subject most suggestively, but Davis' two books and Jordan's one treat the Revolution as an incident, however crucial, in the unfolding of their respective topics: the development of the antislavery movement, and the rise of American racism. Davis and Jordan also obscure the nature of revolutionary thought on slavery and freedom by their psychological determinism. Both use the methods of intellectual history, but neither believes that ideas are more than expressions of cultural and psychological imperatives. Davis looks for tensions and stresses to explain the emergence and nature of abolitionism, and Jordan develops the idea that white racism is an irrational function of suppressed sexual guilt. MacLeod, on the other hand, understands that slavery was an intensely practical matter for half the United States. He anticipated Edmund S. Morgan's excellent more recent book, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (1975), by showing that the tangible liberties of white southerners actually depended on black slavery; in their society it was far from being incidental or anomalous. Therefore MacLeod quotes and analyzes the speeches of John Randolph of Roanoke and the political writings of John Taylor of Caroline, brilliant provincials who spoke the mind of Virginia and the South, while Jefferson (the central figure for Jordan and Davis) more typically tried to sublimate his southernness in national or universal postures.

The most original section of MacLeod's book considers a series of judicial cases which established whether certain slaves had a right to freedom based on descent from a free mother. MacLeod shows that in the years immediately following the Revolution certain state courts showed a decided bias in favor of freedom by accepting as valid evidence local traditions which could be nothing but hearsay. After 1810, however, courts reverted to strict rules of evidence, and judicial manumission accordingly declined. This supports the view that southerners generally became more conscious of the need to defend slavery well before the rise of militant abolitionism.

The last chapter (save the conclusion) is less masterful and original in redefining the work of MacLeod's predecessors, though it maintains his virtues of succinctness and clarity. Following Jordan, he notes the growth of racism in a scientific literature which persistently defined Africans as inferior in mental and moral endowments, and following Leon Litwack he notes the prejudice and ill-treatment of blacks in states that abolished slavery. Like Jordan, MacLeod believes that "sexual insecurity hovered near the center of the web of anti-Negro prejudice," (p. 161) and further believes that there was a "rape complex" (p. 178) in early white southern society. But the evidence cited for these things is fragmentary and inconclusive: in the late eighteenth and early nineteenth century, servants, whether black or white, were considered weak in their moral consciences, but not sexually aggressive. Gentlemen, whether European or American, were society's most disruptive sexual aggressors. If there was a "rape complex" before the Civil War it showed up in antislavery circles, where charges of sexual immorality were standard in the indictment of slaveholders. But even this is not evidence of a "complex" unless it can be shown that abolitionists merely imagined the crimes they attributed to slaveholders, or that they seized on a few rare cases and grossly exaggerated them. The mere mention of sex, or sexual crimes, is not in itself evidence of complexes or irrationality at all.

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Congress: The Electoral Connection. By David R. Mayhew. (New Haven: Yale University Press, 1975. Pp. 194. $7.95.)

For a number of years now, political scientists have lamented the atheoretical nature of the literature on Congress. Scholars seeking a theoretical and conceptual perspective on Congress invariably refer to Woodrow Wilson's *Congressional Government.* Since Wilson's study, few scholars have attempted to develop a systematic, theoretically coherent, overarching interpretation of how Congress operates, the reasons it operates as it does, and the significance of congressional organization and behavior for the changing role of Congress as a representative assembly. While congressional literature has been rich in empirical studies of narrow but important questions regarding congressional elections, roll-call votes, committee decision making or party leadership, students of Congress have failed to integrate this literature in a persuasive and logical manner.

In *Congress: The Electoral Connection,* David Mayhew takes up the mantle from Woodrow Wilson and presents a brilliantly provocative interpretation of congressional organization and behavior. Drawing for support on the recent empirical literature, and utilizing the analytic perspectives and tools of formal theorists, Mayhew builds an argument based on the central assumption that members of Congress are single-minded seekers of reelection. Because of this preoccupation with reelection, Congress members necessarily emphasize position-taking, credit-claiming and advertising rather than policy making. To facilitate such credit-claiming, position-taking and advertising, members create a committee system that disperses decision-making authority so that members with different types of constituencies can sit on committees serving their particularized constituency interests, thereby helping to ensure their reelection.

For Congress to process broad-scale policy decisions and thereby maintain its institutional integrity, its members do establish a small number of "control" committees that run Congress and exercise decision-making authority in difficult policy arenas without immediate reelection payoffs. Since these control committees may not directly serve individual members' reelection interests, but rather may involve them in controversy and time-consuming work that could threaten their reelection, members of Congress will not voluntarily serve on these committees without additional incentives. To fill these committees and keep Congress afloat institutionally, and to release other members to serve their reelection interests, a subset of members are placed on control committees, given power and prestige as rewards, and allowed to exercise the broad policy prerogatives of Congress. This system both enhances the career security of most members and ensures the institutional integrity of Congress.

In addition, the system appears generally amenable to democratic government because the preoccupation with reelection builds into the system a natural responsiveness to the wishes of the electorate, particularly demands for the distribution of particularized benefits.

Mayhew's interpretation is ingenious and initially persuasive. The book is beautifully written, and its argument concise, elegant and well documented by references to recent empirical studies. However, I have reservations about Mayhew's work as an explanation of congressional organization and behavior. The immediate test of such a theory is the argument's logic and the fit between the empirical propositions generated by the theory and real-world observations. It is with the logic and empirical fit that I take issue. My objections are twofold.

First, if the most commonly held goal of members of Congress is to be reelected, then how can a subset of members be "bought off" by power and prestige, particularly when service on a power or policy committee may undermine reelection security, as Mayhew himself acknowledges? Assumedly, under such conditions, single-minded reelection seekers would view power and prestige as worthless currency. This logical difficulty is particularly troubling because of an empirical observation: the literature on congressional committee assignments emphasizes that most members prefer to serve on policy or power committees rather than on reelection committees (utilizing Fenno's typology). So many members appear willing to be "bought off," in other words, that one must question the extent to which there really exists an overriding preoccupation with reelection. Reelection may be an initial goal of members, and a factor normally considered in political decisions, but it does not seem an adequate basis for explaining committee structure or member behavior. It would appear that control committees are able to exist, and members seek to serve on them, because of a concern with congressional power, a concern so fundamental that the members voluntarily leave reelection committees.

The second major problem with Mayhew's argument is its implicit static image of Congress. He describes a decentralized system of committee government lacking both overarching leadership from centralized party organs and budgetary coordination by a central budget committee. This picture does mesh well with congressional reality during the 1960s, the period on which Mayhew relies for empirical support. But this view of Congress is an inaccurate characterization if applied to the late nineteenth or early twentieth century; it is also inaccurate today. Mayhew's book was published just as reforms occurred within Congress to strengthen party leadership and create a centralized budgetary process. There is little suggestion in the book that such changes are possible; the argument suggests, rather, that the reelection motive permanently locks Congress into the mold of the 1960s. In fact, moves toward party government or towards centralized budgetary processes seem virtually impossible within Mayhew's framework because both would inhibit the decision-making autonomy and flexibility of members, thus undermining their opportunity to take policy positions congruent with personal electoral interests. The primary response to this criticism is the counterargument, implicit in the book, that members of Congress see the power and prestige of Congress as a "collective good." They thus move periodically to reform it in ways conducive to shoring up or increasing the power and prestige of the institution and thus of themselves, even if the end result may weaken members' electoral security. Once again, Mayhew's salvation comes not with a stress on reelection but with a stress on the power motive.

After having read Mayhew's book a number of times and having reflected on the arguments, I am led to one conclusion: if one must ultimately resort to a stress on a power motive as a way to "save" the arguments—both the argument about the existence and maintenance of control committees and the argument (which I have derived from Mayhew) about centralizing reforms—then the implication is that power is a more fundamental long-run goal than reelection. The reelection motive may be a highly salient subcomponent of the power quest: without reelection, members cannot attain power. But attempts to generate a coherent, logical, empirically sound theory of congressional organization and behavior should consider shifting from an emphasis on reelection toward an emphasis on the power motive. Such a shift does not at all suggest that Mayhew's observations are unfounded or useless. Just the opposite. Because reelection is a necessary component of the drive for power, his characterizations of congressional behavior—the emphasis on position-taking, advertising, credit-claiming, particularized benefits—all capture a significant element of truth that must be included in any congressional theory. In additional, we must recognize that reelection security may be an important requisite for centralizing congressional reforms. Before members of Congress can feel free to strengthen party leaders or create a centralized budgetary process, perhaps they must feel reasonably secure in their electoral careers. Mayhew's work documenting the decline in congressional marginality, in combination with the emphasis in this book on the importance of reelection concerns, thus may help us understand why the recent reforms came in the 1970s rather than in the 1950s. Nevertheless, I am increasingly convinced that the development of a coherent theory of Congress requires an emphasis on the power motive as fundamental, with the reelection motive as a necessary component.

Congress: The Electoral Connection is still a landmark book providing a wealth of provocative ideas and insights. Much of what Mayhew sais is appealing. Even in the areas where I have questioned his analysis, his case is still strong, not to be dismissed easily in a few lines in a review. And clearly Mayhew's ideas and arguments must be assessed by any serious student of Congress. He is the first modern congressional scholar to rediscover the path towards broad-scale theory and interpretation that Woodrow Wilson left for us almost a century ago. It is to Mayhew's credit that—in Mayhew has broken out the predominantly empiricist mode of modern congressional scholarship—and has had the courage to delve into theory construction. I suspect that he has unlocked the magic door and that congressional scholars will now move in mass towards a decade of theory and development and argumentation.

LAWRENCE C. DODD

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Policy Analysts in the Bureaucracy. By Arnold J. Meltsner. (Berkeley: University of California Press, 1976. Pp. x + 310. $10.00.)

At a time of a depressed job market for social scientists, one of the few related growth fields has been policy analysis. There is still a demand for policy analysts—economists, political scientists, and sociologists with quantitative and communications skills that can be applied to the formulation of public policy. Indeed, whatever the great social programs of the 1960s did for the poor, they were a boon for enterprising social scientists. Arnold Meltsner has provided a useful service in writing a comprehensive account of this new breed of professional. Meltsner's book lies somewhere between extended case studies like Moynihan's *Maximum Feasible Misunderstanding* and systematic analyses of the use of information in bureaucracies like Wilensky's *Organizational Intelligence*. It is a sometimes rambling amalgamation of war stories, descriptive accounts of the role of analysts in particular policy decisions, and clear, hard-headed advice to analysts.

Meltsner does not define policy analysis. Indeed, his whole book can be taken as an inclusive definition of the term. His approach "is to illustrate the current state of policy analysis by examining the political and bureaucratic aspects of policy analysts at work" (p. 3). To this end, Meltsner draws on his interviews with policy analysts in Washington as well as on his own experience as a government consultant, and describes three types of analysts: technicians, whom one envisions with pocket calculators strapped to their belts; politicians, for whom analytical elegance is less important than political clout; and entrepreneurs whom Meltsner obviously favors as a beneficent combination of the other two types.

Meltsner proceeds to examine the work and working environment of policy analysts from a variety of perspectives, including the selection of the topic to be worked on, the process by which studies are conducted, the relationship between analysts and their "clients," and the use of policy analysis by policy makers. There is, however, no central theme, no coherent line of argument on which to peg a discussion of this book. Instead, Meltsner's discussion is peppered with statements like, "no definition of policy analysis will sufficiently encompass the staff activity of the analyst. Whether providing advice or a service, comfort or support, the policy analyst is largely what his clients want him to be" (p. 80). Any policy analyst reading this book would, at one point or another recognize himself or herself because seemingly all varieties of experience are offered with little indication of what, if anything, is the dominant mode of behavior. Thus, some analysts have to sell their proposals to reluctant analysts while others enjoy a more enthusiastic reception.

As a sort of "cookbook" for analysts, present and aspiring, Meltsner's work succeeds well. There is insight and advice for these readers. But, to a large extent, that is as far as it goes. The book's greatest failing is its theoretical weakness. While Meltsner does use typologies of behavior, as in classifying different kinds of analysts, those classifications are only descriptive with no attempt to relate them to the remainder of his discussion. More importantly, apart from occasionally citing the standard sources he makes no attempt to relate his findings to the intriguing theories of information, advice, and communications in bureaucracy, or to theories of organizational behavior generally. Meltsner's is the most comprehensive first-hand view of policy analysts in the bureaucracy, and yet we are left without any attempt on his part to evaluate various modes of analysis in terms of the likely quality of advice.

By missing the rather obvious theoretical questions, we are left with a mildly interesting collection of tales from the bureaucracy and some useful perceptions of the advisory process in the bowels of the executive departments. This, however, still leaves unanswered some rather important policy questions. Apart from the pronouncement that "knowledge and its purveyors can enhance public policy" (p. 265), there is no attempt to assess the utility of the whole enterprise of policy analysis as conducted by the federal government. This is not to be confused with assessing the impact of analysis which Meltsner does address with the conclusion that sometimes there is an impact from particular studies and sometimes not.

Throughout this book, Meltsner describes the attempts of analysts to bring their studies to the attention of relevant officials. The overall image projected is that of hundreds of analysts scurrying around, dreaming up projects, and trying to interest and influence their political superiors. Is this system of self-promotion by policy planning and evaluation offices the optimal way for the government to apply knowledge to social problems? At the risk of appearing philistine, is it possible that so much paper is being generated that no tree can be seen through the forest?

Then there is the normative dimension. With a remarkable lack of self-consciousness, Meltsner uses the term "clients" to refer to those for whom analyses are done. But the general thrust of his description is reminiscent of an account of record promoters hustling disc jockeys. This salesmanship gives rise to some pointed questions. If the services of policy analysts are really valuable, why do analysts have to sell them so hard? The picture of officials on the public payroll drumming up business to justify their existence is not a pretty one.

Finally, there is an uncomfortable hint of hubris in this book. While Meltsner discusses policy analysis from the perspective of domestic policy, it should also be remembered that in the 1960s there was a concomitant increase in the use of analysts in national security. Among the "best and the brightest" who brought us Vietnam were policy analysts using such marvelously quantifiable data as body counts. Although policy should be made in light of a full array of factual data, a little humility and common sense also should not be overlooked.

MARK V. NADEL

Senate Government Operations Committee

The Virginia Supreme Court: An Institutional and Political Analysis. By Thomas R. Morris. (Charlottesville: The University Press of Virginia, 1975. Pp. 188. $12.50.)

Thomas Morris states (p. x) that he has singled out the following questions for examination: How has the Virginia Supreme Court acquired its place in the political system? Who gets on the court? What are the court's procedures? What types of cases does the court decide? What is the court's relationship with other agencies of government? What decisions does it make and of what significance are they? The answers recorded are almost exclusively ideographic. Factual data for each of the topical subdivisions are arranged in chronological sequence, with very little analysis of the significance of the data for the political role of the court.

Because most of the book is a history of the Virginia Supreme Court, readers interested in such detail will find the book useful. A wealth of factual material is included in the text and in the appendices on the constitutional development of the court, the characteristics and the selections of the justices, court procedures, litigants before the court, and the relationships of the court to other governmental agencies, divisions, and subdivisions.

As political analysis, the book is much less useful, partly because of the low political profile of the court through the years. Despite this low profile, the court on many occasions was a useful political tool of the former Byrd machine in the accomplishment of its goals and programs. Morris mentions a few of these instances, but they tend to be lost in the historical, descriptive orientation of the book. The final chapter is very abbreviated, and Morris ends without giving us a meaningful analysis of the political role of the court. The richness of the book is thereby considerably diminished.

CHESTER W. BAIN

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Improving the Legal Process. By Stuart S. Nagel. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. vii + 394. $15.00.)

Stuart S. Nagel's *Improving the Legal Pro-*

cess builds on his previous work, *The Legal Process from a Behavioral Perspective* (Dorsey, 1969). Nagel states: "the purpose of this book is to bring together a series of studies all of which involve applying social science method, theory, and substance to controversial issues regarding the effects of alternative legal policies designed in some ways to improve the legal process" (p. xvii). It builds on Nagel's earlier volume by moving from a discussion primarily of the stimuli elements which determine both lawmaking and adjudication responses to a more comprehensive concern with lawmaking and adjudication as a stimuli to subsequent responses. Those familiar with Nagel's work will recognize chapters dealing with women as litigants, reapportionment, and sentencing disparities.

Nagel uses a consistent methodology throughout the book and provides a most welcome concise but thorough first chapter on methodology described as using "a system of correlation analysis to choose among conflicting legal policies that are primarily regulatory rather than subsidizing policies, and using a system of linear programming for choosing among conflicting legal policies that primarily involve allocating scarce governmental resources" (p. 4). The discussion permits those not thoroughly schooled in current empirical tools used by social scientists in policy-analysis research to appreciate the discussion. The analysis also demonstrates that empirical social science research can provide policy makers with data and analyses that could be potentially helpful to decision makers in analyzing policy alternatives. Good examples are the chapters on "Minimizing Costs and Maximizing Benefits in Providing Legal Services to the Poor" (13), "Measuring Unnecessary Delay: The Actual versus the Predicted" (14), and "Incentives for Compliance with Environmental Law" (15). These chapters not only break new ground methodologically, they more importantly provide general principles that can be expanded by other researchers and used by policy makers and appliers in "more efficiently allocating scarce governmental resources in a large variety of legal policy situations" (p. 300).

In addition to breaking new ground in the above areas, Nagel's updated treatment of judicial selection is a most welcome addition to the debate over judicial selection. After a thorough discussion of various selection systems based on empirical studies, Nagel concludes "that maybe elected and appointed judicial systems do not differ as much in their results or in the behavior of voters and appointers as the debate literature would have us believe" (p. 230). This coincides with other recent studies (see Berg, et al., *Western Political Quarterly*, June 1975) and again raises questions about adoption of the appointive system currently advocated by the organized bar, among others. As Nagel so aptly points out, one needs to look at the empirical data and then conduct an evaluative comparison of the various systems that include an awareness of what goals one considers desirable and how much weight to give each goal. For example, "if one highly values liberalism and public participation but does not so highly value nonpartisanship and technical competence, he may be more likely to prefer an elected system to an appointed system" (p. 232). In addition, Nagel points out that if one has "a hierarchy of values and has access to raw or analyzed data, one should be able to make a policy choice that will better achieve one's goals on this controversial issue of elected versus appointed judicial systems" (p. 234).

This important book makes other contributions to the field, but several minor shortcomings should be noted. First, no one has given adequate attention to updating thoroughly some of the studies. This is most evident in the discussion of United States Supreme Court Justices (data base prior to John F. Kennedy), but the footnotes and text too often reflect data from the 1950s and 1960s.

Secondly, more direct attention should be given to the role and impact of organized political groups deeply involved in the policy-making process such as the organized bar, judges, prosecutors, and law enforcement. In other policy areas such as the environment, multinational corporations and corporate and union power in general need to be considered more thoroughly.

Perhaps future work in this area should be expanded to include not just primarily policy alternatives, but consideration of alternatives to existing institutions in *both* the public and private sector. These comments probably reflect the current state of the literature but they do indicate the need for broadening research efforts to include other important political factors in the policy-making process.

Nagel has taken us farther down the road of understanding. He continues to build on the past, breaks new ground in several areas of policy analysis and indicates by his style of presenting complex methodology and analysis that it can be done in an understandable way. The work also demonstrates that research by social scientists can be potentially helpful to policy makers and appliers and can provide empirical evidence for use in general public discussion of policy alternatives. These are very substantial contributions.

LARRY L. BERG

University of Southern California

Congressional Power: Congress and Social Change. By Gary Orfield. (New York: Harcourt Brace, 1975. Pp. xii + 339. $4.95, paper.)

Gary Orfield's book, *Congressional Power: Congress and Social Change* articulates a provocative thesis, namely, that Congress is both more "progressive" and innovative than most observers believe. To those of us whose thinking about Congress has been shaped by the writings of James MacGregor Burns and Richard Bolling, this book represents a major departure from the view that congressional affairs are dominated by conservative southern oligarchs whose power is protected by the seniority system and the filibuster. If we accept Orfield's view, lectures on Congress will no longer be replete with "gallows humor" tales of congressional ineptitude and unresponsiveness to changing circumstances.

Orfield's philosophical stance, like that of most other congressional commentators who admit to having one, is liberal. The major difference between his book and theirs is that they see Congress as having only the potential to redeem itself; Orfield contends that redemption has arrived.

Through his careful and incisive analysis of congressional case histories in three policy areas—civil rights, education, and employment—Orfield argues that "the Congressional process may be a good deal more responsive to certain kinds of social change than the executive branch" (p. 306). It is on this point that Orfield is most directly in conflict with conventional liberal critiques of Congress.

Orfield points out that the two elections of Richard Nixon indicate that there is nothing inherently "liberal" in the institutional structure of presidential elections. Also, the domestic program of the Nixon administration in the areas of antibusing legislation, urban assistance, educational support, and equal rights for women casts serious doubt upon conventional liberal interpretations about the role of the presidency in mobilizing support for "progressive" social programs.

In civil rights and education, he sees that it was a congressional majority which prevented the dismantling of Great Society programs by the Nixon administration. And in employment, it was Congress and not the president whose efforts launched the public jobs program of 1971. It is this reading of events which supports his thesis about congressional liberalism and responsiveness.

For these reasons, he feels that Congress has been subjected to unfair criticism by liberal reformers. As he points out, "Congress has changed, but perceptions of Congress remain largely fixed" (p. 9). It is these "obsolete images" of congressional conservatism and inattentiveness which need changing and not the internal rules which govern Congress. Orfield contends that there is no guarantee that reforming congressional procedures will increase the "progressive" quality of social policy. This contention may cause Mr. Orfield difficulty with Common Cause.

Orfield anticipates criticism on this point and he warns liberal reformers that they may be suffering from a delusion regarding the extent to which their beliefs about the objectives of social policy are shared by the electorate. Drawing from public opinion data, Orfield demonstrates that the public has no clear ideological stance. Thus, it is not surprising that "Congressional policy-making is often characterized by inconsistent or even contradictory policy directions" (p. 280). Congress may be responding appropriately to the public's mixed messages.

On the responsiveness question, Orfield argues that "this criticism is often based upon a very unrepresentative selection of issues" (p. 50). However, he realizes that his own argument about Congress may also have been buttressed by a selective reading of the record. "While the case studies in this book have illustrated Congressional activism in the face of a conservative President," he points out, "a scholar wishing to prove Congressional impotence might prepare apparently contradictory studies on Vietnam or Welfare policy" (p. 258). And he goes on to say, "Obviously, the conditions and the limitations of Congressional policy initiatives must be clearly specified" (p. 258).

It is this point which I believe is Orfield's most important one. He states quite clearly that this book is designed to add dimension to the conventional perceptions of Congress. It is not intended to replace the congressional-passivity portrait with a congressional-activist one, but rather to indicate that no firm generalizations can be made about interactions which are as dependent upon the personal element as are those between Congress and the various presidents who must confront it. With this in mind he cautions reformers not to assume that one can institutionalize these interactions in a way which will always guarantee preferred outcomes. It is well to remember that the widely-denounced House seniority system was partly constructed to protect liberal committee chairmen from the vicissitudes of conservative speakers.

This book is very much a "Washington" book. The detail contained in the case histories is extensive, perhaps too extensive for many readers. Orfield has benefited enormously from the superb coverage of Congress provided by Washington's newspapers. He has also made judicious use of congressional documents, particularly committee hearings and publications. As a result, this book is far less dependent upon the *Congressional Quarterly* than most other academic studies of Congress.

Roll call analysis is not stressed in this book. This has both positive and negative ramifications. On the positive side, Orfield recognizes that key decisions regarding the content of legislation are often made long before the members are called upon to announce their votes. This recognition gives a much fuller portrait of legislative interaction than a recapitulation of voting patterns could provide.

Because of his downplaying of roll call analysis, Orfield does not test systematically a number of propositions which he lists under "conditions of domestic leadership" (pp. 260–62). As a result, this book does not contribute as fully to the development of legislative behavior theory as some academic observers of Congress might want. This is a negative aspect of the book, but it is a minor drawback.

Congressional Power is a fascinating book, filled with insightful observations about the ways in which Congress contributes positively to the nation's life. It is an informed antidote to much that passes for wisdom in the appraisal of Congress.

GARRISON NELSON

University of Vermont

The Politics of the Property Tax. By Diane B. Paul. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. 158. $14.00.)

As an economist, I found chapters 5 to 7 of this book, which was adapted from Paul's doctoral dissertation, extremely interesting. These chapters describe the political forces that prevent businessmen and blacks in Boston from attempting to overthrow an illegal system of property assessment that burdens them relatively heavily; they also describe the forces behind assessment reform in San Francisco that increased burdens primarily on those that had supported it, especially home owners.

Among the explanations in the case of Boston are the political clout of white home owners, a system of city government that allows the mayor and his cronies to retaliate against taxpayers who question assessments, the relative lack of black interest in the largely indirect and invisible benefits of equal assessment. Other factors are businessmen's fear of uncertainty, mayoral retaliation, tax-induced defaults on mortgages, and loss of white residents to suburbs, and the availability of abatements on an ad hoc basis in cases where legal challenges to admittedly illegal practices would bring down the whole system of *de facto* unequal assessments. The most important challenges to Boston's unfair assessment practices seem to have come from outside groups largely unaffected by the considerations just mentioned, professors and law students at Harvard University and residents of Sudbury, a Boston suburb. The first groups were interested in reform, per se, and not in monetary gain, either for themselves or their clients. Sudbury objected not to Boston's unequal assessments, but to receiving less than its share of state aid as a result of Boston's failure to assess at 100 percent, as required by law.

Reform in San Francisco was precipitated by a scandal in which newspapers revealed that certain businesses were given preferential assessments in exchange for kickbacks. That businesses were taxed more heavily as a group than residences, despite preferential treatment for some, was not publicized, perhaps in part because of the newspapers' economic interest in assessment reform. The complexity of the issue and the difficulty of comparing assessments across classes of property added to the likelihood that home owners would not believe the few who warned that residential property taxes would be increased after reform. (A useful appendix describes Paul's data and methodology in determining the differential effects of reform.)

The remainder of Paul's book, including its overall conception and execution, are less impressive than the chapters on assessment, including chapter 3, which discusses the structure of assessing in big cities. The discussion in chapter 2 of the recent controversy over the incidence of the property tax is usually excellent (except in one basic respect to be discussed below) and should prove quite valuable to political scientists. But none of the remainder of the book depends in any way upon this discussion. Similar comments apply to chapter

4, which provides a model of assessment behavior based upon the notion that those responsible for assessment act as though they wished to maximize revenues. This simple idea, though interesting, is not developed theoretically, is not tested empirically, and is not employed to any important degree in the latter discussion. Thus it might better have been a long footnote.

Though the incidence controversy cannot be summarized here, it is worthwhile to make one point that has eluded many tax economists, as well as Paul. Traditionally, property taxes on improvements to land were thought to be shifted to consumers. According to the "new view," such taxes are *on the average* (for the nation as a whole) borne by owners of capital, and only *local differentials* are shifted. But the average tax rate for the nation is of policy relevance only at the national level. In virtually any city *any change* in property tax leaves the national average unaltered and is reflected almost entirely in changes in local differentials that are shifted. Thus for the questions discussed in this book—and for most other local property taxation issues—the burden of the national average property tax is largely irrelevant.

This book contains technical faults that could—and should—have been avoided. A feeling of *déjà vu* is created by unnecessary repetition of a few basic themes. Beyond that, there are many typographical and grammatical errors.

In summary, economists can benefit from the discussions of the politics of assessment in Boston and San Francisco, and political scientists can probably benefit from both those and the discussion of the incidence of property taxes, though the latter is not necessary for the former and contains an important omission.

CHARLES E. MCLURE, JR.

Rice University

School Politics Chicago Style. By Paul E. Peterson. (Chicago: University of Chicago Press, 1976. Pp. xvi + 304. $15.00.)

The comparative utility of decision-making models, pioneered by Graham Allison, is considerably advanced by Paul Peterson's study of *School Politics Chicago Style*. This advance is provided both by expanding the models and by comparing different policies within an urban context. He thereby strengthens the utility of the rationality model which Allison had earlier weakened in considering a single event in foreign-military policy.

Besides the familiar organizational model, the bargaining model is here distinguished conceptually into "pluralist" and "ideological" types. The former is familiar from urban studies, while the latter conceives actors as "motivated by broader, more diffuse interests, such as those of racial or class faction or of a political regime, which are regarded as of such enduring significance that the participants become deeply, ideologically, committed to them" (p. xii). Such persons pursue intense conflict and permanent allies across a range of issues, but characteristically—unlike the pluralist bargainer—they prefer not to compromise.

The rationality model is rescued by Peterson from critics' impossible criteria for it, i.e., a hierarchy of goals and the simultaneous evaluation of them in any single act of decision making. Rather, he urges and well employs the notion of "instrumental rationality" which rejects these criteria for "a rational actor [who] selects from the alternatives available to him the one that is most suited for achieving whatever goals (rational or not) the actor has in mind." The concept denies the requirement of a consistent fit of one goal with overall values; rather, members act "so as to maximize one or another of these particular goals in a defined policy context" (p. 130).

The strength of the book depends upon the utility of such comparative analysis in explaining policy results. For this purpose the author focuses upon the efforts at desegregation, collective bargaining, and decentralization by the Chicago school board, mainly during the 1960s. The interplay among the models in his analysis of these turbulent events is rich, subtle, and complex enough to match the phenomena at hand. Like Allison, Peterson shows that each model provides us, in *Rashomon* style, with different perspectives of an elusive "reality." More, he concludes that the model that one employs depends in good part upon the policy result one deems normatively important. Thus the rationality model stresses the consensus of the board, while the organizational model identifies constraints on the board's results and the bargianing model emphasizes specific outcomes of board action. As Peterson notes, these differences also underlie the familiar pluralist-elite research on urban politics, as they are conceptual distinctions which have often masked normative differences.

In a concluding chapter important for all urbanists, Peterson expands on the work of Roland Warren, Norton Long, and myself about the degree to which city politics is constrained by external forces. Here, the rational model explains much of the agreed-upon goals of policy makers in all major cities, i.e., stabilizing race relations, keeping a fiscal balance, and maintaining social order. In this unified set of objectives, cities are mutually competitive but ultimately constrained by external influences they can but little control, even when Daley was alive.

This work is not without problems. The effort to distinguish ideological from pluralist bargaining is strained, and ultimately they are not so different. Pluralists can be just as passionate and moral as ideologues, they can expand conflict, and they can believe their goals to be comprehensive for the interests they serve. The use of the organizational model for desegregation lacks supporting evidence for the deductions drawn and instead imputes motivation (p. 163). The "role interest" concept is still unconvincing, with its aspects of "false class consciousness." Too, the book's organization is curious: the four major models are not set forth until Part II, although two are used heavily in Part I; the details of desegregation conflict are placed within the models *before* the conflict is fully described; and there is too much repetition of desegregation events. There are also incongruent data totals in Tables 8 and 14.

But this is otherwise solid scholarship, addressing and contributing to theoretical questions about decisional processes in a useful comparative fashion. It refocuses attention upon rationality models, thereby providing a real, not unrealistic, analytical framework and reminding us that persons with power, as a class, are not basically stupid.

FREDERICK M. WIRT

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Prophets on the Right: Profiles of Conservative Critics of American Globalism. By Ronald Radosh. (New York: Simon and Schuster, 1975. Pp. 351. $9.95.)

Ronald Radosh, by presenting brief sketches of Charles A. Beard, Oswald Garrison Villard, Robert A. Taft, John T. Flynn, and Lawrence Dennis, has followed the pioneering efforts of William Appleman Williams in an attempt to demonstrate that the "internationalist" consensus constructed by Roosevelt and largely maintained by Truman consistently failed to take seriously these responsible and often incisive critics of American "globalism." Libertarian critics who challenged the fundamental assumptions of "internationalism" and "containment" were gradually relegated to the extremities of the American political spectrum. It is the burden of Radosh's argument to demonstrate that these five anti-interventionists offered frequently trenchant analyses which, had they been seriously considered by Roosevelt, Truman, and Eisenhower, could have helped prevent many of the global excesses of post-World War II American diplomacy.

Charles A. Beard, a leading "revisionist" historian of an earlier generation, whose public stature fell as his opposition to Roosevelt grew, deserves, Radosh contends, to be re-examined. Although recently treated quite roughly by such historians as Richard Hofstadter and Warren I. Cohen, Radosh claims that "Beard's predictions [about executive secrecy and the erosion of Congressional authority] make eerie reading in the light of the Vietnam war" (p. 51). Instead of defending "the dangerous and shadowy shibboleth of the Open Door" as Roosevelt had done in the Pacific," Beard "preferred the approach and diplomacy of Herbert Hoover [who] ... judged that imperialist Japan would never be able to effectively control China ... [but] will be absorbed or expelled by the Chinese" (pp. 55–57). Yet, given these insights, Radosh asks, why did Beard fail to embrace a "program for structural rearrangement of the economy, so that an economy would be created that would not depend upon new foreign markets for domestic prosperity and well-being?" (pp. 64–65). Primarily, Radosh answers, because no program of decentralized socialism "was advanced in the America of the 1940s, [for] the American Left viewed itself as part of the New Deal" (p. 65).

While Radosh manifests almost unbounded admiration for Beard, his analyses of Villard and Taft are more evenhanded. For example, in evaluating the editor of the *Nation*'s pre-World War II stance on American support for England, Radosh argues that "Villard's position was in some respects contradictory" (p. 78). On the other hand, Villard's participation in the America First Committee, is vigorously defended by Radosh who claims that "to repeat the charges of reaction [against Villard] only obscures the nature of Villard's critique and provides an apologia for those who argue that policy must be left to the experts" (pp. 84–85).

Taft's opposition to Roosevelt's post-1937 "globalism" is warmly reviewed in a fascinating chapter which concludes that the Ohioan's "outspoken views reflected rare political courage and acumen" through his unpopular attacks on "concentrated executive power," on the "usurpation of an independent congressional role by the executive," and on the "whole notion of a post-war Pax Americana" (p. 145).

But Radosh is generally disappointed by Taft's later foreign policy oscillations. Thus while applauding his condemnation of the Nuremberg trials and his break with Vandenberg over NATO, Radosh is clearly puzzled and disappointed by several of Taft's positions: his naive and ambiguous reliance on international law, his vitrolic attacks on Acheson's Far Eastern "appeasement," his support of MacArthur in Korea, and his unwillingness or inability to formulate a coherent, alternative, Republican foreign policy. Nevertheless, on balance, Radosh claims that "from the perspective of the 1970s Taft's views seem sober, wise and realistic" (p. 195).

The book's most interesting chapters are devoted to the "McCarthyite," John T. Flynn, and to the "Fascist," Lawrence Dennis. Here Radosh's resuscitative methods are most sorely tested, for both of these men have been generally remembered with opprobrium.

Radosh sketches Flynn as a classical liberal who early came to oppose what he saw as the monopolistic, fascistic tendencies of the New Deal. Whereas Roosevelt proclaimed that America must arm itself to remain free, Flynn argued that the creation of an armaments industry would lead to the destruction of America's traditional constitutional liberties. But how could this laissez-faire noninterventionist of the late 1930s be transformed into the enthusiastic supporter of Joseph McCarthy's Red Crusade of the early 1950s? Upset by the alleged secret diplomacy at Yalta and Potsdam, Radosh suggests that Flynn was "somehow continuing" his "long battle with the power grab of the executive" (p. 249). Hence Flynn opposed American intervention in Korea and Indochina. Despite the fact that "Flynn was waging a fundamental assault on the premises of the cold war" (p. 251), his "hatred of the cold-war liberals had blinded him to McCarthy's demagoguery" (p. 269). Radosh concludes, somewhat unsatisfactorily, that although "Flynn's fight was marred by poor tactics and by his perverse support of Joe McCarthy ... it does not take away from the magnitude of his courage and the accuracy of his effort" (p. 273).

Lawrence Dennis is the final figure discussed in this series of profiles in libertarian courage. Radosh believes that Dennis ought not to be dismissed as a Fascist, but rather should be perceived as a politically isolated, largely misunderstood conservative who consistently refused to accept the major assumptions of cold-war liberalism. With Beard, Villard, Taft, and Flynn, Dennis asked fundamental questions about congressional authority; a bloated executive branch; secret diplomacy; a global American imperium; the absence of meaningful foreign policy debate; the erosion of civil and political liberties; and the emergent military-industrial complex.

Radosh has written an exceptionally timely and provocative book which helps us recall the remarkably narrow ideological base on which the Cold War consensus was erected. The suppression of dissent in the name of "calls for national unity and security," Radosh argues, was a mistake which America cannot afford to make again.

RICHARD A. MELANSON

Kenyon College

Don't Make No Waves ... Don't Back No Losers: An Insider's Analysis of the Daley Machine. By Milton Rakove. (Bloomington: Indiana University Press, 1975. Pp. xii + 296. $10.00, cloth; $3.95, paper.)

In recent years, the subject of Chicago's politics seems to have become something of a literary genre in itself. New York may be the country's artistic and intellectual center, Los Angeles its mecca of mass culture, and Peoria its most typical city, but it is Chicago which seems most representative of the nation's local politics. In fact, of course, the Daley machine and its forerunners are not representative of most American cities' politics but stand in dubious contrast to the norm. The representative nature of Chicago's politics is more symbolic than real in that it symbolizes and therefore reinforces the negative view of politics that most Americans seem to share. Hence, if there is a genre of Chicago politics it is as much literary as it is scientific. Moreover, it is probably this slightly mythic quality which explains the number of works produced on the topic. Accordingly, there are two markets for such works, a scholarly audience, and a popular audience which reads for the enjoyment of the "flesh-and-blood" nature of the subject. Needless to say, the two are not mutually distinct.

Milton Rakove's work, though scholarly in its execution, will appeal more to the general audience of intelligent readers than it will to scholars, or at least to political scientists familiar with local politics and with Chicago's brand in particular. To the specialist, there is little that is new in this work. Rather it says everything you've always known about the Daley machine: that Daley's control was virtually unassailable, that the ward structure is the bulwark of such control, that the Republican and reformist oppositions are hopelessly ineffectual and that there is a mounting challenge to the party from the suburbs of Cook County and from the increasing black population which now comprises about one-third of the city's total.

Compared to other works on Chicago, this book has neither the polemics of Mike Royko's *Boss* nor the trenchant analyses of the several works by Banfield or Wilson. Nor does it make claims in either of these directions. Rakove's intentions are simply to describe the workings of Chicago's Democratic machine. The book, therefore, is basically modest both in its scope and in theoretical import; for this reason I think that some political scientists will find it somewhat uneventful and journalistic.

However, despite the highly descriptive nature of the work, there is something to be said for the easy and authoritative way in which Mr. Rakove handles the numerous details of the machine's operations, including its *quid pro quo* alliances with the Republican party and its relationships with Springfield and Washington. The author is clearly at home here, as indeed he should be: in addition to being a practicing political scientist at the University of Illinois' Chicago Circle Campus, Rakove has been deeply active in the Daley organization. Moreover, as is often the case with good case studies, the book is not without sharp insights, e.g., that given the highly pluralistic nature of Chicago's politics (as per Banfield's *Political Influence*) in which political decisions often as not offend some group, the machine and its politicians thrive on the art of non-decision making, i.e., by avoiding decisions whenever possible, especially controversial ones (p. 66).

In regard to the currently absorbing question of the machine's future in the post-Daley era, Rakove's hunch is that it will not wither away for want of leadership but that it will be transformed as a result of demographic factors. That in the first place the machine will lose its hold on the Cook County suburbs, as it has already begun to do, and therefore will be weaker than it has been in the past, and, secondly, as far as Chicago proper is concerned, the machine will be affected by the increase in the city's number of blacks and will most likely be transformed into the nation's largest and most efficient black political organization.

In short, this book has a modest purpose, yet says a great deal, by virtue of its author's intimate knowledge of the subject matter and his care in handling it. I strongly recommend it for general reading and as an adjunct text in both undergraduate and graduate courses in city politics.

JAMES D. FRATTO

Hobart and William Smith Colleges

The Supreme Court and the Commander in Chief. By Clinton Rossiter. Expanded Edition. (Ithaca, N.Y.: Cornell University Press, 1976. Pp. xxiv + 231. $15.00, cloth; $3.95, paper.)

The first edition of this book appeared in 1951. Rossiter's purpose, he said, was to examine the relations of the Supreme Court and the president as commander in chief, in order to determine "What future trust can we place in the Court as defender of constitutionalism in time of great national crisis?" (p. 2). After a fairly casual review of decisions, he concludes that the Court administers two constitutions. The wartime constitution gives the president "a virtually uncontrollable discretion" (p. 1); the peacetime constitution contains judicial reproofs of the president, made after the war has ended, for his wartime conduct.

Cicero said: "Silent enim leges inter arma." What is perplexing about Rossiter's book is his jubilation over illegal actions, which he calls "presidential prerogative" under the wartime constitution, and his scorn for the "rhetoric" and the "judicial self-hypnosis" of *Ex parte Milligan*. It is perhaps inconsistent that he is indignant at the decisions in the *Hirabayashi* and *Korematsu* cases, which upheld the government's restraint and incarceration of the Japanese-Americans during World War II.

His treatment of the cases is deficient. Neither *Martin* v. *Mott* nor *Luther* v. *Borden* involved "martial rule," the rubric under which he places them. He represents President Lincoln as defying Taney's opinion in *Ex parte Merryman* holding Lincoln's suspension of the writ of habeas corpus invalid, although Merryman had been surrendered to the civil authorities three days before the case was decided. *Ex parte Vallandigham*, in which the Court refused to entertain an appeal by a civilian from a military commission, was not an evasion; it was dictated by the law as it stood. *Ex parte Milligan*, which held such trials unconstitutional, did not reach the Court until 1866, and is derided by Rossiter for that reason; but surely he is wrong in saying that the case had no significance. Not only did Milligan survive; he also won a damage suit against the general who arrested him and the officers who tried him, and the opinion was in effect followed in *Duncan* v. *Kahanamoku* in 1946.

Rossiter ignores a large number of lower court cases which deny that there is a wartime constitution. Three federal courts and a state court held Lincoln's suspension of the writ of habeas corpus unconstitutional, and the Department of Justice did not appeal to the Supreme Court because it feared affirmation. On several occasions courts have fined or awarded damages against military officers for arresting civilians. Rossiter even ignores important Supreme Court decisions, such as the case of the Floyd Acceptances.

The judicial power of the United States includes admiralty and maritime jurisdiction. Lacking direction by Congress, the courts apply the law of the sea; but maritime law can be changed by Congress. Similarly, the president is commander in chief, and in the absence of statute possesses the powers which that title carries under the law of war; but the law of war can be changed by statute and by treaty. Under the power to make rules for the government of the armed forces, Congress has altered the powers of the commander in chief extensively; in the Command of the Army Act of 1867, which Rossiter does not mention, it even took away his power to give orders to the armed forces.

Richard P. Longaker has written an introductory note and a concluding chapter which reviews the cases since 1950. The analysis of cases is much superior to Rossiter's and one infers that Longaker does not share Rossiter's peculiar value system; but one chapter cannot save a book.

FRANCIS D. WORMUTH

University of Utah

Unchosen Presidents: The Vice President and Other Frustrations of Presidential Succession. By Allan P. Sindler. (Berkeley: University of California Press, 1976. Pp. 118. $5.95, cloth; $2.95, paper.)

In this short book Allan Sindler completes an analytic framework for presidential succession. He confronts the unforeseen outcome of the Twenty-fifth Amendment—an unelected President—and finds this development workable within our political tradition. The primary object of his analysis is the existing succession arrangements centering on a vice-president, but it is not confined to them. Sindler recognizes the present difficulty in recruiting a person of the first order to the dependent position of the vice-president; however, his analysis is not devastating to the current process. Additionally, the book covers adequately for its purpose the historical development of the vice-presidency and the succession laws.

Sindler's analysis is aimed at the practical problem: "How can we assure that vice-presidents will be selected with primary preference to their successorship role?" The most fragile parts of his framework are the measurements "status and repute" of a successor and the "intent behind the succession" (p. 9). His definition of status is a "positive and national standing and recognition" (p. 9), and "intent" is defined as whether successors "are chosen primarily for their presidential successorship role" (p. 9). The framework's basic normative criterion is that a successorship device must "provide a rapid and stable process which will produce a successor considered legitimate and acceptable by the public" (p. 10). His other imperatives for a succession system are: same party succession, same policy outlook as predecessor, and "acceptability" of the potential successor to the incumbent president.

The same criteria are applied to major alternatives to the current succession system. He sees as failing his criteria of "key constraints" certain other suggested means of providing for presidential succession. Undesirable proposed reforms include: increasing the authority of the vice-president by law or constitutional change; pre-convention nominating changes such as pre-convention designation of a candidate's vice-presidential choice; or convention nominating changes, for example, "automatic award" of the party nomination of vice-president to the runner-up. Sindler finds as potentially passing his criteria of "key constraints" such proposed reforms extending the time for the vice-presidential nomination or having the presidential candidate share the choice of the vice-president. These options include the possibility of the convention's choosing from among a set of names provided by the presidential nominee; a party committee's recommendation—even a post-convention determination; or the most daring alternative—an appointive vice-president (appointed by the new president and approved by Congress as the normal pattern).

Sindler's work provides a rational framework for the future discussion of this problem. It is a more useful framework for students of the presidency than such Watergate-inspired reactions as that of Arthur M. Schlesinger, Jr., and Sindler's analysis held up well during the successor selection process of 1976. In spite of a slight factual error in regard to John Connally's service as Secretary of the Treasury (p. 100, n.), this work is a most valuable systematic framework clearly and succinctly set forth.

LANDIS JONES

University of Louisville

Economic Integration in New Communities: An Evaluation of Factors Affecting Policies and Implementation. By Helene V. Smookler. (Cambridge, Mass.: Ballinger, 1976. Pp. xxxiii + 236. $15.50.)

From the inception of the twentieth-century new town movement, when the famous British planner Ebenezer Howard advocated the development of *de novo,* totally planned and controlled small cities, through the implementation of the British New Towns Act of 1946, to the privately financed Reston, Virginia and Columbia, Maryland, and the Urban Growth and New Community Development Act of 1970 (Title VII), one of the major attractions of new communities has been the promise that their *tabula rasa* nature would afford the opportunity to replace social, economic and racial housing segregation with "balanced communities."

This book examines the record of economic and racial housing integration in 15 new communities (13 totally privately financed and 2 federally guaranteed) vis-á-vis 15 paired conventional communities in the United States. The data cited in this book are drawn primarily from personal interviews with 161 key decision makers involved in the development of those cities. The 15 new communities were classified as (a) those that had planned for economic integration and had been successful (n=3); (b) those that had not planned for mix, but were integrated (n=3); (c) those that had attempted, but had not achieved mix (n=2); or (d) those that had no integration goals and were homogeneous (n=7). This classification establishes the premise, and I believe correctly, that the traditional suburban economic and racial segregation patterns are largely being replicated in planned new communities.

My methodological reservations about this book are threefold and are primarily directed at the NSF/RANN study from which it is excerpted. First, and most importantly, many of the 15 new communities examined were in early stages of development. Certainly, one must be cautious before making any assessment of integration patterns when 13 of the 15 developments had yet to achieve 50 percent of their target populations and several projects were less than 5 years old. Secondly, there are a number of important variations within the planned community category. For instance, the ages of the developments varied from 2 to 26 years, and the populations ranged from 1,500 to 34,000. Moreover, many developers use "planned" as a code word precisely to signal economic, and indirectly racial, exclusion. Therefore, to attribute policy intent on the basis of self-designation is hazardous typology. Thirdly, the paired communities were selected on the basis of four criteria: same urban area, age, price range, and type of housing. In complex housing markets, such as Chicago, these factors could select a number of matched pairs with considerable in-group variation. While these methodological concerns raise doubts concerning the within and between group comparisons, the bulk of the data would still seem to argue that, while planned communities may have achieved worthwhile land-use and amenity goals, they have not achieved economic and racial balance.

My conceptual concerns derive from Smookler's statement that "the findings of this study indicate that in no case has the existence of subsidized housing adversely affected the housing or property values in any of the communities" (p. 115). This categorical assertion seems more normative than empirical in view of her data which indicate that the percentage of blacks ranges from 0–19, with a median of 2 percent, and that the percentage of subsidized housing units ranges from 0–36, with eight communities having none and only two having more than 10 percent. Smookler, herself, dismisses the proposition that new communities are initiated by a "new breed" of socially enlightened developers and concludes that, because of the magnitude of their financial investment, they are extremely sensitive to marketing prejudices. Moreover, the first residents of new communities, frequently through local governments, themselves pressure the developer to avoid threats to their property values. Further, Smookler observes, again I believe correctly, that even the federal Title VII loan guarantee program has not been able to lure developers away from the safety of the exclusive upper-middle-class, white, suburban market. Dr. Smookler's conclusion that "social objectives can be pursued without damaging the housing market [of new communities]" (p. 125) unfortunately does not negate the reality that those investment risks are not generally being taken and housing segregation is not being reversed in planned communities.

This book provides a good starting point, but a great deal more research is needed on all aspects of new community policy, including economic integration. Hopefully Smookler's excellent questionnaire will be used again. It is particularly important, however, that future investigations include all 17 of the federally sponsored new towns. If new communities are going to serve as "laboratories of democracy," as both Smookler and I hope, then economic integration must succeed in the Title VII developments.

ERIC L. STOWE

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Senate vs. Governor, Alabama 1971: Referents for Opposition in a One-Party Legislature. By Harold W. Stanley. (University, Ala.: University of Alabama Press, 1975. Pp. xii + 82. $6.00.)

This study explores "the salient referents for legislative behavior (mainly voting) in a one-party legislature, using data from the Alabama State Senate of 1971" (p. viii). The five referents, or sources of clues, which are analyzed are the legislator's constituency, the influence of the governor, the importance of factions, the role of committees, and the impact of lobbyists. These influences were studied independently of each other, and the results are reported in separate chapters. Each of these examinations tends to be thorough, and the conclusions reached are supported by facts. However, as Stanley notes, in the real world of the legislature these influences do not operate independently upon legislators but are usually referents that influence them simultaneously. Consequently, his study tends to resemble a set of still photos rather than a moving picture of the legislative process. Nevertheless, the book gives a very good overall view of the Alabama Senate. The author concludes his study by stating that "the single most explanatory fact about power in the Alabama State Senate concerns the role assigned to a governor by law and tradition. The Alabama legislative system offers a solid example of a strong executive" (p. 79). This general conclusion is eminently true of the legislative process in Alabama, both in the House and the Senate.

The realistic tone of this study is primarily the result of interviews with 27 out of 35 members of the Alabama Senate. These 27 interviews, as well as other interviews with lobbyists, journalists, and former legislators, add depth and strength to the study. However, the decision of the author to protect the confidentiality of his sources leaves the reader at something of a loss in making any independent check of his conclusions. This problem is further compounded by the fact that Stanley failed to ask all of his interviewees the same questions. Consequently, he is not able to use statistical analysis to support his conclusions. The chapter which deals with factions is an exception; here Stanley did use a questionnaire for the majority of his interviews and presented his results statistically.

While Stanley's study is most interesting, his choice of the Alabama Senate in 1971 appears to limit its usefulness. The Alabama Senate was not operating in a typical fashion in 1971 because of the presence of a definite anti-Wallace faction. Stanley apparently found studying this faction to be the most interesting of the various referents he examined. However, since this Senate was admittedly atypical, the question arises as to whether or not his research might have been more productive if he had studied a more typical situation.

One possibility would have been to study the House in 1971. As Stanley points out, his original intention was to replicate Samuel Patterson's study of the Oklahoma House in 1959. He did not carry out this intention, however; and he explains his decision to study the Senate by saying that "the choice of the Alabama State Senate was far from arbitrary but less than scientific." If Stanley had stuck to his original goal of replicating the Oklahoma study, particularly with the improvements in methodology he suggests (p. viii), perhaps it would have given him a comparative base for somewhat broader conclusions on referents in a one-party legislature. It is to Stanley's credit that his conclusions derive only from his data, but it seems unfortunate that he did not choose to make a study that might have provided a basis for much broader generalizations.

COLEMAN B. RANSONE, JR.

The University of Alabama

Community Action Groups and City Government: Perspectives from Ten American Cities. By Frank X. Steggert. (Cambridge, Mass.: Ballinger, J.B. Lippincott, 1975. Pp. 84 + appendices. $13.00.)

Given the growing importance of organized citizen participation in urban politics, any addition to our knowledge of the range and focus of group activity, conditions which activate groups, and factors that influence effectiveness is indeed welcome. Steggert reports on common findings from the studies of community action groups undertaken as part of the Urban Observatory Program. One of the most promising research developments for urban scholars, this program was designed to generate cooperative multi-city research for use by city officials and scholars. The program's first joint effort was a survey of citizen attitudes in ten cities (Floyd Fowler, *Citizen Attitudes Toward Local Government, Services and Taxes*, Ballinger, 1974); the second consisted of studies in each city of organized citizen participation. Though Steggert uses data from both projects, his primary purpose is to summarize findings of the individual city participation studies.

After he briefly explains the project, describes the cities and gives an initial summary of findings, Steggert uses survey data to locate differences in membership rates across the cities and to look for correlations between differential membership rates and selected individual and contextual characteristics. The remainder of the book draws upon the participation studies—chapter 3 giving examples of the different schemes used to classify existing groups, chapter 4 focusing on issues around which groups organize, and chapter 5 showing how some of the study groups approached evaluation of group effectiveness. Steggert closes the narrative part of the book by summarizing his findings, developing a model of the political system which includes organized citizen participation, and suggesting ways in which local political systems might better adjust to accommodate citizen participation. The appendices contain supplementary data from the attitude survey, excerpts from city reports and some bibliographic suggestions.

Developing and testing theories about the urban political process requires the kind of comparative data normally unavailable to urban scholars. Thus the existence of both survey and case study material from ten cities provides an extraordinary opportunity to study the conditions under which variations will exist in the number, types, strength, behavior and effectiveness of groups. Unfortunately, Steggert does not take full advantage of this opportunity—instead of making direct city comparisons, he restricts himself to presenting only common findings, which are then illustrated by selections from the city studies.

Given this narrow focus and the consequently limited nature of the findings, the book will likely prove more useful to local officials than to scholars. The tone of the final chapter suggests that the author is, in fact, writing primarily for that audience, hoping to convince city officials of the need to adjust political processes to better accommodate the kinds of organized citizen participation that the studies indicate are likely to arise when social change threatens a neighborhood. It might also be useful reading for leaders of community organizations interested in the kinds of groups and group behaviors which city officials have judged most successful.

For scholars, however, the book is likely to prove rather frustrating, for it will be virtually impossible not to wish the author had chosen a different approach to the data. There is no place in the book where data from all ten cities are synthesized and systematically presented. Therefore, readers can neither themselves determine such things as which cities have the largest number of groups, which have the highest ratio of active to potentially active groups, which are the most receptive city officials or where groups are most effective; nor can they verify the author's conclusions about communality. Steggert also fails to integrate adequately the findings of the citizen survey with those of the city studies. The survey permits identifying those cities with particularly high or low membership rates; structuring the findings from the participation studies around these differences might have told us what differentiates group activity in low membership cities from those with average or high rates, or what characteristics of city government are associated with variation in membership. There has also been virtually no attempt to connect with the scholarly literature on group politics. Finally, one finds a number of methodological problems, particularly in the chapter based upon the citizen survey. Although the author of the book presenting initial findings from this survey (Fowler, cited above) specifically says the sampling techniques used make it inappropriate to mesh the ten separate samples (p. 6), Steggert combines them through most of this chapter. Although available, statistical techniques are avoided which would permit assessment of the comparative importance of some of the variables he finds associated with membership (race, class, age, residence). In addition, conclusions are sometimes drawn which require column percentages when tables show only row percentages (pp. 14, 15).

While the book's narrow focus and methodological problems limit its usefulness to academicians, one comes away with the sense that community action groups will be well advised to learn more about political systems in order to participate more effectively, and officials may have to adjust political systems to admit new players.

LYNN FOSTER

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The Children's Cause. By Gilbert Y. Steiner with Pauline H. Milius. (Washington, D.C.: The Brookings Institution, 1976. Pp. viii + 265. $9.95, cloth; $3.95, paper.)

Gilbert Steiner and Pauline Milius explain in the first chapter of *The Children's Cause* that there are two groups concerned with the status of children in American society. The first group is found in Congress and the bureaucracy, the second in foundations and in voluntary associations: "The former are uneasy about the latter's faith that expressions of love and imprecise concepts like 'developmental care,' 'maximum potential,' and 'services for all who need them' can become manageable public policy" (p. 4). The authors maintain that their book is intended to answer the questions of how and why this dichotomy has evolved between the advocates of what should be done for children. A brief history of federal policies affecting children in the twentieth century is also provided in the first chapter where it is explained that "ambivalence is the best description of federal-level responses that involve intervention" (p. 5).

The second chapter discusses the philosophy behind day care programs such as Head Start and the mixed results of the Head Start program. The Westinghouse Learning Corporation and Ohio University conducted an evaluation of the program, according to the authors, which showed relatively few cognitive gains for children who attended Head Start programs when compared with children who did not attend Head Start. Steiner and Milius maintain that what Head Start did contribute, however, was a demonstration of the public's willingness to accept state intervention in the development of children as young as three years.

The third chapter deals with the infighting among federal agencies dealing with child care during President Nixon's dismantling of the Office of Economic Opportunity. The authors discuss the creation of the Office of Child Development in HEW with the responsibility of administering Head Start, Community Coordinated Child Care, and Parent and Child Centers.

Chapter 4, "The Bureaucratization of Child Development," is a critical examination of the Office of Child Development. The first director of this new agency was a Yale psychologist, Edward Zigler. The authors contend that Zigler was "unlucky" in his inheritance of Head Start, Parent and Child Centers and Community Coordinated Child Care programs. They further contend that Zigler was innovative but the new programs that were initiated were "fuzzy in conception and sloppy in execution" (p. 61). The chapter concludes with an excellent discussion of child abuse.

"The Politics of Comprehensive Legislation" discusses the passage by Congress of a major piece of child development legislation vetoed by the president in 1971. Steiner and Milius are pessimistic about the future of such programs given the realities of the economy of the mid-seventies.

The authors maintain that during the past ten years those interested in child development were offered three opportunities to construct child development policies: the Joint Commission on Mental Health of Children, the 1970 White House Conference on Children and the National Research Council Advisory Committee on Child Development. All three failed and the authors contend "that children's policy is not successfully nurtured in official conferences, commissions, or advisory committees" (p. 118).

Chapter 7 explores the past and present lobbying activities for the benefit of children that have been pursued by various groups: the Child Welfare League of America, the National Organization of Women, the National Council of Organizations for Children and Youth, the Children's Lobby and the Children's Defense Fund of the Washington Research Project, which the authors believe to be the strongest of these groups.

The national school lunch program is viewed by the authors as "the success story of the children's cause" (p. 176). The eighth chapter discusses the history of the school lunch program as well as whether it currently needs reform.

One of the best chapters covers health programs for children. The authors find that there is no single national health policy for children. Rather, there are four "streams" of national activity which the authors identify as "a child health research institute within the National Institutes of Health; grants to the states for maternal and child health services; screening and treatment of welfare-eligible children; benefits to handicapped children" (p. 206). The authors discuss these programs and call for a unified federal policy aimed at children's health to replace presently uncoordinated programs. The authors conclude that "the children's policy most feasible—and most desirable—is one targeted on poor children, handicapped children, and children without permanent homes" (p. 255).

Steiner and Milius have done an excellent job of bringing together a wide variety of information bearing on the well-being of children. The book is indispensable for those interested in public policies affecting children and highly recommended for those interested in the broader field of public policy itself.

JOHN S. ROBEY

New Mexico State University

Explorations in Convention Decision Making: The Democratic Party in the 1970s. By Denis G. Sullivan, Jeffrey L. Pressman, and F. Christopher Arterton. (San Francisco: Freeman, 1976. Pp. xi + 147. $6.95, cloth; $3.50, paper.)

The political science community has begun to produce its own continuing series of books on national party conventions, courtesy of Denis G. Sullivan, Jeffrey L. Pressman, et al. The et al. for the 1972 Democratic National Convention were Benjamin I. Page and John J. Lyons, and the book was *The Politics of Representation*. The single added author for the 1974 Democratic Mid-Term Convention is F. Christopher Arterton, and the book is *Explorations in Convention Decision Making*. The research site for this second book in the series was the Democratic Party's first, and perhaps only, interim national convention, a device which was conceived as a quadrennial means for transmitting rank-and-file desires to national party leadership, in the period between presidential nominating conventions, but which was ultimately reduced to adopting a party charter, at its only meeting, in December of 1974.

Mid-term conventions, at least as embodied by 1974's version, are a new and peculiar forum for political activity in the United States. They are, in effect, a highly specialized arena in which self-starting, relatively intense political elite groups can fight over future party rules. These are arguably important decisions, indirectly affecting the fortunes of large numbers of people, but there is little in the preparation for such battles which can attract the ordinary citizen, and, indeed, citizen participation in the selection of delegates to the 1974 mid-term convention was remarkably, abysmally low. Most mid-term convention delegates were selected through participatory convention systems, and even in a presidential year, turnout at the local level in such systems is low. Thus 1976's participatory conventions featured a high of 5 percent of all eligible Democrats and a low of .9 percent, with more states leaning toward the lower end; no one in national party politics believes that turnout in 1974 reached even a quarter of that. By the same token, many state party leaders saw no reason to become embroiled in conflict over such arcane matters. Those who were the beneficiaries of ex officio seats (and about 17 percent of all delegates were beneficiaries) attended; others declined to get involved. Otherwise, as a result, the modal delegates were either ideologically motivated independent activists, with an interest in expanding their party influence through manipulating the rules, or organized labor sponsored delegates, with an interest in shoring up labor's political influence, by manipulating party rules.

Within that very peculiar collection of actors, Sullivan, Pressman, and Arterton conducted their research. As they did in the earlier *The Politics of Representation*, they assigned themselves two tasks in *Explorations in Convention Decision Making*. First, they sought to use the convention context to investigate some more general social phenomena; in this case, the central phenomenon was decision making, and the subfoci were the structure of organized blocs, the tactics which such blocs use to influence decisions, the particular bargains which result, and the way participants ultimately perceive those bargains. Beyond that, the authors also sought to describe a particular political event of some significance, in this case the mid-term convention, and to place it in a developmental framework.

The two tasks require slightly different standards for evaluation, but neither presents any serious problems. The more generalized social science analysis is carried out carefully, with most measures fully described. This is the heart of the book, and is found in its three central chapters, "Reform Issues and Party Cleavages," "The Institutionalization of Group Caucuses," and "The Problem of Legitimation." The stickiest methodologists may be made slightly uncomfortable by their sample, for although they did succeed in interviewing 145 out of their selected 165 convention delegates, that number is actually only 7 percent of the total; when it is subdivided into five or six categories for further internal correlation, the Ns in those categories must become quite small. Unfortunately, in the first half of the book, most tables are presented without their Ns, so that the reader cannot be automatically reassured on these grounds (which is a failing shared with *The Politics of Representation*). Occasionally, this omission has substantive implications. For example, their discussion of the perceptions of women who did and did not attend the Women's Caucus leads the reader to believe that only deviant women did not participate, whereas later figures show that fully 70 percent did not attend. Most of the time, however, this is not the case; the authors are scrupulous in their explanation of other methodologies, so that there is little reason to question their presentation even when Ns are missing.

Surrounding this section of the book are two introductory and two concluding chapters, which seek to put the theoretical work back into a more concrete, present-day, explicitly political framework. Here, the authors address the current impact on the Democratic Party of its structural reforms, as well as their probable future impact. The authors clearly share a set of general premises with those who have been in the vanguard of party reform movements, including, for example, the belief that the 1970s are a time of great social change, that party rules which advantage the national-level elite are preferable to those which advantage state and local elites, that party rules battles ipso facto feature ideological liberals versus conservatives, etc. To their credit, however, they also offer both tables and descriptions which would argue in the other direction, e.g., their revelation of the impressively secretive and closed nature of the Women's and Black Caucuses, much more closed than the average state delegation. Thus the authors cannot be accused of trying to steal a final verdict in line with their prejudices.

In fact, the single finding which unites their more general social science analysis with their specific event analysis, and which turns up over and over again, is likely to be as discouraging to them as it would be to nearly all Democratic party identifiers: Namely, that there is a persisting, unabated division within the party between purists and professionals, or between reformers and regulars, if you prefer. Pessimistically, Sullivan, Pressman, and Arterton suggest that continued participation, and even success, is not moderating the orientation of reformers, while that same continued participation and success is making the regulars more intransigent. At only one point, in their factor analysis of orientations toward various objects and leaders within the party, do they hold out any hope that this division could be moderated, and that hope is really a methodological artifact of the presence of orientations toward George Wallace. With Wallace removed from the picture, even this factor analysis collapses back into a single dimension, with regulars at one end and reformers at the other. The authors suggest, finally, that one way out of this box would be the multiplication of candidates and issues, with subsequent bargained solutions; the recent Carter nomination campaign suggests that a second way out might be a de-emphasis of issue content in general; but the main thrust of this book, for practical politics, is that there is an underlying problem within the Democratic party which is not getting better, which will not go away, and which holds the potential for more conventions like 1968 or 1972, rather than 1976.

BYRON E. SHAFER

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Energy Planning and Policy: The Political Economy of Project Independence. By Thomas H. Tietenberg with Pierre Toureille. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. vii + 168. $15.00.)

The revival of interest among political scientists in "public policy" during the last five or six years has unfortunately not been matched by the renewal of professional competence. To put the matter simply, but correctly, "economic efficiency" ought to be a primary consideration in designing public policies, yet political scientists generally know almost nothing about this elusive concept. Although it is not too hard to master the essential concepts of microeconomics, public finance, and applied welfare economics (i.e., benefit-cost analysis), political scientists shy away from the effort. They also tend to excuse their ignorance (or laziness) on the grounds that public decisions are much less affected by efficiency considerations than they are by "politics." "Politics" is then implicitly endorsed as superior to economists' and systems analysts' mindless efficiency-mongering. Thus does fact become value, and thus do political scientists take up normative positions without quite meaning to. "Politics" often extracts a very high price from the citizenry, however, and political scientists ought certainly to reckon its magnitude before advising others to pay it.

Thomas Tietenberg is an economist who knows a fair amount about politics, though not quite enough. His subject is what the Federal Energy Administration, the president, the rest of the executive branch, and Congress did, and did not do, during 1973–1975 regarding what he loosely calls "energy" issues. Tietenberg was an analyst with the FEA for part of the period he writes about. Hence his account, which is informative and full of interesting detail, has both the virtues and the shortcomings of a (very low-echelon) insider's view. He is especially interesting on President Ford's decision in early 1975 to impose a tariff on imported oil. Tietenberg argues that the announced goal of reducing imports by one million barrels per day would not have brought down the OPEC price or protected us against a new Arab embargo. He has good insights about why the gesture was satisfying politically. But he is missing the crucial explanation as to why the gesture escaped criticism: the only agency in town with the analytic capability to defend or attack it decisively was the FEA, and the FEA was afraid of making trouble because it was at that time struggling for survival.

Tietenberg takes great pains to develop a very good case on economic and foreign policy grounds, for a crude oil stockpile. Yet in his discussion of the "responsiveness of the policy process" he does not raise the important question of why such a proposal has so few backers in Washington. A political scientist might have pointed to the absence of a weighty bureaucratic constituency for such a program. Since a stockpile is in effect a weapons system, should not the Defense Department be interested? Of course not, because a stockpile lacks glamor and typically draws criticism for being "too big" and/or "too expensive." Only the FEA has a bureaucratic interest in a stockpile, but Tietenberg's identification with the agency prevents him from seeing that FEA support is worth almost nothing (and may even have a minus value) in Washington politics.

But like most economists, Tietenberg favors an orderly withdrawal of government regulation from domestic energy markets. He also understands the main political obstacles. Tietenberg understands that if deregulation is to come about, it must be slow and must entail, along with some covert manipulation by administrators, a whole series of bribes to beneficiaries of the regulatory regime to induce them to let it lapse. I suspect a political scientist writing on this subject would have given us a richer picture of the "group struggle," depicting the views of oil companies, environmentalists, consumer groups, Russell Long, and so forth. But such a picture would have missed an important "non-group" as well, namely, those consumers who are indirectly, and often unwittingly, harmed by governmentally maintained low energy prices, especially natural gas prices. These are primarily (1) persons living in energy-poor regions who will experience "shortages" when natural gas prices do not permit markets to clear efficiently, (2) persons and firms that substitute electricity, coal and oil for gas in uses for which gas would be preferred but for its reduced availability, and (3) future generations who will "run out" of gas prematurely because its present low price permits us to squander it. In short, a political scientist's picture would have scored higher on contrast but lower on composition. Until political scientists learn a little economics, they will not be able to get the basic picture straight.

EUGENE BARDACH

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Regulation and Drug Development. By William M. Wardell and Louis Lasagna. (Washington: American Enterprise Institute for Public Policy Research, 1975. Pp. 181. $3.50, paper.)

It is unusual to find a book written by professors of pharmacology and toxicology reviewed in the *American Political Science Review*. Yet Wardell and Lasagna attempt something that is very "in" in political science: the evaluation of public policy.

The policy under evaluation is in itself evaluative in nature. Studied is the way U.S. drug laws and the Food and Drug Administration handle the review and testing of new prescription drugs being introduced to the market. Part 1 of the volume, in addition to introducing the topic, discusses quite elaborately the various research designs and evaluative criteria that can be used to test drugs. Part 2 consists essentially of three journal articles published previously by Wardell which compare new-drug regulation in Britain with that in the U.S. Part 3 develops the author's conclusions, which are that the American approach—in comparison to the British—is overly conservative in the approval and release of new drugs for therapeutic purposes.

The authors argue that in American drug regulation the assumption is "that to minimize toxicity is to maximize benefit" (p. 162). Emphasis is placed almost exclusively on avoiding the risk of harmful side-effects and the introduction of less efficacious drugs, rather than on the development and improvement of drugs capable of enhancing health care and saving lives. The authors criticize this American reliance on what the authors call "pre-marketing restrictions" in favor of the British practice of allowing faster and easier introduction of new drugs, followed up with a system of post-marketing monitoring for safety. The monitoring system they advocate for the U.S. would entail administration of newer drugs in hospitals and would rely upon doctors who are specialists in the therapeutic area concerned.

The authors' critique of U.S. drug regulation as stifling and heavy-handed should, as this is being written, find many sympathetic ears in official Washington. Yet their contentions are totally opposite in thrust to attacks on the FDA in recent years by consumer advocates. Writers like Richard Harris, James Turner, and Morton Mintz argue that the FDA has irresponsibly given in to the pressures of the big pharmaceutical houses to introduce drugs before they are established as safe and effective. The thalidomide disaster is always mentioned in this connection. Indeed, it would be difficult to convince the mother of one of the "seal flipper" babies that keeping the drug out of her hands would not have been preferable to a "monitoring" of her disaster.

This outright contradiction in the conclusions of what might be considered two lines of policy evaluation should not surprise us. Only if we impute true objectivity and rationality to policy evaluation should we expect from it consistent outcomes. Evaluation involves, of course, values. Harris retains the muckraker's view of a predatory capitalism which must be aggressively checked by regulators to protect the public. Wardell and Lasagna bear the scientist's faith in the potential of new knowledge and its application to human welfare, if the suppressive effect of regulation can be released.

Both viewpoints are important when confronting an issue as critical as new-drug availability, and at no time should either one be missing in public discussions of it. This book is poorly organized, highly repetitious, and politically naive at points, but it is a welcome addition to the literature on drug regulation policy as a provocative challenge to criticism by consumer advocates.

CHARLES T. GOODSELL

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College Professors and Their Impact on Students. By Robert C. Wilson, Jerry G. Graf, Evelyn R. Dienst, Lynn Wood and James L. Barry. (New York: John Wiley, 1975. Pp. xii + 220. $12.95.)

For well over a decade college students and college faculty have been the subjects of a considerable number of survey research studies. The annual surveys of the American Council on Education and the special surveys conducted under the auspices of the Carnegie Commission on Higher Education are the best known. *College Professors and Their Impact on Students* reports on studies conducted by the Center for Research and Development in Higher Education at the University of California, Berkeley. These reports on modes and impact of teaching are of interest to faculty and administrators regardless of field. The data should provoke political scientists interested in undertaking inquiry on organizational behavior and using universities and colleges as subjects. It would be instructive for political scientists to review the survey data on higher education.

Wilson, et al. accurately characterize this presentation of survey data on faculty and students: "... as relatively separate and isolated panels of a mosaic depicting some of the variety of ways in which students and faculty of a variety of institutions pursue learning and teaching." They describe the attributes of effective teachers and of students who are effective learners as follows:

1. There is no difference between the faculty who are effective teachers and other faculty with respect to their choice of instructional methods or their organization of instructional materials.

2. Faculty who are successful teachers are more accessible to students to discuss intellectual issues; they relate course materials to current social and cultural issues; they strive for a dynamic style of presentation; and they value undergraduate teaching more than other academic activities.

3. Students who achieve intellectual growth are more aggressive in contacting faculty (as well as their peers) to discuss class material and other intellectual questions; they select independent study programs; they prefer to read biographies, autobiographies and discussions of current social issues.

In sum, effective teachers and their students share a personal preference for intellectual discussion and interaction. Wilson et al. propose changes in the instructional environment that should encourage other faculty and students to adopt comparable activities, i.e., to foster the interaction between students and faculty, the "relationship between students and teachers that is at the start of the educational experience." The changes include individualizing instruction, and increasing instruction in values and value choices.

Comparison among the findings and recommendations in *College Professors and Their Impact on Students* reveals a classic conflict in social research: a prescription for structural change in a situation dominated by individual choice. A useful study would be an experimental design to determine whether deliberately altering instructional methods can result in greater contacts and greater discussion of intellectual issues by students and faculty who otherwise would not choose these activities.

SHEILAH KOEPPEN MANN

The American Political Science Association

Comparative Politics

Saints and Samurai: The Political Culture of the American and Japanese Elites. By Lewis Austin. (New Haven: Yale University Press, 1975. Pp. 197. $12.50.)

How can such a bad book be so good? It is, after all, a very weak study. Consider the sample: 42 Japanese business executives who were administered an MIT Managerial Attitudes Survey (in Japanese) in 1972, in comparison with 42 (of 300) Americans who were given the questionnaire at a Yale reunion in 1969 and mailed it back in.

Of what are we to assume the samples are representative? The Yale alumni and the MIT management participants? Business elites of the two countries? Elites of the two countries? Citizens of the two countries? Austin seems to consider them representative of one and all at one time or another, but I doubt that they are representative of anything. They may not even represent the attitudes of those who returned the forms, though we might assume they do. But that they can be taken as indicative of anything beyond that—at the time they were completed, much less now—is very dubious.

The author devotes five of the book's seven chapters to an extremely labored description of the responses of his 84 subjects, complete with bar graphs and chi-square rules-of-thumb. Even though he seems to think his sample warrants serious treatment, he overlooks many opportunities to compare the responses of his subjects with those of many other studies in both countries which have used identical or similar questionnaire items (e.g., the well-known Rosenberg "Faith in People" scale; the Rokeach Dogmatism Scale, and the TAT pictures). It also might have improved the value of the study if he had, for example, compared the attitudes of his Japanese respondents with those of the Government's Japanese National Character surveys; or with the literal flood of Japanese attitude studies reported monthly in *Chosa Geppo;* or the annual reports of government-funded surveys contained in *Chosa Nenkan.*

Conversely (or, concomitantly), Austin might have dug a little deeper and related the contemporary attitudes of his Japanese respondents to the more traditional concepts of *on, gimu,* and *giri;* or to those found in Prince Shotoku's "Constitution," for two examples of many comparative roads not taken.

While it is often considered to be poor form to criticize a book for what it does not do, this is such a thin volume (148 pages of text—short, narrow pages, and big type, at that—of which 99 pages are entirely devoted to the labored description of the responses, as previously indicated, coupled with 44 pages comprised almost entirely of the questionnaire reprinted in English and Japanese—a very helpful thing to do, by the way) that it is difficult to believe that the author could not have performed a more extensive comparative analysis—especially since the Japanese data were obtained in 1969, and the book was published in 1975!

Indeed, as it stands, this is not a thin book but a fat article that would have been better in a management journal, or perhaps as a monograph in a series on "comparative business elites." It does little credit to the usually fine Yale Studies in Political Science series in which it stands. In fact, without trying to define "political science" to narrowly, one looks in vain for any special relevance to that discipline beyond that which can—and is—found in any study of human behavior.

One last carp in this vein: did anyone at Yale University Press read the manuscript? Then why not either translate all titles in Japanese or none? A glaring example is in chapter 7, n. 5, where one title is translated and another is not.

At the outset, I said this was a bad but good book. So what's good?

Well, chapter 7 does a very good job of collecting and reporting the conventional wisdom about Japanese business practices versus American practices. Not much new here, but the sort of thing an IBM official would like to have read before doing business with the wily orientals—before 1973.

JIM DATOR

University of Hawaii

Conflict in Africa: Concepts and Realities. By Adda B. Bozeman. (Princeton: Princeton University Press, 1976. Pp. xiv + 429. $27.50, cloth; $12.50, paper.)

From the perspective of one who believes that cultural factors are both significant and frequently neglected in political analysis, this extremely well-written and erudite culturalist analysis of conflict in Africa is surprisingly unsatisfactory. The sources of this dissatisfaction can best be explicated through an attempt to summarize the substance of the author's complex thesis and to describe the methodology in terms of which she endeavors to demonstrate its validity, since questions can be raised concerning both individual pieces of the presentation and the way in which they are put together.

Bozeman begins with a synopsis of "occidental" thought on the subject of conflict, and a discussion of the ways in which this thought is culture-bound. Although recognizing the benefits of conflict in certain circumstances, occidental thought according to Bozeman basically assumes that conflict resolution is both possible and desirable, especially when conflict takes the form of war among nations, and further assumes that agreements possessing a high degree of lawfulness are the most effective instruments for achieving such resolution. Implicit in this line of thought is the assumption that cooperation and conflict, war and peace are polar opposites, or at least clearly distinguishable from one another.

Patterns of conflict and accord found in the contemporary African political scene are summarized next, all too briefly, and are shown to contradict not only the normative standards of western "peace studies" thinking, but also the empirical assumptions underlying them. The major points being made here—that there is a high level of conflict in Africa and that peace is not the primary value guiding African political behavior—are sound, but Bozeman unfortunately overstates the extent to which African domestic and international politics are characterized by violent and unremitting conflict, as well as the extent to which black African nations have mobilized their resources for a racially motivated confrontation with the white-ruled south. A more complete and systematic treatment of the degree and types of contemporary African political conflict would enhance the credibility of Bozeman's major thesis as presented up to this point without changing its basic tenor.

This criticism might be considered unfair, since the author's major purpose is to explain rather than to describe contemporary African conflict. These two processes are closely related, however, and inaccurate description of one's dependent variable can direct the researcher away from the most fruitful independent variables which might be utilized in explaining it. Bozeman utilizes a cultural explanation for the prevalence of conflict in Africa. She argues that, for a variety of reasons delineated below, conflict was the key to understanding precolonial African culture, and as the influence of the relatively brief interlude of European rule fades with the passage of time and the search for authenticity by African regimes, the precolonial conflict-oriented culture is rapidly regaining hegemony in the determination of African political behavior. The absence of a systematic treatment of contemporary conflict forecloses the possibility of relating it systematically to specific characteristics of precolonial culture, or of systematically comparing the influence of this independent variable with the influence of others suggested below.

An even more significant set of problems is raised by Bozeman's method of relating precolonial culture to contemporary culture and behavior, for she simply describes numerous points of congruence between the former and the latter, and assumes that this is sufficient to demonstrate a causal relationship between them. In fact, she usually speaks of African culture rather than precolonial African culture, and the inference that she intends to refer to the latter is based on the overwhelming preponderance of her examples rather than on her stated intention. Thus she may not be suggesting that precolonial culture influences contemporary culture, but rather may be equating them by definition. But African culture has undergone fundamental changes in recent decades under the impact not only of colonial rule itself, but also of socioeconomic change, nationalist movements, and postindependence events. A comprehensive culturalist explanation of African conflict must deal with the influence of these new culture traits. The obvious weakening of European influences and resurgence of African culture is not a return to pristine precolonial culture.

To suggest that the influence of culture change needs to be taken into account is to raise the question of why culture changed. The validity of culturalist explanations can be greatly enhanced by systematic analysis of the mutually causal relationships between cultural and other factors, especially material interests. Bozeman defines culture so broadly as to include many of these other factors, but this prevents a systematic analysis of their interaction with culture and one another.

The author's synthesis of the major characteristics of precolonial African culture and the place of conflict, war, and conciliation in it comprises the longest and best part of the book. Her sources are drawn from history, anthropology, political science, philosophy, literature, legal scholarship and travelers' accounts. Although she tends to treat all sources uncritically, she weaves them together masterfully into a coherent description and analysis. She suggests a number of interesting relationships between culture traits which have not previously been pointed out so forcefully or put together so cogently as causes of conflict. The prevalence of oral communication, an essentially atemporal perspective, a kinship-based social structure, an overwhelming preoccupation with supernatural forces, continually fissiparous political systems and continually changing political boundaries, and suspicion of strangers are all shown to be causes of the degree and types of conflict and conciliation which took place in precolonial Africa, and especially in reaction to Islamic and early European intrusions. Yet this synthesis, although stimulating and based on such extensive and varied citations, contains the same overemphasis on violent and virtually unmitigated conflict which characterizes the much more brief description of contemporary political conflict. In attempting to escape what she regards as the trap of ethnocentric occidental "peace studies" thinking about conflict, Bozeman may have fallen into the trap of portraying Africa as more conflict-prone than it actually was or is because of her reliance on some ethnocentrically biased sources.

JAMES R. SCARRITT

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The Modern Japanese Military System. Edited by James H. Buck. (Beverly Hills, Calif.: Sage Publications, 1975. Pp. 252. $15.00, cloth; $7.50, paper.)

This book comprises nine essays, which the editor, James Buck, calls "an attempt to understand some major aspects of the modern Japanese military system" (p. 10). The essays are practical rather than theoretical but eschew policy advocacy in favor of description and analysis.

Although the editor adumbrates no grand theme to hold the essays together, the essayists are ineluctably drawn into explaining why Japan has not and probably will not ever have a large defense establishment. Leonard Humphreys explains that the egregious behavior of the Japanese military command in the 1930s, culminating in the unconditional surrender of Japan in 1945, precludes using Japan's long military tradition as a tool in building pride in service among the soldiers and civilian acceptance of the armed forces. Martin Weinstein points out that strategic circumstances have led Japanese defense planners to construe the armed forces as a means not to defend Japan, but rather to heighten the threshold of attack. Thus, the primary purpose of the armed forces in external defense is to increase the probability that a Russian or Chinese thrust against Japan would be on a scale large enough to activate the security treaty between Japan and the United States and invoke an American response. Such a purpose does not require large forces.

Theodore McNelly points out that the Japanese courts have yet to settle definitively on the constitutionality of the Japanese defense establishment and suggests that this constitutional controversy adversely influences public attitudes toward the defense establishment. Using his own and other public opinion polls as evidence, Douglas Mendel writes that the Japanese public has little interest in defense and foreign policy, little sense of external threat, prefers "humanism" to "patriotism" as the guide to society, and rejects any big power role for Japan in international affairs. Although many Japanese support the existence of the defense establishment, they believe that its most effective role has been and should continue to be disaster relief. Many Japanese oppose the dispatch of Japanese troops abroad even for UN peacekeeping. "These public views must be considered by any leadership which wants to stay in power," he concludes (p. 171).

David Hopper points out that the business leaders do not lobby hard for the defense establishment. He adduces two reasons: first, the business leaders are not convinced that defense takes precedence in the aggregation of business interests; and second, defense production constitutes only a small percentage of the total output of any company. Thomas Bendle writes about the Japanese armed forces' difficulties in recruiting and retaining personnel.

Gaston Sigur points out that Finance Ministry officials are the real arbiters of defense policy and they give scant regard to the recommendations of the military commanders who, unlike their American counterparts, do not appeal for redress to the politicians who find welfare and environmental control spending more rewarding than defense spending. Evelyn Colbert writes that Japanese strategists tend to see an enhanced military capability as counterproductive to the growth of the nation's influence in Asia. James Buck believes that a change from a minimal defense policy would raise two of the most divisive and emotional issues in Japan–constitutional revision and nuclear weapons.

Only one essay results from primary research yet unpublished—David Hopper's "Defense and the Business Community." The other essays rely heavily on other research or are extensions of the essayist's primary research previously published. But that should not vitiate the value of these essays. They offer insights.

Douglas Mendel disproves the assumption made by many writers on Japanese security that a loss in Japanese confidence in American defense commitments would lead to a rapid military build-up. In fact, "the [Japanese] public has never equated U.S. defense capability with defense needs" (p. 173). He also points out that less than 10 percent of the Japanese public approves of extending military aid to Southeast Asia, Taiwan, or Korea, and less than 20 percent of the Japanese public approves of the United States helping defend either Taiwan or South Korea if these lands were attacked by communist forces (p. 170). He believes that support for the security treaty between the United States and Japan continues to diminish (p. 179).

Evelyn Colbert suggests that the growing Japanese economic presence in South Korea has made the Japanese circumspect in offering political advice (p. 215). In short, greater economic presence has resulted in less political influence. I disagree with Colbert's contention that this growing economic presence contains Japanese interest for greater intercourse with North Korea. Gaston Sigur has dug through the writings of Eisaku Sato, Nobel Laureate and longest-sitting prime minister since World War II and resurrected the following excerpt: "We [Japanese] must . . . be extremely careful not to take any step that might lead to an expansion of our self-defense capability to proportions which exceed our real strength" (p. 186). America's horse becomes a Japanese cart.

Sigur's quotation, Mendel's public opinion polls, and Colbert's observations are examples which demonstrate that Japanese views on security, the nature of military strength, and the importance of the various nations in the Asian balance of power differ considerably from American views. Despite statements to the contrary by high U.S. government officials, many problems remain to be resolved between Japan and the United States and security may well head the list. We need essays like those in this book if only to point that out.

NATHANIEL B. THAYER

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Party Identification and Beyond: Representations of Voting and Party Competition. Edited by Ian Budge, Ivor Crewe, and Dennis Farlie. (London: John Wiley, 1976. Pp. 393. $28.00.)

By an odd coincidence, 1976 brought us at least five books that will enliven debate on the roots of voting behavior for many years to come. Four of these—Nie, Verba and Petrocik's, *The Changing American Voter;* Miller and Levitin's, *Leadership and Change;* Knoke's *Change and Continuity in American Politics;* and Converse's *The Dynamics of Party Support*—are not here under review but their content helps provide perspective for evaluating the contributions of the Budge, Crewe, Farlie anthology.

Description before analysis: Most of the 20 papers in this collection were presented in 1974 at a workshop of the European Consortium for Political Research [ECPR]. The editors have organized the papers into three groups—"Party Identification: Its Theoretical and Measurement Status," "Dimensional Analysis," and "Rational Choice and Party Identification." Each group of papers is introduced by the editors with an analytic overview. These three introductory essays set forth the editors' version of the key issues addressed by the papers and they provide a context for the individual contributions.

In addition to these introductory essays, four papers—Warren Miller's "Cross-National Use of Party Identification as a Stimulus to Political Inquiry"; Peter Ordeshook's "The Spatial Theory of Elections: A Review and a Critique"; David Robertson's "Surrogates for Party Identification in the Rational Choice Framework"; and Dennis Farlie and Ian Budge's "Placing Party Identification within a Typology of Representations of Voting and Party Competition and Proposing a Synthesis"—are primarily concerned with questions of concept and theory. The remaining 13 papers deal with partisanship as an empirical phenomenon—as an independent or dependent variable. Here we have single-country studies of Belgium, Britain, Canada, Denmark, France, the Netherlands, Northern Ireland, Switzerland, and West Germany. Although these papers focus on the politics of a given country, they are more or less explicitly comparative. Two essays—Ian Budge and Dennis Farlie's "A Comparative Analysis of Factors Correlated with Turnout and Voting Choice" and Ronald Inglehart and Hans Klingemann's "Party Identification, Ideological Preference and the Left-Right Dimension among Western Mass Publics"—are based on analyses of comparable data from several nations.

Taken together, the essays provide a rich and varied fare. The publisher (John Wiley) has rendered a service by making these papers accessible. Those who use books as I do will judge the publisher's service diminished by the absence of an index. No index means much less efficient return visits to the book. At these prices. . . .

With a general description and my personal gripe stated, I can devote the remaining space to assessing the contributions of these papers separately and as a collection. The theoretical/conceptual analyses, especially the first introductory essay and Miller's, Robertson's, and Farlie and Budge's papers, add up to a clear presentation of the intellectual foundations of the notion of party identification. Warren Miller takes us back to basics: the relationship of the individual to the political party can represent an "extension of ego . . . because an important part of the individual's self-identity as a political actor is assumed to emanate from the sense of belonging to the political group. . . . Political loyalties often begin with the socializing influence of the family and of others, but they come to be incorporated in the self" (p. 22).

To this set of reference group assumptions, Miller adds a second set that speaks to the uses of party identification: "given a sense of belonging to a political party, there are many short cuts [to political comprehension] immediately available." "Party identification permits reliance on many others for cues and guides to one's own beliefs and attitudes." "A citizen who identifies with a political party comes to know habitually, if not instinctively, what to do on election day. All other things being equal, particularly in the absence of ambiguous or contrary information, the party loyalist votes and votes the party line" (p. 23).

In these few sentences, Miller reiterates the basis for the expectation that one's partisanship will be stable over time. So fundamental a part of the self will not change even if one deviates from the expected partisan vote in a given election—i.e., in elections where Miller's *ceteris paribus* is not met. Thus, we have the rationale for normal vote analyses and for the classification of elections in light of normal expectations.

Since nothing in these assumption-sets specifies that the psycho-dynamics of partisanship depend upon a particular party system and since citizens in democracies everywhere come to terms with their identities and the complexities of electoral choice, many of the studies collected here begin with the expectation that party identification will play a role, analogous to its role in the United States, in the electoral systems of European democracies. These scholars are fully aware of the results of previous studies of the crossnational generality of party identification (e.g., Campbell and Valen; Butler and Stokes; and Converse and Dupeux). These are new explorations with new tools (conceptual and methodological), and if some of the ground they cover has been covered before, it is covered more thoroughly in these studies.

As an overall matter, the single-country studies point toward the conclusion that European voters, including Britons, do not identify with party labels in the way Americans do. They are less apt to persist in an identification and more apt to change partisan direction with a change of vote. The Crewe, Thomassen and Kasse papers, inter alia, document these points.

If the matter rested here, we would be little ahead of where we were before. But through explorations of party spaces and dimensions of identification, these papers uncover a potential for stable identifications with parties clustered in political (not necessarily policy) spaces. The studies, published here, of Danish, Belgian, French and Swiss voters point in this direction; panel studies will be required to nail these conclusions down.

The Inglehart-Klingemann findings suggest that across the range of politics and party systems of the nations of the E.E.C. the basis of stable orientation may be self-identification as "left" or "right." But since they find that the basis of this orientation is party identification and religosity rather than policy-related ideology, their work is paradoxical from the perspective of the single-country studies. Again panel data will be required to resolve this paradox. If such data show that one's loyalty is to a set of parties occupying only a small segment of the political spectrum (as the cross-sectional data from Denmark suggest), Inglehart and Klingemann will have to reverse the causal arrow between left-right orientation and partisan identification.

All these papers will reward close reading. Each is interesting in its own right and collectively they represent the product of intense, sophisticated scholarship. They show a methodological creativity that is atypical in studies of American partisanship—perhaps the greater complexity these scholars face calls it forth. For the student of American voting behavior, these papers should be a further stimulus for a concentrated reexamination of party identification in the United States. If citizens in more tradition-bound and more complex politics find party less useful than other orienting cues, perhaps Americans orient themselves in similar ways. The papers here collected suggest that this question is worth a careful look.

RICHARD A. BRODY

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The Dynamics of Soviet Politics. Edited by Paul Cocks, Robert V. Daniels and Nancy Whittier Heer. (Cambridge, Mass.: Harvard University Press, 1976. Pp. 427. $17.50.)

This volume is a festschrift in honor of the late Merle Fainsod. Zbigniew Brzezinski and Adam Ulam have contributed the concluding and introductory essays, and 15 other scholars (Donald Carlisle, Robert Donaldson, John Hodgson, Jerry Hough, Gail Lapidus, Paul Marantz, Gregory Massell, Robert Miller, Richard Mills, Ruth Mouly, Teresa Rakowska-Harmstone, Dina Spechler, and the editors) examine a broad range of topics concerning Soviet political leadership, the policy making-administrative process, political and social change, and foreign policy—mostly in the post-Stalin years.

The editors do not comment on the organization of the book or on any of the chapters. Several pairs of essays complement each other well, but the book as a whole does not appear to be focused on a central theme, and no general conclusions are easily distilled. However, four essays deserve special commendation and will particularly interest students of comparative politics.

Hough introduces a new perspective on political participation in the Soviet Union. He shows that party membership among adult males with higher education is remarkably high (circa 50 percent—p. 125), contends that this important political activity is "extremely widespread" in comparison with comparable levels of participation in the West (pp. 125, 133), and concludes that "the mass character of the 'elite' party is a key fact about the USSR that has not been sufficiently absorbed into our understanding of that country" (p. 132).

Cocks demonstrates that both group and systems approaches can add to our understanding of Soviet politics—the former primarily to explain current bureaucratic realities, the latter to comprehend recent Politburo-level initiatives. Cocks maintains that top party leaders have encouraged systems analysis and PPBS chiefly to enhance the center's capacity to plan and "manage by objectives," to elicit administrative contributions in support of *national* goals, and to control the major bureaucracies. The comparisons of Soviet and Western models and experience are also instructive. (Miller's lucid discussion of the historical antecedents and course of the Soviet debates about administrative science between 1964 and the early 1970s complements Cocks' study nicely.)

Massell's is by far the most theoretically ambitious and conceptually innovative chapter. Seeking to contribute to and benefit from the general literature on political development, Massell inquires: "How, and to what extent, may political power be used in simultaneously modernizing and integrating a culturally and ethnically heterogeneous milieu?" (p. 267) Specifically, what is the capacity of the Soviet system to reconcile "the building of a new unitary order with the full-fledged participation of all national minorities in that order?" (p. 266) In a sophisticated response to these questions, Massell argues that Soviet modernization and national policies in Central Asia have had "markedly contradictory" effects, which produce serious policy dilemmas and the distinct possibility that present forms of centralized control are increasingly hindering effective modernization (i.e., social mobilization and economic development) *and* contributing to political instability throughout the area. (Carlisle's study of the Uzbek intelligentsia provides detailed historical background and substantive and methodological contrasts to Massell's.)

Brzezinski, emphasizing the similarities between the Tsarist and Soviet political systems' relationships with society, concludes that the adaptive capacity of the present polity is "narrowly circumscribed" (p. 349). Brzezinski concedes that the current levels of legitimacy, efficiency, and stability do not pose serious problems to the leadership, but he contends that dominant elements in Russian and Stalinist political culture—and the constraints imposed by "the national question"—are increasingly impeding the problem-solving capabilities of the system. Because these capabilities must be *enhanced* in the present period of rapid social and scientific-technical change (in order to maintain the present levels of political control, legitimacy, effectiveness, and stability), Brzezinski concludes that a process of "degeneration" is under way. (Hough's study does not tend to support this conclusion; Massell's does.)

The three chapters on substantive policy issues are solid contributions. Spechler offers a gracefully written analysis of official policy toward different kinds of dissent (e.g., "social pragmatism," "cultural liberalism," "historical revisionism") between 1955 and 1964. Notable is the early dissident concern with "the alarming discrepancy . . . between the level of Soviet technology and the moral development of the Soviet people" (p. 42). Lapidus examines changing policies in the field of education from 1917 to the mid-1930s. Her purpose is to illuminate the evolving relationships between utopian and developmental goals, and she concludes that Stalinism "represented a redefinition of the meaning of both socialism and modernity rather than a shift from utopia to development" (p. 219). Mills discusses the Brezhnev administration's technical decisions concerning the Virgin Lands, in the context of larger agricultural and economic issues (e.g., might not regular grain imports facilitate planning and be less expensive?).

The two essays on Soviet international behavior cover a lot of ground but are focused more on perceptions than policy. Marantz succinctly traces the history of the concept of "peaceful coexistence," details the Khrushchev innovations, and briefly analyzes the sociopolitical *functions* of this changing precept and conceptual orientation. Donaldson reconstructs evolving Soviet views of the global balance of power in the early 1970s, explores the interconnections among assessments of relations with the United States, Western Europe, and China, and emphasizes the similarities between recent campaigns for "collective security" in Asia and Europe.

Heer scrutinizes Soviet historians' treatment of important political leaders and issues (e.g., Stalin). Daniels convincingly maintains that the Central Committee constitutes a rather stable set of "job slots" and that membership reflects, but does not influence, "status relationships" within and among the major bureaucracies. Essays on the politics of succession (Hodgson), "leadership maintenance" and its alleged "requirements" (Rakowska-Harmstone), and contemporary youth (Mouly) round out the volume.

In short, this is a useful but somewhat disparate collection of original essays devoted primarily to contemporary Soviet domestic politics and to significant but generally familiar topics. Scholars and graduate students will especially profit from selected specialized studies, undergraduates from the analytical overviews. And all readers will find insights, questions, and data—many of them presented very briefly—which (as Ulam rightly notes) should stimulate further research on Soviet politics. As a tribute to Fainsod this book will supplement, but not be supplanted by, Hough's forthcoming revised edition of *How Russia Is Ruled.*

ERIK P. HOFFMANN

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Politics and the Migrant Poor in Mexico City. By Wayne A. Cornelius. (Stanford: Stanford University Press, 1975. Pp. xiv + 319. $12.50.)

This is an excellent study of politics and the migrant poor in Mexico City. It is theoretically stimulating, methodologically rigorous, empirically illuminating, and broadly informed about and pertinent to comparative studies concerning similar populations and communities in other countries.

The study is based on a stratified probability sample of male heads of family, aged 18 to 65, in six predominantly low-income communities on the periphery of Mexico City. The stratification variable was length of residence in Mexico City. Approximately two-thirds of the respondents were migrants to Mexico City, the remainder being nonmigrants. A subsample of eldest sons of migrants was also drawn, such that in one-fourth of the migrant families in the sample both the head of the family and his eldest son were interviewed separately. Interviews averaged between two and three hours. Although 747 interviews were completed, a weighting procedure yielded 1062 cases for analysis (Appendices A through D reproduce the English version of the questionnaire, and specify other pertinent research procedures in detail). The survey was supplemented by participant observation, in-depth interviews with key informants, and archival research.

The principal theoretical concept orienting the study is the importance of the immediate community context to the process of political learning among the migrant poor. Cornelius shows convincingly that it is not helpful to generalize about the political behavior of urban migrants because there is such variation in this behavior. Some migrant communities exhibit intense political involvement, a high and positive sense of community, and general and specific affect for the political system. Other migrant communities exhibit political passivity and alienation, and a weak sense of community. Cornelius provides situational and structural explanations to account for these considerable differences. The experience of founding, defending and improving one's community—or the failure to do so—emerges as a powerful explanatory variable focusing on community-government relations.

The book also shows that, under certain

circumstances, there is substantial and successful political activity among the migrant poor. Political demand-making in migrant communities focuses first on security of land tenure (if they were established first through land invasions); concerns then focus on basic urban services such as water supply, sewerage, and street pavement. Cornelius also specifies the instances when these kinds of demand-making are neither frequent nor successful. He sheds light on the preference of Mexico City migrants for petitioning the government for assistance, rather than relying on self-help efforts as much as in other Latin American cities. Petitioning in Mexico City is favored by the government, to build political support and to control the migrant poor; and it is favored by many migrant poor as a way to obtain certain community services.

There is little evidence, Cornelius argues, for two alternative, plausible hypotheses. First, there is little support for the view that the experience of migration to the city is politically radicalizing by itself. Migrants overwhelmingly prefer to manipulate the political system rather than to confront it. "It is not the sheer amount but rather the *kind* of urban experience that determines a migrant's political behavior" (p. 108). It is thus neither urbanism nor urbanization that explains migrant politics, but the political learning occurring in the migrant's immediate environment. Secondly, there is also little support for the view that migrants are "marginal" to the political process, e.g., that they are cognitively or behaviorally uninvolved in the formal political processes and institutions of city and nation. On the contrary, less than a tenth of the migrants can be considered marginal to politics. Most of them vote in elections regularly, and over half have engaged in some form of political activity beyond voting. Although differences between migrants and nonmigrants on most dimensions of political activity are small, migrants have a higher rate of voting participation.

The government side of the relationship is not so well covered in the book. One learns little about government institutions which deal with the migrants except through the latter's views. The organization, goals, strategies, and constraints of the governments of Mexico and of its capital city, as they seek political support and control, could have been analyzed at greater depth. The question of the institutional dimensions of migrant politics arises also because Cornelius shows that community organizations among the migrant poor tend to break up once a goal is achieved; successful organized political participation is associated with a subsequent decline in political participation. Political organizations of the migrant poor in Mexico City rarely become institutionalized, shifting their goals or adapting to new circumstances. Political demands are parochial, short-term, and not aggregated across communities. "Cases of community organizations that start with a narrowly defined function and subsequently evolve into general purpose lobbying groups are extremely rare" (p. 197). Cornelius describes a case study of a community organization that eventually folded; it would have been useful to have more comparative data to assess this aspect of community political institutions in greater depth.

This book points to a new kind of study about cities which may be more theoretically rewarding and empirically accurate than those of the past. Future studies may seek to be comparative within the same city, to avoid generalizing about the whole city on the basis of a single neighborhood study. Studies may be less concerned to explain the revolutionary potential or the apathetic marginality of poor migrants, and may be more concerned to explain why and how their political behavior varies so much. Studies may seek to understand why certain residential contexts stimulate political activity, while others retard it. There could be fruitful efforts to compare types of residential environments within and across cities, rather than merely to seek to compare cities to each other. In all of these endeavors, this book serves as a model of scholarly insight and research design implementation.

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Civil-Military Relations in Sierra Leone: A Case Study of African Soldiers in Politics. By Thomas S. Cox. (Cambridge, Mass.: Harvard University Press, 1976. Pp. x + 271. $14.00.)

During the last decade we have witnessed the establishment of military regimes in a majority of African states and attempted coups in almost every other African nation. Sometimes military rule has been brief—in most cases it has been a dominant feature of political life. The rash of military coups in Africa has spawned a growing literature examining this phenomenon. Thomas Cox's *Civil-Military Relations in Sierra Leone* focuses on one aspect of the military and politics, the role of civilian and military ties in military intervention and rule. He examines the relationship of these links to: ethnicity, divisions within the military, their effects on creating conditions for coups, and their changing character over the years.

Cox argues that it is the development of cleavages within the army that provides the basis for most coups (p. 12) rather than the politicization of the military (p. 19). He suggests that these cleavages result in links with civilian groups eager for power or fearful of losing it–links which result in intervention for particularistic and personal reasons rather than primarily to overthrow corrupt, inefficient, or venal regimes. Cox seeks to demonstrate that civilians played a key role in Sierra Leone coups (p. 18)–a situation which he argues is generalizable to other parts of Africa.

This case study of Sierra Leone begins with a brief history of the Sierra Leone Army in colonial times. The author goes on to assess its role in the immediate postindependence period, traces the politicization of the officer corps during the Albert Margai regime, and discusses growing problems and tensions within the military. The last half of the study focuses on military intervention in political affairs, coups and counter-coups, the politics of the military in power, and civil-military relations after the return to civilian rule in 1968.

The book is of uneven quality. There are moments of insight and some fascinating material and speculations. At other times the analysis is thin and the conclusions questionable. The first chapters of the study are largely repetitious of other published work on Sierra Leone. It is the second half which is most valuable for its data, description, and analysis. Part of the problem with some parts of the book relates to the presentation of material. The section on colonial rule and the army, for example, is often paternal in tone, focusing on Sir Milton's "eye for the ceremonial" (p. 36) rather than the serious problem of how to maintain a separation between the military and politics–a task Sir Milton handled extremely well. There is a great deal of material based on rumor and labels are often accepted uncritically as when the All People's Congress (APC), the major opposition to the governing Sierra Leone People's Party (SLPP), is tagged "radical" and linked to violence without supporting data. Similarly, Cox seems to accept allegations (p. 106) that the APC was planning a coup against Albert Margai prior to elections. Major APC involvement in a coup attempt seems highly unlikely since the APC leadership expected to win the election and would only dilute their position if power had to be shared with the army.

Another problem relates to the emphasis on primordial and family ties (including marriage) as explanatory factors. Cox treats the two major political parties as ethnic parties (a questionable assumption) and suggests that primordial factors provide a basic link between senior government officials and military personnel (p. 80) involved in the coups. Cox argues that ethnicity was important in recruitment and promotion during the Albert Margai government. Noting the increased recruitment of Mende officers during this period, he states: "[the statistics] show that, largely as a response to fears of possible antigovernment sentiments within certain sections of the army . . . the prime minister decided upon creation of a Mende-controlled army" (p. 75). The statistics certainly *show* neither sentiments nor decisions. Cox notes, but fails to emphasize, the importance of nonethnic opposition within the SLPP–opposition to particular policies which stands on its merits with or without ethnicity. The opposition to Albert Margai by important Mende SLPP leaders like Jusu Sheriff, L. A. M. Brewah, and Kutubu Kai-Samba are much more plausible related to issues like blatant corruption, decision making in the party, democracy versus the rule of a clique, national development and national priorities. While ethnic identification is a tempting (and perhaps possible) explanation of alliances and actions, the proposition is not convincingly supported and contrary explanations are neither well analyzed nor destroyed. While primordial sentiments are important in Sierra Leone, they cannot bear the explanatory weight Cox attributes to them.

The most important contribution of this study is the analysis of cleavages within the military and the ways in which they played a part in military intervention. Cox traces the development of divisions within the military and attempts to identify and evaluate ties between politicians and military officers. This is a difficult task even under ideal research conditions and the evidence, understandably, is often speculative or circumstantial. The focus is important because it gets away from the usual "lofty" motives attributed to coups both by perpetrators and analysts. Cox is most convincing when he details the divisions between the force commander, Brigadier Lansana, and his senior officers. His analysis of the cleavages within the NRC during the period of military rule is among the best material to date.

Cox completes his study with a discussion of civil-military problems after the return to civilian rule under Siaka Stevens and the APC. While he correctly documents the difficulties encountered in controlling the military, he does not seem to recognize the problems Stevens faced on his return to power. Stevens was confronted with a military without officers (they had been imprisoned by the enlisted men in their counter-coup which brought back the previously elected APC government) and a history of political intervention. The conclusion to *Civil-Military Relations in Sierra Leone* is disappointing. It is weak, particularistic, and includes few attempts at generalization.

The major contributions of this work consist of some interesting data, analysis and speculations about alliances and cleavages in the military and some very useful information on the period of military rule. This material is suggestive for those concerned with the military in politics in general and for those who are interested in Sierra Leone in particular.

FRED M. HAYWARD

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Patterns of Political Leadership: Egypt, Israel, Lebanon. By R. Hrair Dekmejian. (Albany, N.Y.: State University of New York Press, 1975. Pp. 323. $20.00.)

This is a study of some characteristics of cabinet members in three Middle Eastern countries. Cabinet members were chosen because Dekmejian believes that the "cabinet constitutes the most important institutional power collectivity" (p. 4). Elites are described as a component of the polity, and if politics are defined "in terms of power, nothing can be more central to it than the study of those who hold power" (p. 3). The book does not attempt "to find the locus of power and identify the individuals who exercise it" (p. 4). Instead, Dekmejian contents himself with assembling some interesting and some not so interesting information about individual cabinet members. The regime context is passed over lightly, but average age, jobs held before and after, religion, and education are tabulated, graphed, listed and discussed in the text. Three chapters about Lebanon, Israel and Egypt briefly recapitulate major cabinet changes. A final chapter compares the three and briefly notes that similarities and differences among cabinet members are due to a number of independent variables which are not the major subject of the book (e.g., the party system). Finally a few paragraphs are devoted to the cabinets' effectiveness and possible changes in policy as the result of changes in cabinet members' education, occupation, and socialization. The most significant conclusions are that the Egyptian elite has been strengthened by winning the war in 1973 while the Israeli cabinet has been weakened. Nasser's and Ben Gurion's departures improve the prospects for peace.

It is unrealistic to expect that area studies applications of elite analysis will yield significant theoretical breakthroughs, or even useful methodological innovations. For the most part, the area specialist is handicapped by a dearth of quantifiable data, by contact with only limited segments of the population, and often by a lack of adequate knowledge of the languages used in the area. To cope with these difficulties and at the same time to employ analytical paradigms critically might appear to be too much to expect. On the other hand, the essence of comparative analysis is revealed precisely when a paradigm which appears to make sense in one context is recognized as obviously inappropriate in another. By failing to take advantage of an opportunity to make such meaningful distinctions, Dekmejian has deprived us of a useful comparison of these three regimes and he has passed up an opportunity to offer a critique of elite analysis itself.

LEONARD BINDER

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Coalitions in Parliamentary Government. By Lawrence C. Dodd. (Princeton, N.J.: Princeton University Press, 1976. Pp. xx + 283. $14.50.)

For many political scientists, coalition theory has at best only a heuristic value for theoretical exercises in scope and methods classes. But coalition theory has another dimension to its reputation: it is thought to be empirically sterile concerning problems of importance to many teachers who nonetheless find it a useful pedagogical tool. Dodd's important book should go far toward removing the stain of empirical sterility from coalition theory in the study of parliamentary politics, for it demonstrates that the theory has considerable utility when imaginatively applied.

Dodd sees his own study as "an extension and synthesis of two schools of comparative analysis" (p. 226). The first is represented by the work of A. Lawrence Lowell and others who emphasized "the potentially critical influence that a parliament's party system may have on cabinet durability" (p. 226), arguing "that durable cabinets require majority party government" (p. 6). Unfortunately, Dodd notes, the Lowell school was essentially inductive and descriptive, and its practitioners "failed to develop a coherent framework that could explain the linkage between party systems and cabinet durability" (p. 226).

The second school of analysis, of more recent origin, is reflected in the works of many contemporary political scientists like Riker, Axelrod, Browne, Groennings, De Swaan, and especially Michael Leiserson, who "emphasized the potential utility of game-theoretic models in the study of multiparty parliamentary settings" (p. 21). Unfortunately, Dodd contends, the Leiserson school focused primarily on predicting the appearance of "minimum winning coalitions" while failing to link "the study of parliamentary coalitions to fundamental political questions" (p. 227).

Dodd proposes to make this link by relating cabinet coalitional status to cabinet durability. For "cabinet coalitional status," Dodd envisions a continuum ranging from "*under*sized or less-than-minimum winning cabinets" through "minimum winning cabinets" to "*over*sized or greater-than-minimum winning cabinets" (p. 18). This sounds very much like the application of Riker's "size principle" to the prediction of coalition formation, and indeed it is—and it isn't. Dodd is not really interested in predicting which coalitions will form; hence he does not introduce any constraints of ideological compatibility among parties as a criterion for coalition eligibility (p. 121). Instead, Dodd is interested in specifying the parliamentary conditions which predict to the formation of undersized, oversized, or minimum-sized coalitions. Specifically, he considers (1) the degree of a priori willingness to bargain (cleavage conflict among the parties, or their issue positions weighted by their proportion of seats), and (2) information certainty (party system fractionalization and instability).

Dodd's treatment of these factors is primarily verbal rather than mathematical, so no one should be prevented from following his theory for lack of mathematical facility. Nevertheless, his discussion is complicated by the interactions hypothesized in his theory, as different degrees of willingness to bargain and degrees of information certainly will "produce exactly the opposite behavioral patterns" (p. 128). At high levels of willingness to bargain, low information certainly produces *over*sized coalitions. At low levels of willingness to bargain, low information produces *under*sized coalitions. At moderate levels of willingness to bargain, high information produces coalitions of minimum winning size (pp. 132–33, 208).

Dodd tests his propositions about parliamentary conditions predicting to coalitional status primarily with data on 238 cabinets in multiparty parliaments (those with no majority party) in 17 "western" nations from 1918 to 1974, omitting the war years of 1941–1944. Additional data on 41 cabinets in majority parliaments enable him to extend his analysis beyond the multiparty situation. Using mainly regression analysis, Dodd concludes "that the complex interaction between party system fractionalization, instability, and cleavage conflict determines cabinet coalitional status," with these three variables accounting for over 40 percent of the variance.

Despite his evaluation of these findings as "strong, parsimonious, and theoretically coherent," Dodd asks the critical question "whether they matter: is cabinet coalitional status related to cabinet durability?" He employs an admittedly limited definition of durability: "a cabinet exists so long as there is no change in the parties that compose the cabinet" (p. 122). The distribution of cabinet durability by coalitional status is triangular, and Dodd's grapplings with the relationship conclude that coalitional status explains from 21 to 27 percent of the variance in durability (pp. 140–42). It follows from his theory that "cabinet durability is an indirect function of the parliamentary party system," and Dodd estimates that the same three variables that explain coalitional status also explain about 20 percent of the variance "among peace-time multiparty parliaments, 1918–1974" (p. 143). Dodd devotes considerable time to examining the stability of his findings for the "interwar" (1918–1940) and "postwar" (1945–74) periods and to extending his findings to cabinets in majority parliaments. By and large, the theory applies in both investigations.

For Dodd, his research is important for puncturing the "myth of multipartism" and its corollaries: "*the myth of party coalitions:* coalition governments are necessarily nondurable; *the myth of majority partism:* countries seeking durable cabinets must achieve majority party government and hence a majority party system" (p. 10). By stating these "myths" so categorically, Dodd can be accused of constructing straw men, for the presumed consequences of multipartism are surely couched in statistical rather than universal statements. Disregarding Dodd's bit of hyperbole, one can hope with Dodd that his study "will help remove the blinders from scholars concerned with parliamentary politics" by revealing that "party coalitions can and do endure" (p. 243). Dodd's findings encourage him to speculate about the positive contributions rather than the negative consequences of multiparty politics. Regardless of one's reactions toward his speculations concerning democratic theory, it is significant to note that a book on coalition theory yields empirical findings that prompt such speculation. It is to Dodd's credit that he has convincingly demonstrated the empirical utility of the theory in the study of parliamentary politics.

KENNETH JANDA

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China's Forty Millions: Minority Nationalities and National Integration in the People's Republic of China. By June Teufel Dreyer. (Cambridge, Mass.: Harvard University Press, 1976. Pp. 333. $14.00.)

June Dreyer's excellent study fills a longstanding need for an account of China's policies toward her national minorities. Six percent of China's population, some 40 million people, belong to approximately 50 recognized national minorities. Although some of these groups are small, primitive tribes, others are large populations with complex cultures living in areas of great strategic importance and containing important natural resources–for example, the Tibetans, Uighurs, and Mongolians.

Anyone who teaches about China has been asked for reading materials on China's minorities policies. If the Chinese have dealt creatively with problems of agricultural modernization, health care delivery, and law and order, have they also found new ways to deal with the intractable problems of ethnic and cultural diversity? Have the dominant Han (ethnic Chinese) managed to integrate minorities politically while protecting their cultural autonomy? Has Chinese policy in this area, as in others, diverged from Soviet practice? What is the influence of the Chinese tradition? Dreyer answers all these questions in clear, unpretentious language which will be accessible to students interested in China mainly as a comparative case study; at the same time, she connects trends in minorities policies to national political events (for example, the Great Leap Forward, the Cultural Revolution) with sophisticated Pekingology that China specialists will admire.

The book is organized chronologically, beginning with chapters on minorities policies under the traditional dynasties and the precommunist republic. There are excellent analyses of Soviet theory and practice and of Chinese Communist policies before the assumption of national power in 1949. The next five chapters deal with variations in policy during the consolidation of power, the Great Leap, the post-Leap retrenchment, the Cultural Revolution, and the recent period up to 1975. The conclusion (and sections throughout) point to the relevance of the Chinese experience for the general problem of cultural pluralism. Each chapter, as appropriate, discusses not only overall central government policy but the specific experience of major nationality areas.

Dreyer handles a complex subject with verve. Her analysis covers not only a wide geographical and temporal scope but a great range of topics and methods. At one extreme she deals with policy toward national arts and costume; at another, with the composition of successive Central Committees. Whether describing foreign policy, agricultural policy, language policy, cadre policy, shifts in propaganda messages or changes in leadership composition, her judgment is sound and her command of the context firm.

In principle, the book should open the way to more detailed studies of the experiences of particular minority areas under Chinese Communist rule. In practice, there may not be sufficient source material to support such studies, except perhaps for the largest areas and those from which refugees are available such as Tibet. In any case, Dreyer's findings are not likely to be overturned. Students of cultural pluralism can now include the Chinese case in their studies with confidence.

ANDREW J. NATHAN

Columbia University

Comparative Union Democracy: Organisation and Opposition in British and American Unions. By J. David Edelstein and Malcolm Warner. (New York: Halstead, Wiley, 1976. Pp. viii + 378. $19.95.)

Organizational theory for the most part emphasizes bureaucratic and essentially undemocratic organizations. The components of organizational democracy are seldom given more than tangential recognition and are considered of no more than peripheral relevance in general scholarship in the field. Most social theorists have argued that oligarchy in complex organizations is highly likely, have offered reasons for this, and let it go at that. Large unions, like large business enterprises and government agencies, are considered to have certain essential structural characteristics which predispose them, no matter how formally democratic they may be, to domination by the few. This pessimistic bias regarding complex organizations has been particularly noticeable in the case of trade unions. Contrasts between unions and other organizations ordinarily emphasize the labor organization's supposedly greater obligation to respond to membership desires and lament failures in this regard.

It is true that union democracy is fragile and easily shattered, empty formalism too often taking the place of genuine membership influence on decisions. The authors here contend, however, that there is more competition for top posts in British and American unions than is generally believed. More fundamentally, their study assesses the underlying variables in organizational structure which makes the leadership selection process in some unions in each country more competitive, hence presumably more democratic, than others.

Edelstein and Warner proceed on the assumption that democracy inheres in unions in which consistently effective opposition regularly competes with incumbents on a roughly equal basis for top offices. They conclude that such equality can be encouraged if autonomous suborganizations are permitted at regional or occupational subdivisions of the national union, and that such autonomy is possible to a greater extent than Robert Michels or S. M. Lipset have thought possible. They have far less confidence in externally-imposed guarantees of civil rights procedures than do commentators such as Alice Cook and Robert Dahl, although they emphasize that officer selection by membership referendum rather than election by convention delegates tends to ameliorate inequality of power.

Factually, the findings of the volume are interesting. Top union officers were more frequently defeated for reelection in the period studied, 1949–1966, in the United States than in Great Britain. More British union elections were closely contested, however, even though incumbents were rarely defeated. The authors seem unsure of what to make of this—whether close reelections of "semipermanent" leaders nevertheless evidence democracy. They suggest such results may occur because British unionists with stronger Socialist traditions than their American counterparts are less tolerant of the genuine factionalism that leads to real change.

Alternatively, I would hypothesize that these results, and the fact that the authors find them somewhat surprising, may stem in part from the authors' emphasis on structure rather than procedure as the dependent variable in union democracy. In the United States in 1959 the enactment of the Labor-Management Reporting and Disclosure (Landrum-Griffin) Act forced many unions to revamp their election procedures to conform to the standards of Title IV of the new law. Two of the three case studies of union elections in the United States contained in chapter 10 of this volume were heavily influenced by Department of Labor activities under the Act. While the authors are correct in several of their criticisms of the Act's provisions and enforcement, the slight attention given it—a little over one page in 360 pages of text—hardly seems appropriate. In fact, throughout the volume the authors, though respectively nationals of the two countries they study and each with research experience in the other's nation, seem somewhat more comfortable with British than American experience. On several matters they rely too heavily on secondary and slightly questionable American sources.

Despite these few misgivings, I welcome this study as a worthwhile addition to the slim literature on the internal workings of unions as organizations. In emphasizing the amount of leadership change that does take place it serves as an antidote to the scholarly and popular impression of unions as just one more bureaucratic oligarchy. Moreover, in times marked by the growth of the conglomerate and multinational corporation as well as a growing development in some nations of trilateral negotiation of tax-wage-price policies, both of which increase the pressures for centralization of decision making, concern with maintaining democracy within unions is important. Government of unions without the consent of the governed will in time make both economies and nations unstable.

CHARLES M. REHMUS

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Canadian Political Parties: Origin, Character, Impact. By Frederick C. Engelmann and Mildred A. Schwartz. (Scarborough, Ontario: Prentice-Hall of Canada, 1975. Pages and price not given.)

The appearance of a new book on Canadian political parties should be an occasion for applause. Compared to the literature on American parties, the Canadian literature is grossly underdeveloped and almost any addition should be an improvement. Unfortunately, this is not the case with Engelmann and Schwartz' new book. The book, a substantially revised, reworked, and expanded version of the authors' *Political Parties and the Canadian Social Structure* (Scarborough, Ontario: Prentice-Hall of Canada, 1967), attempts to examine and explain the origin, character, and impact of Canadian political parties. However, the promise of the book is largely unfulfilled.

The authors have two points of departure: a modified version of Duverger's classification of political parties and a systems analysis of Canadian parties. In the introductory chapter, the authors pose five questions about parties ("what are parties, how do they come into being, how many parties are there, what do parties do, and what kinds of parties are there" [p. 3]), make a number of observations about parties in democratic systems, and introduce a typology of political parties, classifying them according to organizational form (cadre v. mass), focus of appeal (pragmatic v. principle), and breadth of support (broad v. restricted). Subsequent sections examine "the roots of Canadian political parties in historical traditions and social and political institutions" (p. 21), more immediate influences, parties as conversion agencies, and finally the uses of the party system.

The authors poke into so many nooks and crannies of the Canadian political system that it is difficult to summarize their conclusions. Early on we learn that Canadian parties have developed because of the need to provide responsible government, that party discipline is a response to the needs of operating a parliamentary system (which doesn't explain why it exists), that federalism has permitted the development and survival of third parties, and that many cleavages in Canadian society have been taken up by all parties. Later on, we learn that the prevalence of cadre parties reflects Canadian social structure (though it is not clear why this should be so), and that parties have indeed contributed to legitimacy and effectiveness, but not to the degree that they could. A variety of other points are raised, often in the form of caveats. The authors conclude by introducing a number of hypotheses about the effects of competition among the types of parties delineated in the introductory chapter.

The major themes and conclusions of the book are difficult to summarize largely because they are blurred by its approach and organization. Rather than telling us what Canadian parties are like and why, and why this is important, the authors employ systems analysis and follow a rigid progression from inputs to outputs. The authors treat parties as distinct sub-systems with their own inputs and outputs. Rather than following customary usage and discussing the functions of political parties, the authors are forced to talk about the inputs and outputs of political parties. As a result they end up telling us that, among other things, nominations and the provision of government are outputs of political parties. The language is cumbersome and unilluminating.

These difficulties are compounded by poor organization. The book is written around a comprehensive set of chapter and topic headings which function as a series of boxes which must be filled in in one way or another. These obscure the subject matter and interrupt the flow of the narrative. For example, chapter IV treats cleavages under the rubric of "internal and external stimuli to party formation" (p. 71). Surely, though, the relationship between cleavage and party systems (though by no means automatic) is important and well enough known to merit treatment in its own right. This chapter is followed by a section entitled "Origins: Special Influences" (p. 93), containing chapters on elites, mass media, and interest groups. Each of these is comprehensive but disjointed and full of material only tangentially related to the subject matter of the book. The chapter on elites is typical of the rest: the authors present a general discussion of elites, defined as anyone from admen to intellectuals. On the way, they digress and discuss the relationship between politicians and civil servants, the formation of electoral strategies, the prior experience of prime ministers, "the consequences of elite involvement in party politics" (p. 115), and whether or not there is a power elite in Canadian politics. Most of these topics have more to do with the effects or outputs of political parties than with their origins.

These difficulties might be forgiven if they were in the interest of either systematic testing of propositions or greater comparability. However, neither is achieved. The attempt at systems analysis falls flat and the book ends up as a descriptive exercise in which conclusions are asserted rather than demonstrated. In view of the paucity of the literature about Canadian political parties, this might have been useful, but the flow of the description is constantly interrupted by awkward terminology, and poor organization. While in the concluding chapters, Englemann and Schwartz attempt to develop hypotheses and draw conclusions, their proclivity for universal categories gets in their way. The authors employ Parsonian categories (leadership, policies, resource mobilization, legitimacy, effectiveness) to evaluate the influences of parties on society. However, by the time the authors operationalize these terms, they have lost general applicability and comparability. Leadership, for example, is operationalized as the provision of single or multi-cleavage leadership, while effectiveness is measured by cabinet duration. The former definition is narrow and cumbersome and more closely related to integration than leadership, while effectiveness and cabinet duration might well be inversely related. In the last chapter, the authors develop hypotheses about the effects of competition among different kinds of parties, defined by the typology developed in chapter 1, but the patterns of competition, delineated in charts full of initials (e.g., c-b-p x m-r-i, pp. 314, 316–17) are confusing and difficult to grasp, and the hypotheses are often trivial or tautological. For the first hypothesis (H1) tells us that "competition between cadre-broad-pragmatic parties encourages multi-cleavage leadership." Surely the fact that parties are broadly based requires them to have broadly based–i.e., multi-cleavage–leadership.

There are more criticisms to be made about this book. The quality of writing is poor and sometimes turgid. There is little original material. Basic data needed for the convenience of the reader–e.g., election results–are often missing. Reading the book, one cannot help wondering whether the book was edited, nor can one help feeling that someone else could have done a better job synthesizing the existing literature. However, to date, no one else has tried. Readers interested enough to force their way through the book in spite of its poor writing and poor organization will find a wealth of material and sources and many interesting observations about Canadian parties and politics. Nevertheless, if the authors had abandoned their pretense at systems analysis and simply told us about Canadian parties, they could have produced a better and far more useful book.

STEVEN B. WOLINETZ

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Elite Recruitment in Democratic Polities: Comparative Studies Across Nations. Edited by Heinz Eulau and Moshe M. Czudnowski. (New York: Halsted, Wiley, 1976. Pp. 299. $17.50.)

For scholars interested in solid empirical work on elites in modern societies, this is an extremely useful and suggestive collection. It consists of six original studies employing data from six countries (U.S., West Germany, Netherlands, France, Italy, and India), studies which use data, often ingeniously, to test various components of democratic theory. Two of the studies are explicitly comparative (U.S. state legislators, and French and Italian mayors); three rely heavily on historical analysis. The six empirical analyses are preceded by two introductory discussions by Heinz Eulau and Dwaine Marvick which review the developments in the field, link current work with the earlier directions suggested by Harold Lasswell and others, and discuss models and approaches emerging from current work.

Clearly the study of the recruitment of political elites is an area of inquiry which has tremendous potential, both in terms of data availability and theoretical promise. As Marvick puts it, out of a congeries of marginally-related studies we are now seeing the "de facto definition of the research field" (p. 40). These new empirical efforts demonstrate what can be done and what needs to be done. There is still today, what Lasswell noted in the last stocktaking evaluation which occurred in 1961, a "conspicuous lack of elegant intellectual unity" (p. 12). Yet, more careful application of theory is now evident, as well as more rigorous testing of propositions which are relevant to understanding the linkage of elite structures to social and political reality. This is, then, more sophisticated empirical work than we have previously seen, and probably intellectually additive in the long run.

Among the questions asked in these studies, a few of the most central are:

(1) Do full-time incumbent political officeholders reveal different personal and systemic value preferences than part-time or aspiring activists, or than the average voter? Moshe Czudnowski explores this through interviews with three types of 66 Illinois politicians.

(2) Are state legislators who are recruited by the party ("party agents") inclined to have stronger partisan attitudes and to be more loyal to party in roll call voting behavior than coopted candidates, entrepreneurs, or careerists; and how does this relationship vary by type of state and district? Three Iowa scholars (C. L. Kim, J. Green, and S. Patterson) examine this question by reanalysis of original data from a 1962 study, plus interviews with 122 Iowa legislators in 1967.

(3) In what respects in a modern industrial society do the social attributes and governing skills of the "appointed executive elite" change over time, and are these changes a result of party fluctuations or a function of the gradual broadening of the recruitment base and/or "a skill evolution"? Kenneth Prewitt and William McAllister examine this carefully with the biographical data of some 700 U.S. federal executives in selected periods from 1930 to 1970.

(4) In a non-Western democratic polity, how do "pre-apprenticeship" patterns (concern-

ing caste, education, family income, occupation, family political socialization) differ for parliamentary candidates, state legislative candidates, local party activists, and non-party political personnel, as well as for those politicians whose careers are "localist" in contrast to those who are "translocal" or "in transit"? Dwaine Marvick documents these differences with an analysis of a national sample of 1,000 Indian leaders who were interviewed at the time of the 1967 election.

(5) To what extent do municipal council members selected through a *partisan* electoral process (in the Netherlands, in contrast to a *nonpartisan* process as in the San Francisco area) reveal distinctive role orientations and perceptions of the importance of party in their political careers? Galen Irwin analyzes the Dutch data from interviews with 316 council members and commissioners, and attempts a general comparison with Bay Area study results.

(6) Do local political elites recruited during a period of crisis exhibit different characteristics (in age, occupation, party attachment and political activism) than those recruited in noncrisis periods; and in terms of what type of model does one explain the differential systemic findings in such recruitment patterns? Sidney Tarrow and V. C. Smith seek an answer to this problem with data from interviews with 121 Italian and 109 French mayors.

(7) How "open" is the political elite in relation to other elites (labor unions, business, bureaucratic, mass media, and others), in terms of social profiles, socialization and career patterns, and exchange relationships? Werner Kaltefleiter seeks an answer by presenting 1972 interview data for 1825 German leaders from all sectors.

(8) When political-electoral realignment occurs in a system, does elite recruitment contribute to the occurrence of the crisis, and what are the effects of the emergence of new leadership on political coalitions, elite behavior, and policy output? Michael King and Lester Seligman look systematically at the data on U.S. members of Congress from 1872 on, contrasting the "critical election" years of 1896 and 1932 with six other noncritical elections.

These are significant questions, answered in these studies with a detailed analysis of complex reality. The hypotheses are at the micro-level, but they have important macro-level relevance. In the process, broad-gauged theories are implicitly (and occasionally explicitly) tested, such as Lasswell's theory of the accountability of the elite, Huntington's theory of the poor linkage between electoral coalitions and governing coalitions, speculations about Third World political development, Michels, Pareto and Mosca's views on oligarchy and the "circulation of elites," and current theories from specialists in the U.S. on realignment. One can see these studies as explorations of bits and pieces of larger theoretical concerns.

One may well deplore "discontinuity" and the disjunctive character of current research, as well as plead for more longitudinal and truly comparative work. Yet these studies perform a service in alerting us to the fascinating variety of elite analyses possible, illustrating various types of designs, and pointing to the values and consequences of such systematic research efforts. We have much elite and mass data available, both historical and comparative; it is now a matter of doing what Lasswell exhorted us to do years ago, to exploit effectively this tool of elite analysis.

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Public Policy and the Smoking-Health Controversy: A Comparative Study. By Kenneth Michael Friedman. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. 216. $15.00.)

Public-policy analysis has become a major preoccupation among political scientists in recent years. One would expect comparative analysis to be a major focus for such work; however, few such studies have appeared. Case studies in a single setting have rather become the primary vehicle for this type of investigation. In view of the complexity of the public policy process in any setting, such an eventuality is not too surprising. Nevertheless, from a research perspective, precious little can be generalized from most case studies. So Kenneth Friedman is to be applauded for his attempt to undertake a comparative study into the efforts of three governments—the United States, the United Kingdom and Canada—to deal with a common policy problem.

The choice of these three governments would appear to be most appropriate for comparative study. All three speak the same language and enjoy a common political and economic heritage; two have parliamentary and the other a presidential form of government; two have a federal and the other a unitary system of government. The potential danger of tobacco consumption to health has been a major public-policy issue in all three countries for the past ten years. Thus, given these circumstances, it should appear possible to determine how differing forms and systems of government affect the operation of the public-policy process.

Friedman approaches his study by presenting case studies of the smoking-health controversy in each country. In order to facilitate comparison, he organizes the information presented in each case study according to: interest-group behavior, governmental policy and cigarette consumption.

These case studies reveal a remarkably parallel set of actions taken in each country. In each instance, public concern was triggered by the publication of scientific reports by the health community, followed by: (1) the requirement of a warning on the product (cigarettes) and/or product advertising; (2) the introduction of filter-tipped cigarettes with lower tar and nicotine content; (3) the banning of cigarette advertising on radio and television; (4) the required reporting of tar and nicotine content on the product; and (5) the demand for the setting of a maximum tar and nicotine content allowable in cigarettes sold. Not only have the same types of actions been taken in all three countries, but in each instance the actions have occurred in practically the same time sequence.

It is true that the means employed for taking such actions have differed in all three countries. In the United Kingdom and Canada, with parliamentary governments, voluntary action by the tobacco industry has been stressed, while in the United States legally based actions by Congress and administrative agencies have predominated. In Canada the threat of the development of fragmented policy by the provinces has been an impetus for the emergence of national policies. One may assume that these differences can be attributed to the different forms and systems of government found in the three countries.

Yet these differences may be due as much or more to the differences in the economic circumstances existing in each country. In the United States tobacco growers and manufacturers are both concentrated in the same states, while in Canada growers are concentrated in different provinces than manufacturers. Agricultural price-support politics is important in both countries; but given the smaller number of growers in Canada and that government's strong traditional support to producer cooperatives, the system almost certainly operates somewhat differently there than it does in the United States. The United Kingdom, of course, differs from both the United States and Canada in that its major economic concern in the smoking-health controversy deals not with the growing or manufacture of tobacco but with the tax revenues received from its sale. Although tobacco tax revenues are important to all three governments, they make up over 80 percent of the retail price of cigarettes in Britain compared with less than 50 percent in the United States. (In Canada the proportion varys from 45 percent in some provinces to 75 percent in others.) Finally, the economic dependence of Canada upon both the United States and Britain has certainly affected that country's stance on the tobacco-health issue.

In view of the multiple decision points and multiple decisions revealed by these case studied, Friedman concludes that the use of the concept, power or "who governs," is far too ambiguous an analytic tool to be helpful in identifying how the policy process operates. He then considers the use of four other possible analytic models: (1) Referee, where government lays down the rules for the competition of groups and associations; (2) Vector-Sum, where the legislature resolves conflicting interests into a national policy; (3) Interest-Group Liberalism, where government assures access to the decision-making process; or (4) Competitive Government, where different legislative, administrative and political groups compete to control public policy while maximizing their own clientele's needs. Friedman concludes that only the Competitive Government model is sufficiently reflected in the three case studies to provide a viable tool for studying the public policy process. The other models all suffer from oversimplifications which prevent them from reflecting the dynamics of the process.

In short, while the author finds the decision-making process too complicated to draw firm conclusions about how different formal governmental configurations affect policy formation, he does suggest that the Competitive Government model emerges as a viable perspective for studying the operation of the public policy process in different settings. This is a fascinating conclusion, especially in view of the fact that the actions of all three governments on this policy issue appear to have had approximately the same effect on tobacco consumption within their respective constituencies. Unfortunately, Friedman fails to develop the characteristics of the Competitive Government model sufficiently to provide an operative tool capable of answering why this has occurred. (The author does make reference to the origin of the model in Niskanen's *Bureaucracy and Representative Government*, Aldine Atherton, 1971.)

Most of the study is taken up by the

presentation of the three case studies; and even the case studies are quite truncated. The chapter on the United States, for example, should certainly be supplemented by a reading of Lee Fritschler's *Smoking and Politics* (Aldine Atherton, 1975), especially in relation to the competitive practices of the various governmental instrumentalities involved. But the comparative style Friedman has employed to present the cases is certainly useful. If he had gone on to use the case material to elaborate the Competitive Government model, his contribution to the comparative study of public policy could indeed have been significant. But he has launched out into a field of investigation few have deigned to travel. And in so doing he has demonstrated the feasibility of comparative study. Everyone interested in this form of investigation should certainly give careful consideration to Friedman's volume.

FREMONT JAMES LYDEN

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Regionalism and National Unity in Nepal. By Frederick H. Gaige. (Berkeley: University of California Press, 1975. Pp. xvii + 234. $13.75.)

Frederick Gaige has written an interesting and worthwhile account of the *tarai* region of Nepal and the efforts of the Nepalese government to link the *tarai* more closely with the country's economy and its central administration. Nepal's *tarai* region encompasses the southern 15 percent of Nepal and borders upon India; some 30 percent of Nepal's population live in this plains region. Many of the *tarai*'s linguistic, commercial, and religious characteristics are shared with adjacent India and the efforts of the governing elite in the capital, Kathmandu, to extend its rule to the *tarai* provide a focus for Gaige's study.

Gaige devotes the first section of his book, three chapters, to the *tarai*'s differentiating traits. His vivid description of the *tarai*'s specialized physical and cultural landscape is based on an extensive field survey of village communities completed in 1967 and 1968. These chapters consider also the interaction between the *tarai* and India and how that interaction is viewed by Nepalese decision makers. His second section, seven chapters, deals with the problem of national integration, with "nation-building policies." Throughout, Gaige exploits skilfully those publications of the government of Nepal which were made available to him.

If Nepal is to become a nation, Gaige contends, the plains people must be drawn into the national framework. He describes those policies of government which seek to develop the *tarai*'s economy. He also offers a clear account of how the Kathmandu elite expects to profit from that region's development. Of particular value is Gaige's description of the government's land reform program and the discontent which the administration of that program fostered among those inhabitants of the *tarai* who had little or no land. When the *tarai*'s land became attractive to the influential in the Kathmandu Valley, the landless and the small landowner in the *tarai* found only very limited access to land titles. New landowners from the hill regions were able to meet the restrictive requirements the government had established.

Gaige's book should appeal to those interested in Nepal. It is also a fine case study of a small developing country which is experiencing tension between two of its major regional groups.

MERRILL R. GOODALL

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The Mensheviks: From the Revolution of 1917 to the Second World War. Edited by Leopold H. Haimson. Translated by Gertrude Vakar. (Chicago: University of Chicago Press, 1975. Pp. 476. $22.50.)

This volume fulfills a major goal of the Inter-University Project on the history of the Menshevik movement, begun in 1959 under the direction of Leopold Haimson. It brings together the historical recollections of five major Menshevik figures—David Dallin, George Denicke, Leo Lande, Boris Sapir, and Simon Wolin—who join their individual energies and narratives to construct a general history of Menshevism from February 1917 to the Second World War. Each contributor has focused on the events of the movement he knew best; and each has supplemented his memory with documentary materials from Menshevik archives. During the volume's preparation, each contributor also discussed his work in biweekly seminars with American and European scholars, and other Menshevik survivors. The resulting book is at once an invaluable source for any future work on Menshevism, and a history in its own right. As a source, it contains recollections and perspectives of men whose lives have been immersed in Menshevism, and who maintained a remarkable publicistic activity through five decades of émigré life. As history, it is a richly detailed narrative, remarkably thorough in its presentation of major party activities and its analysis of changes in program and theory. It is also thoroughly annotated, enhanced by an excellent index, a chronology, and appendices on the Menshevik Unification Congress of 1917, the Social Democratic press in 1918, and the Menshevik trial of 1931. Its very publication, in fact, might be seen as one of the last integral parts of Menshevik party history.

It is not, however, an easy book to review. Its attributes as a memoir-history, and hence as a source, are clearly different from its function as an historical narrative. Its contributors consistently offer a rich mass of detailed information throughout but as with all collective histories, the quality of insight and analysis vary, as do points of emphasis and focus. As such, it is an invaluable contribution to, but not a substitute for, a full party history.

As an historical narrative, its principal general strength is the enormous mass of information it contains, and its principal weakness is a general narrowness of vision. Chapters on 1917 (by Leo Lande) supplement extant literature with detailed discussions of Central Committee activities in the summer of 1917, the disintegration of the defensist bloc, the party's attitudes towards the last coalition, the Bolshevik seizure, and Vikzhel; but they make little effort to comprehend the revolutionary movement as a whole, or even to evaluate the Mensheviks' particular role in the evolution of Social Democracy in this period. Chapters on the Civil War (by David Dallin and George Denicke) constitute the strongest part of the volume as an historical narrative, but assume a comprehension of underlying social and economic processes few general readers are likely to have. Simon Wolin's account of the Mensheviks under NEP and emigration will similarly be most interesting to those who understand the dilemmas of Groman, Sher, Cherevanin and others who stayed and worked in Moscow, and who are generally familiar with other aspects of emigration politics. Hence as history, the book will be engrossing for specialists, and a rich mine of material for those doing further work. Others, however, may find its very richness overwhelming.

To me, the volume is more interesting as a source of Menshevik perspectives. Most striking is the way each contributor views the revolution as essentially a political, rather than a social process. "Democracy" as the term is used in the volume, means political form, not social leveling; and collectively, the authors fail to exhibit what one might call an "orthodox Marxist" concern for the social underpinnings of political development. What is compelling for each is the relationship between revolution and the collapse of civil liberties, an obvious focus given the volume as memoir-history, but also a revealing one in terms of the movement's historical approach to revolution. The profound changes in social relations which underlay the Bolshevik consolidation of power are little noted, nor are their implications drawn in terms of Menshevik strategies.

The question of the Constituent Assembly, for example, is presented as: "Did it represent the essence of democracy, or was democracy possible without it?" (p. 182), not in terms of whether a parliamentary form of this sort was either possible or appropriate to Russian social conditions in 1918. Similarly, one follows Martov's tortuous arguments ("mainly in private conversations," Dallin reports [p. 182]) that democracy in Russia meant civil rights and a rule of law—arguments which underlay the party's formal abandonment of the Assembly in October 1918; but nowhere is there any clear evidence that Menshevik leaders then or later appreciated the correlations between social levelling, the evaporation of legality, and the Mensheviks' own political weakness. In the turbulent months of 1917 and 1918, Russia needed above all a clear-headed, consistent, democratic government, with the strength to enforce its policies and to effect Russia's social revolution systematically and carefully. Little in this volume suggests that the Mensheviks had this capability in any greater degree than their political adversaries, or enhances the reputation of Martov and his colleagues for political perspicacity.

Also revealing, I think, is the fact that for all its brilliance of detail, which allows for a new and full appreciation of Menshevik organizational and ideological diversity, the volume as a whole indirectly reinforces a commonly held view of Mensheviks as ideological scholastics. It does so because its contributors examine each change in the party's program and theory with extraordinary attentiveness, while entirely ignoring questions concerning its organic base, the practical problems of political mobilization, or such aspects of practical politics as organizational strengths and weaknesses. Thus one finds an interesting discussion of the oppositional Conference of Factory Representatives in 1918, but no discussion of the problems of translating oppositional groups into an active political strategy. And one reads in detail about the December 1918 party conference, which David Dallin notes "marked the beginning of a revision of ideology and policy that was to absorb the party's best minds until Martov's death" (p. 196); however he finds little effort to correlate these ideological revisions with practical activity. Thus those accustomed to simplified generalizations about the Mensheviks' preoccupation with theory are not likely to find much here to alter their conceptions.

Finally, there is, in my view, an interesting woodenness in Menshevik perspectives here, perhaps the result of contradictions between the goals of the volume as memoir and as history. Throughout there seems to be an almost painful strain for dispassion, an effort to present material objectively, without traces of emotion. Similarly, there is little effort at sustained analysis or evaluation, as if efforts to judge were considered inappropriate to objective history. As a result, the warm and committed personalities of the contributors are hidden, as are the subjective sides of the party's collective experience. Perhaps this is because the authors felt their views and feelings have been extensively presented elsewhere; perhaps they wanted very much to avoid "memoir," and write "history." On the other hand, perhaps this represents the Mensheviks' own historical dispassion, their determined effort historically to relate to Russian events with unemotional objectivity. In either case, the result, I think, is a certain loss: one comes away without really knowing more about Dallin or Denicke than before, and in this sense, the volume's usefulness as a source is diminished.

None of this, however, diminishes the extraordinary achievement of the book's publication. It is a singular validation, if one were needed, of the importance of the Inter-University project.

WILLIAM G. ROSENBERG

University of Michigan

Freedom and Labour: Mobilization and Political Control on the Zambian Copperbelt. By Peter Harries-Jones. (New York: St. Martin's, 1975. Pp. 256. $19.95.)

The publication of this book represents a major step in interdisciplinary cross-fertilization between anthropology/sociology and political science in the study of Zambian politics. These disciplines have a long history of cooperation in Zambia, with both sides having borrowed from one another, but Harries-Jones has brought them much closer to merger. His work is good political science written from the perspective of the anthropological/sociological approach of network analysis; the theoretical literature which he cites in framing his central questions is drawn from all three disciplines. The most significant contribution of this particular interdisciplinary approach is to provide an extensive description and explanation of the relationships between the lowest organizational level in a nationalist movement about to become, or having recently become, a governing party, and other organizational levels in that party.

The setting for Harries-Jones' study is Luanshya, a city in the Zambian copperbelt which has been the subject of several major research projects. Where possible, the author compares his data with the findings of previous studies, thus providing a valuable longitudinal dimension to some aspects of his own analysis. During the period covered by Harries-Jones' research, the dominant nationalist party in Zambia—the United National Independence Party (UNIP)—was transformed from a clandestine movement seeking to organize demonstrations against the colonial government into the governing party of an independent state. He demonstrates that, at least in Luanshya, this transformation involved a substantial change in decision-making relationships among organizational levels, and that the party was unable to adapt to these changes in a manner satisfactory to the mass of participants at the lowest level—the section. As a consequence of this adaptive failure, there was a substantial increase in the degree of elitism in the party, a development which has had considerable long-run significance.

After presenting some historical background, largely in the form of a manuscript written by a woman party organizer to detail the difficulties of early mass mobilization efforts in Luanshya, Harries-Jones turns to traditional concerns of urban anthropology—the interactions among kinsmen, home mates (people identifying themselves as coming from the same place), and neighbors by means of which they adjust to the conditions of urban life. Political mobilization of such people into party sections, carried out by other such people who assume the role of sponsors, is shown to be a part of this adjustment process. Because it was based on existing relationships, especially those among home mates, and because it gave urban dwellers the opportunity to work for improvement of their situation, this mobilization was extremely successful, and at the same time relatively noncoercive. A fascinating example, described at some length by Harries-Jones, was the UNIP Funeral Committee, which made honoring the deceased and political mobilization mutually reinforcing activities.

The mobilization of large numbers of people by a nationalist party raises problems of control: the party leadership wants the attitudes and behavior of followers to conform to party policies, while the followers want the leaders to be responsive to their perceived needs. Combining anthropological and political science perspectives allows Harries-Jones to examine control from both above and below. From above, he examines the roles of the branch, the constituency, and the region, which are, in that order, ascending levels of party organization, and thus have differing perspectives on policy issues as well as on mass participation. From below, he examines the attempts made to reform or ostracize dissidents through the spreading of derogatory rumors about them, and the dissidents' attempts to retaliate with the same technique.

Harries-Jones' analysis is weakest when he shifts to a more macro level and discusses the emergence of elitism among Copperbelt Africans. Previous and subsequent to this stage of analysis he focuses on the municipal African residential area of Luanshya, but here he switches over to the separate mining company township, without giving any justification for doing so. He correctly argues that the elite which emerged in the latter area through the creation of an association for African salaried staff separate from the African Mineworkers Union was not able to exercise effective political and moral leadership in the struggle for African rights. Since he turns next to the manifestations of elitism in UNIP in the municipal mayoral election, the reader might assume that this elitism was equivalent to that manifested in the salaried staff association. Such an assumption would be unwarranted, and Harries-Jones does not really appear to make it. His digression away from the municipality to the salaried staff association, however, remains unexplained.

Mobilization and control can easily come into conflict with one another, as Harries-Jones demonstrates in his discussion of the mayoral election. Here pressures from above—from the national, regional and constituency levels—to have a highly competent and experienced mayor, and to make the selection on a nonracial basis, came into sharp conflict with pressures from below to have a mayor representative of the majority of the people. The outcome of this conflict represented a victory for the higher organizational levels, as a number of African councilors ignored the decision reached in a public meeting and cast their votes to elect a European mayor. Some of those who did so are quoted as saying that educated people cannot be greatly influenced by the opinions of illiterates, but since the emergence of elitism among Africans in the municipality is not described, the reader does not fully understand the relationship between local elitism and party pressures from outside Luanshya in the making of this decision. Some observers of the Zambian scene would argue that outside party pressures represented the elitism of the party's national leadership, but this argument is outside the scope of a study of one city.

The mayoral election is seen by Harries-Jones as an example of UNIP's failure to adapt to increased organizational complexity engendered by its assumption of new functions as a governing party. This explanation of the election and the significant trend which it exemplified is quite sound, but incomplete, and needs to be supplemented by an analysis of the emergence of an African elite within UNIP in the municipality. But no book does everything which it should ideally do, and what has been said above clearly indicates that reading this book is a necessity for all those who want a full understanding of Zambian politics.

JAMES R. SCARRITT

University of Colorado

Comparative Public Policy: The Politics of Social Choice in Europe and America. By Arnold J. Heidenheimer, Hugh Heclo, and Carolyn Teich Adams. (New York: St. Martin's, 1975. Pp. iii + 296. $12.95, cloth; $5.95, paper.)

American public policies usually deviate from the pattern of other western countries, and the deviation is usually on the cheap side. Most other countries with which we are more or less akin plan better cities, run better trains, build more public housing, provide more equitable income maintenance, etc. The litany is familiar; the specifics are not. How do we differ, in detail, from Sweden, Britain, Germany and other European countries? Why do we differ? The *how* question is answered pretty well by Heidenheimer, Heclo, and Adams. As to *why*, the answers they provide are disappointingly familiar and flat. We have fragmented decision structures, weak parties, a funny ideology, and a strangely loose social structure. Sometimes another country gets a low score on a particular policy, but the U.S. consistently brings up the rear (with the qualified exception of education). This book will tell you how much the difference is—usually in clear and historically sensitive detail. It will leave you wondering why—in terms more sophisticated and generalizable than the common textbook notations of our party incoherence, fragmented federalism, etc.

The citation for the 1975 Gladys M. Kammerer Award of the APSA says this volume "should provide a major stimulus to cross-national comparative studies of public policy" (*PS* 9 [Fall, 1976], 441). The encomium is deserved. The book will be useful in one of the most rapidly expanding and theoretically promising areas of political inquiry. Those of us who, in our seminars and research, are now trying to map and explain variations in national policy patterns should all welcome this book. Good detail is good scholarship.

This work's claim upon scholarly attention is warranted primarily by the clarity and richness of descriptive material on the content and evolution of major policies. It compares the experiences of Britain, Sweden, West Germany and occasional other examples (e.g., Netherlands, France) to the policy performance of the United States. The breadth of description of alternative policy options and the forces active in their evolution are backed up by an excellent bibliography. The book provides a fine base from which Americanists can reach out and examine the experiences of those other countries with which we most often compare ourselves. Persons schooled in the comparative patterns of a particular policy area can begin broadening their substantive coverage with the aid of this volume.

Alas, if one is seeking theoretical refinement or improved explanations of policy variation across nations—a stage to which the field must soon move more vigorously if it is to earn its scientific stripes or be of value to policy planners—this is not the book for the job. The authors disclaim any effort at constructing a policy model (p. 1). But they have one anyway, which would be familiar to the advocates of a responsible party system and rational unified executive planning. The fundamental problem, however, is that the descriptive mode pursued here—with notable variations from chapter to chapter—makes it very difficult either to trace through the causality hypothesized or to map the direction or sequencing of relationships of the political, social, and administrative forces discussed, let alone to suggest modes of verification or falsification.

Comparative Public Policy contains occasional citations of efforts at more rigorous hypothesis testing, whether in the case analytic mode of Anthony King (*British Journal of Political Science*, 1973) or the more aggregate, i.e., larger N, approach of Harold Wilensky (*The Welfare State and Equality*, 1975). But no effort is made to suggest anything that might approach second-order causation, especially for the American policy peculiarities. Would not the relative impact of war and its devastation, combined with population density, for example, help explain some of the variation in (and low U.S. commitment to) public housing, urban planning, or transport? And surely the fact of biraciality deserves more than passing notice in assessing the full gamut of American deviation from the cross-national pattern.

This volume inventories key policy differences; it suggests dimensions of variance in some detail. That will surely be useful for future efforts at explanation. But the fundamental terms of comparability between units (the U.S. vs. Sweden? Amsterdam vs. New York City?) are usually ignored. Multiple causes (e.g., ideology, government form, socioeconomic forces) are listed rather than sequenced, thus playing into the hands of correlation-hunting debunkers who prefer the simplicity of partialing to theoretically more sensitizing techniques such as path analysis or process modeling.

It is a cliché of comparative inquiry that case studies inform and enrich broad generalizations based on large numbers. But, at the risk of being charged with incurable quantophilia, I beg the case analysts—especially those as erudite as Heidenheimer, Heclo and Adams—to relate their narratives in more than a perfunctory manner to the general findings of their allies in the field; to be more self-conscious about the equivalence of their comparisons; and to array hypothetical causal factors in a manner subject to clear statement, if not quantitative test.

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The Limits of Administration. By Christopher C. Hood. (London: Wiley, 1976. Pp. xii + 213. $14.95.)

In his seminal 1887 article, "The Study of Administration," Woodrow Wilson was optimistic about the possibility of developing a "science" of administration. Let the political leaders make the basic policy decisions, said Wilson, and then let administrators execute those decisions neutrally and efficiently. Wilson's search for a scientific and rational system of administration has been kept alive by generations of planners, policy analysts, and managers. But other students of administration—particularly in recent years—have pointed to numerous difficulties inherent in administering public policy.

Christopher Hood's book adds to this latter tradition by providing a rich description and analysis of administrative pitfalls. The book begins by presenting a model of "perfect administration"—characterized by authoritative lines of command, uniform rules, clear goals, perfect information, and adequate time for action. Then the author shows how the perfect model breaks down in practice. He deals skillfully with the numerous administrative problems, among them coordination, recalcitrance of nominal subordinates, limits on information, difficulties of categorization, and conflicting time perspectives.

These topics have been examined before, but Hood brings to his analysis a good eye for organizational detail and a host of imaginative illustrative examples. To make his points about administrative difficulties, the author draws on materials from public and private administration in a number of countries and historical periods from ancient times to the present. Beginning with a Dickens novel, he moves easily and unostentatiously to early Egyptian taxation strategies, to recent works on public policy. And to prove his points he makes use of three in-depth case studies (on contracting and procurement, on taxing land speculators, and on taxing gamblers).

While the richness of detail constitutes a major strength of this book, it is also a weakness. Analytical points are often accompanied by so many examples, supporting sources, and counter-arguments that the main thrust of the argument becomes hard to follow. This is particularly true in the case studies, which contain an over-abundance of historical and substantive factual material.

Although the symptoms of failure in program execution have become depressingly familiar to readers of the literature on public policy, this book provides some fresh insights into a number of administrative problems. These include the difficulty of framing useful general rules and the particular dangers of rules that are too strict. Certain organization theorists have recently written about "organizational learning" as an aid to solving administrative problems; Hood convincingly points out the ways in which the objects of administration can also learn, thus creating new difficulties for administrators. The discussion of types of administrative control is also useful, and the use of multinational examples will be welcome to students of comparative administration.

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No Easy Choice: Political Participation in Developing Countries. By Samuel P. Huntington and Joan M. Nelson. (Cambridge: Harvard University Press, 1976. Pp. x + 202. $10.50.)

This brief monograph has its origin in a report by the authors to the Agency for International Development, which funded a research program for investigating political participation and its interactions with socioeconomic modernization. The research program was based at Harvard's Center for International Affairs between 1969 and 1973, and involved case studies of political participation in Columbia, Kenya, Pakistan, and Turkey, as well as crossnational and intensive studies of peasants and the urban poor in several nations. In only four densely packed chapters, Huntington and Nelson succeed in linking various individual-level findings from this and other research programs with a comprehensive theory of developmental processes and choices. Although few of the individual findings are new, the integrative effort and empirical review demand the attention of all students of participation and of development.

After defining their concept of participation, a much more sophisticated and many-faceted concept than appears in Huntington's extremely influential and important formulation *Political Order in Changing Societies*, the authors introduce a theory of modernization, participation, and developmental strategies. As in Huntington's earlier work, they find totally inadequate the old "liberal" model of development, in which all of the good things of economic growth, equality, participation, and stability "went together." They argue, rather, that "in the early phases of modernization, a conflict exists between the goals of socioeconomic equity and political participation, while in the later stages there is a conflict between economic growth and political participation.... In most developing countries, the dominant political elites must choose among the various goals and means" (pp. 168–69).

Huntington and Nelson introduce, therefore, a fourfold typology of dynamic developing systems: bourgeois, autocratic, populist, and technocratic. The typology is based upon the participation-enhancing effects of the level of social mobilization and upon the chosen elite strategies for political response. In each type, there are fundamental conflicts between often desired goals; in each type, participation plays a key role in the dynamics; in each type, "vicious circles" of instability can emerge. This analysis, with its sophisticated attention to dynamics operating at different levels, and its emphasis on the effects of conscious elite strategies and choices, provides a powerful context for the analysis of the causes and effects of participation at the individual level. It is likely also to receive substantial attention in its own right, as an extension of Huntington's earlier formulations.

In the three subsequent substantive chapters, the authors focus more intensively on participation itself; first in terms of macrointeractions between socioeconomic modernization, socioeconomic equality, and participation; later in terms of microtheories of individual behavior and the forms and causes of participation. Effective and imaginative use is made of the theories of social status, group identification, and organizational involvement as alternative paths to participation, which will be familiar to readers of the work of Sidney Verba and Norman Nie. The general microanalysis is supplemented by an especially valuable chapter on political participation of the poor, and the alternative models and routes available for individual mobility and mobilized participation among the disadvantaged. The authors also bring to bear here some of the perspectives offered by students of clientelism and political machines. The interactions of leaders' goals, participants' objectives, and the scale and form of action are analyzed from the perspective of the special obstacles to participation by the poor.

Despite its many merits, the essay is not without limitations. Little attention is given to the construction of elite coalitions which can integrate various participation patterns into support for alternative developmental strategies; nor to the economic and cultural conditions, such as land availability and reform, population dynamics, ethnic fragmentation, and international dependence, which facilitate such alternatives. We also miss systematic analysis of the development and maintenance of those political structures, such as types of parties and clientele networks, which, as the authors emphasize, do so much to encourage, inhibit, mobilize, or repress various types of individual involvement. The authors also deliberately eschew discussion of two other developmental types: revolutionary mobilization systems and "low-level, steady-state" stagnation, often coupled with instability. The latter is one of the most common operative types. The benefits of a narrow focus on participation, in short, also entail certain analytic costs.

Despite the inevitable constraints on such a brief monograph, which is preliminary to expanded studies by both authors, the questions posed, and the answers proposed, in *No Easy Choice* are worthy of the attention of scholars in a variety of fields. The efforts to join micro- and macro-levels, process dynamics and policy strategies, empirical data and theoretical perspectives, are insightful and invigorating.

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The Arab Left. By Tareq Y. Ismael. (Syracuse, N.Y.: Syracuse University Press, 1976. Pp. vii + 204. $15.00, cloth; $5.95, paper.)

At one level, this is a fine presentation of a complex body of literature on radicalism and revolution in the Arab world. The author displays a deep understanding of the issues, and shows an incurable curiosity to explain the emergence of a new form of leftist ideology in this part of the world.

The book is a mere 125 pages plus appendices, which are rich with translated documents, policy statements, and constitutions of the movements under discussion. The main topics are: Transformation of Arab Nationalism (into leftist, and National Liberal movements); The Socialist Arab Ba'ath Party; The Progressive Socialist Party in Lebanon; the Arab National Movement; Nasserism (written by the author's wife); Political and Organizational Roots of the New Left; Ideological Strategies of the New Left. The author relies heavily on sources translated from the Arabic to shed light on the above. Interestingly enough, we are told that the New Left has emerged as a result of the evolution of all the movements in the book, but most importantly of the Arab National Movement (ANM). The ANM began as a "petty bourgeois" ideology, influenced by Nasserism at first. Ironically, most of the Palestinian extreme leftist organizations, and their offshoots, trace their origin to the ANM.

At another level, the book seems fragmented, without analytical framework. We are told other leftist organizations were excluded from the discussion because they do not fall into a specific set of categories: National, Neutral, Socialist, Radical. We are not told how these categories were devised, and why. An inherent problem, thus, is conceptualization. As a result, arguments are lost. For example, the chapter on the Ba'ath attempts to discuss ideology, theory, history, events, all at once. This leads to confusion, and dilutes the central issues. In other words, there are too many levels of analysis in each chapter.

By relying heavily on translating the primary sources from Arabic, the author shows skill in delving into complex research. But, it is perhaps natural that in light of his background, he gets caught up in the literature representing these movements, and hence in presenting the material, he reproduces it as believers would understand it. But, of course, the analyst must do more. In addition to faithfully rendering the thoughts of the actors so as to convey the meaning of their ideas, he must also ask himself: what are the anchor points of this kind of thinking? Why can the believers and their movements see harmony where others see contradiction? What really holds together these seeming incompatibilities (like nationalism and socialism, like individualism and community, like freedom and necessity)? What role, for example, does Islam play in the overall ideology of such movements? For Islam symbolizes for many such movements, a return for a passing age, a longing for history, and a never-ending search for identity. This is inherent in the search for the ultimate solution to the discrepancy between political fragmentation of the region in general, and the past glories of the Pan-Arab Empire. It is therefore the myth, not the party nor the ideology, that gives rise to such contemporary movements.

Finally, I disagree with the author that the New Left, "... will sustain, flourish, and perhaps, dominate the politics of the area, if suffering of the Arab masses is not alleviated and if the basic aspirations of these masses are not considered" (p. 125). The New Left emerged as a temporary phenomenon as a result of the tragedies of the Palestinian wars of 1948 and 1967. For, when the Arab leaders within the existing state system managed to appear victors (as in the 1973 War), the masses seemed content with such leadership at the expense of the so-called Left, and of course, this sheds light on whether ideology takes precedence over political leadership, or personality politics in the Arab world. I happen to believe the opposite. Arab movements are created to augment charismatic leaders. Arab leaders know only too well that in order to be effective, they must rely on their personal appeal to win the support of the Arab masses whose loyalties are gravely split along religious, ethnic, regional, local, and even cultural lines.

Apart from all this, Ismael's book will remain indispensable as a handbook for the serious student of extreme leftist organizations in the Arab world.

KAMAL ALI BEYOGHLOW

University of California, Berkeley

Handbook of Major Soviet Nationalities. Edited by Zev Katz, Rosemarie Rogers, and Frederic Harned. (New York: Free Press, 1975. Pp. 481. $25.00.)

This is a much-needed reference book on politically the most important nationalities that inhabit the Soviet Union. Originally, this handbook was commissioned by the U.S. Information Agency and prepared under the auspices of the International Studies Center of MIT. The study was first mimeographed and then with only slight changes (including the same mistakes) published by the Free Press. The book's 16 contributors are Richard Pipes, Dina R. Spechler, Roman Szporluk, Jan Zaprudnik, Rein Taagepera, Frederic T. Harned, Mary K. Matossian, Richard B. Dobson, Frank Huddle, Jr., Zev Katz, Allen Hetmanek, Aman Berdi Murat, Donald S. Carlisle, Teresa Rakowska-Harmstone, Gustav Burbiel, and Stephen Fischer-Galati.

The book is introduced by Richard Pipes of Harvard whose main thesis is that modernity has not destroyed nationality and nationalism, but that, on the contrary, in some cases it has even helped to fan it. Pipes considers that the nationality question in the Soviet Union has reached "a decisive state of development" (p. 5) which will be followed by intense conflicts between the Russians and non-Russians. The first part of his thesis is empirically demonstrated in this work. The book also reveals past and present conflicts between the Russians and the others as well as the regime's efforts to promote Russian influence which has had, as Dina Spechler correctly says, only limited effect (p. 16). However, the author of the tantalizingly short essay on the Russians minimizes the Russian achievement. The dynamics of russification which really remain unexamined in this book should be analyzed in centennial perspective—say, since the Imperial census of 1897. Such an analysis would show unparalleled Russian nationality expansion territorially, linguistically and otherwise. The perspective of the imperial past puts the current interaction and future conflict between Russian and non-Russian nationalities in a more realistic framework.

Fortunately for the "handbook" aspects of the work, the study was not designed solely to prove the thesis that modernization does not denationalize nationalities. The collection, on the other hand, does not offer randomly written essays on individual nationalities either. Fifteen authors have followed a disciplined comparative design for surveying 17 nationalities, their historical background, indices of their modernization and current nationalist tendencies. This description then is completed by a discussion of Western and Soviet evaluation of various nationalisms in their relationship to the process of modernizing development under Russian leadership. Thus, the book does not merely register encyclopedic data; it has theoretical underpinnings that make this nationality study academically, and politically relevant to recent Soviet development. The examined nationalities, furthermore, are not treated as "minorities" (a misnomer for the Soviet situation; indeed, the word "minority" appears only twice in this book), but as self-contained entities that display individual historical profiles. This approach, facilitated no doubt by a really catholic use of sources, vastly advances our understanding of the Soviet state as a multi-national system that is run as "a nationally homogeneous state" while in effect it is an empire (p. 4).

Criteria for the choice of the 17 "major" nationalities apparently included neither their size nor the degree of their modernization. Fifteen nationalities were chosen because they are titular to the 15 constituent republics of the Soviet Union. Two additional ones, the Jews and the Tartars, are surveyed because of their significance and topicality (p. 354).

The book quite obviously could not include many more groups, but, unfortunately, a student of nationality development must regret the exclusion of smaller but geographically compact nationalities as well as various diasporas that currently provide both some demographic gains for the Russians as well as the additional 13 million people who list themselves as non-Russians but declare Russian as their "native" tongue. This kind of russification has an enormous bearing on the future of national relations in the Soviet Union, and a study of at least one such feeder group is highly necessary in the work of this nature.

The surveyed nationalities, furthermore, are not grouped together according to the scale of modernization indices, but on a more simple and more realistic basis of their ethnic, linguistic or geographic affinity. However, in the appendix the editors have provided tables with various indices of modernization, although, curiously, the appendix includes neither a table for all Soviet nationalities which would seem to be required for the total picture nor republic maps to make it easier to identify and locate various cities and republic regions discussed. Questions, furthermore, must be raised about the tables, esp. the table on "political vitality" which combines in a methodologically unacceptable way figures on ethnic and linguistic identity with those of Communist party membership. The editors here did not distinguish between republic and all-Union percentages, between results in compacted areas and in diasporas. Since both the ethnic-linguistic as well as party participation percentages are different for each category, the book's computed index is not as representative as it should be.

Essays by individual authors are almost of uniformly high quality. In some cases—I will stay in the Slavic and Baltic area—certain details could have been improved by better consultation. For example, the editors did not dare to face the ticklish problem of transliteration from non-Slavic languages. Among other relatively numerous little deficiencies, the view that "Adam Mickiewicz is widely considered the greatest Lithuanian poet" (p. 125) is strange; it was accepted by some Lithuanians during the period of national awakening, but it has long been forgotten. Historically a bit more serious is a suggestion found in the essay on Belorussia that the history of provinces that today constitute Belorussia not only had much in common with medieval Lithuania but also sort of superseded the history of Lithuania itself (p. 50). In technical matters, further spotchecking indicated that the text sometimes alludes to references not listed in the key or not identified correctly (p. 452). In a work that is bound to be used as a reference such errors are inadmissible, especially because this book was prepared with adequate financial support and edited by an entire *troika*. Nonetheless, this handbook belongs in every library and on the desk of every student of nationality and nation building.

V. STANLEY VARDYS

University of Oklahoma

Influence in Parliament: Canada. By Allan Kornberg and William Mishler. (Durham, N.C.: Duke University Press, 1976. Pp. xxi + 403. $14.75.)

This significant study of Canada's 28th Parliament (1968–72) will be welcomed by students of Canadian politics as well as by those interested in the comparative study of legislative bodies. Drawing on detailed and carefully constructed interviews with 189 individual members, and on much previous research, Kornberg and Mishler have produced a sophisticated study of modern parliamentary politics. The authors attempt to get beyond the more traditional topics of legislative research such as recruitment, socialization, or representational roles, and focus instead on fundamental questions of the distribution of power and influence among participants in the legislative process. Believing with Mosca that parliaments, like societies, can be understood in terms of 'rulers' and 'ruled,' they proceed first to classify and then to analyze from this perspective the nature of influence in parliament.

The central purpose of the book is clearly stated: "to explain variations in the extent and character of individual participation in the formal and behind-the-scenes activities in which members of parliament engage; how these are affected by the backgrounds, the values, and the perspectives of members, and the formal positions they hold; and how these, in turn, are related to the amount of influence they enjoy" (p. 17). This formulation implies a model of the determinants of parliamentary influence–the testing of which forms the dominant theme of the book. Kornberg and Mishler specifically hypothesize that parliamentary influence is a function of position and level of participation, and that these are affected by a number of identifiable attitudinal and environmental qualities. Political attitudes and motivations are shown to be related in turn to socialization, recruitment, and background.

With this in mind, the authors set out to analyze the determinants of influence in the 28th Parliament. Beginning with a series of "vignettes" of individual members (some of whom keen students of Canadian parliamentary politics may recognize), they proceed systematically through an examination of the correlates of influence, participation, and position, and then to analysis of the interrelationships and linkages among these several elements of the hypothesized model. In a key chapter of the book ("Models of Influence"), the hypothesized model is examined critically in light of the data, and revisions appropriate to the findings are introduced and tested. Differences between the pathways to influence for members of the different parties are also noted, and the relative weights of factors determining influence are reexamined within party groups.

Although the central purpose of the book is the formulation, testing, revision, and validation of a proposed model of parliamentary influence, the authors also present valuable data on the evolution of the parliamentary process in Canada, the growing importance of committees, the inner workings of the House of Commons, and the nature of parliamentary activity and participation. This work is likely therefore to become an important source book for students of the Canadian parliamentary process as well as a model for analysis of other legislative systems.

As is to be expected with a study which is attempting to break new ground, we can advance a number of criticisms, some of which are anticipated by the authors in the course of their analysis. The central measures of influence are reputational, raising issues familiar to analysis of power and influence in other settings. Reliance on a reputational measure of influence becomes a particularly critical matter in assessing the relationship between influence and position, as leaders are likely to acquire a reputation for being influential because of their leadership positions. Also, there is room for considerable debate regarding the direction of some of the casual arrows in the authors' model, a point much discussed as the proposed model of parliamentary influence is developed and tested. But the methodological debate which follows and in which the authors vigorously participate, is highly constructive, likely to contribute to the advancement of research and analysis in the comparative study of legislatures.

A typology of parliamentary activity developed by the authors as a result of their research is also likely to be of value to analysts of other legislative systems, for it leads to projections regarding the future course of development of parliamentary bodies. But some of these observations are necessarily speculative. It is an arguable proposition for instance, particularly in Canada, that the importance of legislative committees will continue to grow, or that "influence" in a parliamentary system can genuinely derive over time from committee participation alone.

In all however, Kornberg and Mishler have made an impressive contribution to the literature on legislative systems, certain to be widely recognized and cited among students of Canadian politics. It is to be hoped that its reading will not be confined to area specialists, but that the authors' contribution to comparative research on legislative systems will be equally noted.

LAWRENCE LEDUC

University of Windsor

Nonconformity in Modern British Politics. By Stephen Koss. (Hamden, Conn.: Shoestring Press, 1975. Pp. 272. $14.50.)

This is primarily a historian's book, important in that respect because it uses for the first time new evidence for a specific phase or facet of British politics, drawn from the private papers of participants and from the journalism of the time. The episode is that of nonconformity operating "as a viable and fairly homogeneous tactical unit in modern British politics" (p. 13). Stephen Koss defines it, for his own purposes, as extending from the agitation against W. E. Forster's Education Act of 1870 to Lloyd George's last challenge in politics, the Council of Action, which totally failed to influence events during the prologue to World War II. During this period the set piece is the story of A. J. Balfour's Education Act of 1902 (which turned out to be the foundation of the public system of education in England, as it still exists); the attempts of the Nonconformists to organize pressure against it, largely through the Liberal Party; and the collapse of their campaign, largely because after 1910 the Liberals needed Irish votes in the House of Commons more than they needed the Nonconformists.

This book is a scrupulous piece of historical scholarship, made readable by effective quotations and occasional shafts of wit. What is there in it for political scientists? The biggest issue is that suggested by Koss' quotation (p. 225) from the well-known book by David Butler and Donald Stokes: "as new generations entered the electorate the parties arrayed against the Conservatives drew their support less from religious beliefs than from class interests" (*Political Change in Britain* [New York, 1971], pp. 101–06). A rival hypothesis might be that English politics has been divided since the Civil War of the seventeenth century, and even earlier, by a temperamental and ideological division between "establishment" and "radicals," and that at times the radicals have been able to exercise power temporarily by alliance with class interests (those of the industrial middle class in the days of the Corn Law League, those of the working class in the rise of trade unions, consumers cooperatives, and the Labour Party), but that the dissenters from their very nature cannot retain power for long. They rose to power briefly in the 1640s, the 1840s, in the Liberal government of 1902 and the Labour government of 1945. But since 1950 they have reverted to their traditional role of harassing the establishment, as constituted by both the main parties, and by salting the routines of politics and administration with "enthusiasm," dogma and ideology. At present, the rhetoric appeals not to the Bible but to humanity; yet it has a well-recognized and continuous line of descent from the sectarian rhetoric of the 1630s and 1640s.

These grand hypotheses are themselves articles of faith rather than of proof; Koss certainly does not substantiate the Butler and Stokes hypothesis with which he ends the book. Indeed, some of his most interesting evidence relates to the continuity of family tradition (the Chamberlains and the Foots, for instance) in spite of changing party alignment. Perhaps one must somehow contrive to treat the rival hypotheses as complementary rather than opposed.

There is much else in the book for students of British politics, and also for those interested in the comparative study of churches and sects. There is much that is new about Lloyd George as political manipulator; about the relation between politicians and the press, particularly the sectarian press; about the practical limits of pressure by groups, however well represented, which have no effective control over votes in the House of Commons; about the movement of individuals between parties in the formative period from 1918 to 1939. And there is much to compare and contrast with the roles of establishment and dissent in other Protestant countries.

W. J. M. MACKENZIE

University of Glasgow

Comparative Public Policy: Issues, Theories, and Methods. Edited by Craig Liske, William Loehr and John McCamant. (New York: Halsted Press, 1975. Pp. 300. $20.00.)

This misleadingly titled book is a collection of 11 mostly empirical studies plus a concluding chapter on some aspects of the public policy of this country and others. There is little in common among the 11 pieces save their authors' conviction that science advances by confronting theory with data. The title is misleading in several ways. It is by no means a survey of the issues, theories and methods involved in doing public policy analysis. The studies—of budget allocation between domestic programs and defense in Brazil, political development in Sweden, rural electrification in the U.S. and India—are mostly examples of using crude theory and rather primitive statistical techniques to estimate some relationships. Interesting examples of these relationships are the findings by Margaret Daly Hayes (in "Policy Consequences of Military Participation in Politics: An Analysis of Tradeoffs in Brazilian Federal Expenditures") that in Brazil defense spending is not at the expense of government investment in economic and social development; by Barry S. Rundquist and John A. Ferejohn (in "Observation on a Distributive Theory of Policy-Making: Two American Expenditure Programs Compared") that Congressional districts receiving disproportionate U.S. Army Corps of Engineers funds are not overrepresented in the Public Works Committee and the Appropriations Subcommittee on Public Works of the House of Representatives; and by Farid Abolfathi and Tong-Whan Park (in "Military Spending in the Third World: The Interaction of Domestic and International Forces") that defense expenditures in Third World countries are more easily and precisely predicted than are those of the most affluent and powerful nations. At a number of places one's expectations of sophistication are disappointed. Regression analysis is the most commonly used statistical device. Problems of multicollinearity are mentioned, then ignored; also ignored is the notion that individual relationships typically ought to be estimated in the context of a system of equations. The discussion of estimating the demand for and incidence of public goods by B. Guy Peters and Timothy M. Hennessey (in "Political Development and Public Policy in Sweden: 1865–1967") is evidently unilluminated by the recent contributions of Henry Aaron, Martin McGuire, and others. Terms and procedures with precise meanings are sometimes used imprecisely: games by Hayes, cost-benefit analysis by Susan G. Hadden (in "Two-Phase Welfare Policies: Rural Electrification in the United States and India").

The title is misleading in another sense. One would expect some emphasis on how to affect policy; rather this book gives preponderant attention to predicting policy. I have three observations. One is that prediction is not as useful as control. My bias is that policy analysis ought to have to do with how to affect policy. Second, several of the authors, not disciplined by an effort to identify policy manipulable variables, seem to have built a kind of anthropomorphism into their studies of policy making. For Hayes the central research question becomes: "Does military spending have negative consequences for economic and social investment?" Her answer "is a qualified 'no'." A somewhat similar formulation was used by Barry Ames and Ed Goff (in "Education and Defense Expenditures in Latin America: 1948–1968"). It is as though defense expenditures themselves are the *deciders* of the level of domestic expenditure. Besides being anthropomorphic, that formulation diverts attention from the obvious fact that the opportunity cost of defense expenditures is not zero. They came from somewhere, they could be spent elsewhere; the people who decide to spend money on defense could decide to spend it domestically. Third, if one is interested in affecting policy then "explaining" a large proportion of variance, finding a high multiple R^2, is not interesting in itself. The *cost* of achieving increments in a dependent variable by changing an independent variable is what is interesting. (See Glen G. Cain and Harold W. Watts, "Problems in Making Policy Inferences from the Coleman Report," *American Sociological Review*, 35 [April 1970], pp. 228–52.)

Bright spots in the book are the taxonomies of kinds of theories that are presented by Rundquist and Ferejohn, Abolfathi and Park, Peters and Hennessey, and Edmund P. Fowler and Robert L. Lineberry (in "Comparative Policy Analysis and the Problem of Reciprocal Causation"). The theory building efforts of Todd Sandler (in "The Economic Theory of Alliances: Realigned") and, especially, of R. Harrison Wagner (in "National Defense as a Collective Good") are to be applauded. It is significant and a sad commentary on the state of policy analysis that these theoretical articles are more subtle and insightful than are the braver empirical pieces. Maybe the field is advanced by sorting through poor data using ad hoc theoretical structures. Maybe not.

JOHN E. BRANDL

University of Minnesota

French Politics in Transition: The Years after De Gaulle. By Roy C. Macridis. (Cambridge, Mass.: Winthrop Publishers, 1975. Pp. 165. Price not given.)

In this informative and stimulating essay, Roy Macridis picks up the "decline of ideology" argument and concludes, in essence, that it applies to France, a country often viewed as one of the strongholds of ideological politics. He explains the relative stability and effectiveness of French government in the Gaullist era partially in terms of institutional reforms (especially the popularly-elected presidency), but primarily in terms of the impact of social and economic "modernization" upon political attitudes and behavior. Macridis perceives a "growing national and consensual political community" (p. 64), with elections dominated by two "catch-all formations" (p. 127), which have more in common than ever before "with regard to both political institutions and social values" (p. 93).

The new French politics is dominated by "a new bourgeoisie that spans the ranks of the electorate from some of the Communists and many of the Socialists to the Gaullists. It is the real majority" (p. 94). As the working class increasingly is assimilated into the existing social and political system, and as the citizenry at large comes "to yearn for what we call 'community control' " (p. 51), the traditional political style of confrontation and administrative fiat gives way to pragmatism and broadened participation.

Some of these trends have been noted earlier, for example by Harvey Waterman in *Political Change in Contemporary France* (1969). Yet Macridis adds deeper perspective to the analysis from a vantage point six years after De Gaulle's resignation. Writing for the student and general reader at least as much as for his colleagues, Macridis at times overstates the trend toward consensus, especially in his impressions of public opinion. Yet he usually proceeds to qualify his own *belles phrases*, e.g., by noting the possibility that with the Soviets supporting its sizable conservative wing, the Communist Party might once more turn against bourgeois democracy (pp. 127–28).

In the absence of longitudinal survey data, one might question whether interest in ideology at the mass level was ever significantly higher than it is now (p. 130), or whether the citizenry today is indeed impassioned with a new desire for political participation (pp. 51, 160). The evidence offered also seems insufficient to support the author's conclusion regarding the impact of social class on French politics. At p. 60 he writes: "To the extent to which 'class' was ever a significant factor for party identification and voting, it is losing the saliency and weight it had in the past." By the author's own account, the percentage of industrial workers voting for parties of the Left rose from less than 50 percent in 1965 to 64 percent in 1973 (p. 84) to 71 percent in 1974 (p. 113). Despite the alienation evident in the general strike of May, 1968, it is quite possible that French workers today feel themselves more integrated into French society than in the 1950s, as Macridis argues. Decline in the intensity of class antagonisms does not necessarily lessen class voting: witness the high levels of class voting in Sweden and Britain in the 1950s and 1960s compared with much lower levels in Italy and France. As Robert Alford has shown in *Party and Society* (Chicago: Rand McNally, 1963), class may emerge as the most important residual cleavage in a society of increasing consensus.

Most readers who are familiar with France of the 1930s, or even of the 1950s, will agree with Macridis that French parties today exhibit greater consensus than in the past with regard both to institutions and to issues like church schools and foreign policy. Some will feel that he underestimates the continuing importance of traditional cleavages like religion (church attendance remains the best predictor of voting preferences) and class. Others might wish for a more systematic formulation and testing of the "modernization" thesis. Nonetheless, this book is solid fare. It offers the best global account available of French politics after De Gaulle.

JOHN S. AMBLER

Rice University

Recasting Bourgeois Europe: Stabilization in France, Germany and Italy in the Decade After World War I. By Charles S. Maier. (Princeton, N.J.: Princeton University Press, 1975. Pp. 650. $27.50, cloth; $11.50, paper.)

Charles Maier has written a weighty book: long, closely-argued, incredibly dense in parts, so thick with facts that on many pages the forest of his argument is lost in thickets of specifics. Brilliant cameos (too brief, but brevity is their virtue) liven the book and console one for its more plodding sections. Most of the time, Maier's evident talent for description and pithy generalization gives way to dogged accumulation of data. Yet the persistent reader who, after 594 pages, comes forth, like Dante on his Easter Sunday, "to look once more upon the stars," has not labored in vain.

Using the procedures familiar to political and economic historians, enriched by impressive scholarship, Maier has given us a comparative study of how Germany, Italy and France, emerging from the holocaust of war, set about stabilizing their public lives. Economies exhausted, social relations strained, political structures riven by new tensions as well as by old, late 1918 and early 1919 brought not relief but the prospect of a new social apocalypse: the near-collapse of the bourgeois order. How this was avoided and the order saved—or, rather, recast in a new corporate mold that would serve as pattern for decades to come—is the theme of Maier's work.

France, Germany and Italy each had peculiar problems. But in all, war and its aftermath had left similar legacies. The distinction between public and private economy had been eroded. Organized labor, increasingly committed to productivity and national welfare, had been integrated into a bargaining system in which the state became ever more involved. Political disunity among sections of the middle classes had been aggravated by economic problems. Such rifts would be further deepened by inflation, with its uneven impact on different groups, regardless of "class." Traditional alignments and antagonisms would thus be complicated, and very soon replaced, by new ones. On one side capital and labor, both organized as producers, on the other consumers and savers, largely unorganized, were cast as the chief losers in almost every conflict of the twenties.

The political ballet followed a choreography established before 1914, without much overt reference to the underlying realities that the war had wrought. But crises, international or internal, were only settled because in the crucial decade, 1914–1924, big business and big unions could resolve their differences through a price-wage spiral, with capital making the concessions needed to stave off political polarization, and labor, in the last analysis, endorsing conservative solutions to keep the parliamentary regime from collapse. By mid-decade, the Dawes reparation settlement, the Locarno accords and the Franco-German commercial agreements had cleared the international air, while different versions of corporatism had resolved the internal divisions besetting two of the three countries (the French resting on the laurels of victory and economic archaism). True, as Maier remarks, when there were settlements, the Right tended to get paid off in cash and the Left in principles, but even the latter seem to have proved more satisfying than the frustration of the unorganized losers. Nor should we underestimate the temporary gains of labor when, Maier tells us, by the later twenties, "proto-welfare-state compromises seemed feasible and imminent" (p. 515).

The small producers, shopkeepers, debt-ridden farmers, the old lower middle class of artisans and the new one of white-collar workers, all the *kleine menschen* at whose expense the struggles were resolved and who bore the brunt of resultant inflation, responded by becoming increasingly hostile to organized labor, to capitalism, and also to the liberal institutions that failed to defend them, and by looking for defenders elsewhere. The tacit reconciliation of the major and obvious adversaries thus laid the groundwork for fascism—that last infirmity of ignoble minds, especially when capitalism is on the blink.

All this appears a bit too cut and dried. National parallels seem strained, often unconvincing. And the comparative approach, including an ill-fitting France and excluding a more relevant Britain, seems more a salute to novelty than a productive method. Indeed, the most serious criticism to be levied against Maier's argument is that it fits Germany best, France least, and Italy only with reservations. But a book need not be readable nor a thesis indisputable, to prove useful and suggestive. Maier's pages teem with fascinating bits of information. His use of economic archives blazes a trail others will follow. His argument offers a new view of familiar events, a new way of looking at them and of making them make sense.

We have been so fascinated by the thirties that earlier years had slipped out of sight: an awkward interlude, crisis-buffeted and difficult to define, between one war and the preliminaries of another. After Maier, the early 1920s need no longer rattle uncomfortably between epilogue and prelude. They have acquired significance of their own as a formative part of the twentieth century; and, as the author concludes, a conservative achievement.

EUGEN WEBER

University of California, Los Angeles

The Future of Political Parties. Edited by Louis Maisel and Paul M. Sacks. Sage Electoral Studies Yearbook, Volume I. (Beverly Hills, Calif.: Sage Publications, 1975. Pp. 279. $12.50, cloth; $7.50, paper.)

This anthology is the initial volume in a series designed, in the editors' words, to "transcend both territorial and disciplinary geography" (p. 8) and to diminish the communications gap between scholars and practitioners, i.e., "politicians; political consultants; speech writers; advertising specialists; radio, television, and print journalists; pollsters" (p. 7).

The contributions can be compared along any one of several substantive or methodological dimensions, but for purposes of this review the essays will be discussed in the following categories. The first set utilizes SRC data and analyzes mass partisan stability and change across the American electorate. Finch makes use of cohort analysis to examine the eroding effects that "physical" changes among American voters (such as age, region, class and race) have had on the cleavages which structured party identification in the New Deal party system. He finds that the 1964 election ended the New Deal alignment but appears only to have replaced it with an evolving system of partisan decomposition and fragmentation. Trilling utilizes the psychological concept of party "image" to test the hypothesis that people who change their party identification tend to develop a favorable attitude toward their new party prior to the conversion. He finds that party image interacts with party identification either by "maintaining past identification with present image, or by altering past identification to make present identification consistent with present image" (p. 89). Another interesting finding is that New Deal economic-class-based perceptions of both parties comprise a declining but still prevalent issue yardstick for voter preferences, i.e., "the Democrats are good for the common man." The article by John E. Jackson examines party choice decisions by estimating the importance of current issue evaluations of voters, voter perceptions of party issue stands, and voters' previous party affiliations. Econometric models are developed in estimating equation form and tested with SRC 1956–1960 panel data. The findings and their implications cannot be fully reported in this review but the author shows how affiliations change in response to changes in issue evaluations, how measures of party identification are simple assessments of the historical effects of issue and perceptions of party positions, and how the stability of party affiliation increases with age. In discussing short- and long-run fluctuations in party support, Jackson concludes that only widespread and systematic changes in voters' positions on the issues will cause party realignments. Jackson's methodological rigor and clear writing style is commendable, but he might have discussed more fully the impact that growing numbers of non-leaning independents will have on American parties and elections.

In the non-American category, the volume presents a study of the Australian party system and a longitudinal comparison of the Norwegian and British Labor parties. David Kemp utilizes a "sociological" approach to examine the evolution of Australian society toward a "postindustrial" phase and measures the effects of this sociopolitical change on the bases of class support for the Australian parties. Primarily using Gallup data, Kemp finds a weakening in the class bases of party support from 1946 through 1972 caused by defections from the Labor party by blue-collar voters and similar disenchantment with the Liberal and Country parties among upper white-collar voters. The author offers several interesting explanations of this trend and emphasizes the development of the Australian party as an organized expression of third party discontent among the technocratic elite. Kemp's article is methodologically sophisticated without being poorly written (a rare combination) and rates as one of the more valuable original contributions in the volume. Wellhoffer et al. presents a structural analysis of the relationship between organizational development and leadership change in the Labor parties of Norway and Great Britain. The authors present some data gathered in an earlier study that confirms Selznick's stages of development for both parties.

The development of third-party support in New York and the effects of internal reform on party activism in Maine are presented as case studies. John Gargan uses aggregate voting data in selected New York elections to examine patterns of Conservative party support. The author presents a host of correlations with demographic and "cleavage issue" (on local taxation and expenditures) data and finds greatest support for the Conservative party in the New York City region and in the greater Syracuse area. Gargan concludes that the Conservative party is not a transitory phenomenon. Louis Maisel studies the effects of the McGovern-Fraser Commission reforms on participation at several levels of party organization in Maine in 1972. As the author accurately points out, "obviously the importance of these findings does not follow directly from the specific data generated by this research." I can add little more to such a negative evaluation except to say that Maisel's analysis of the characteristics of Democratic leaders has some merit but could have been better organized and should have utilized some basic statistical measures.

Walter Dean Burnham's essay "American Politics in the 1970's: Beyond Party" quite predictably is the best essay in the anthology. Burnham presents a powerful argument that the American electorate is in the midst of a critical realignment unlike any other previously seen in American electoral history. This phenomenon is characterized by the destruction of partisan linkages between voters and elected officials and the rapid dissolution of the party as a source of guidance for many citizens. Skilfully buttressing his arguments with data from diverse sources, Burnham reviews the previous findings on critical realignments and continues to make a persuasive case for his thesis (presented earlier in *Critical Elections and the Mainsprings of American Politics*) that party decomposition is the most important factor in understanding contemporary (1900–1972)

American political participation. This article is the only contribution that really attempts to grapple with the future of political parties and Burnham is not very reassuring especially to those wedded to a traditional view of the invulnerability of American two-party politics. Burnham's essay should be required reading for all new graduate students and helps assure a rating of qualified distinction for the anthology.

The major weakness that I find in *The Future of Political Parties* is simply that the editors tended to ignore the title in selecting the essays. With the exception of Burnham's essay and several concluding paragraphs in some of the other articles, there is no systematic attempt to develop futuristic themes of political parties. The editors included too many empirical studies of electoral data that provided little predictive power about the future of parties. Several theoretical essays that emphasized scenarios of "partisan futures" would have provided welcome relief from the incessant pounding of the empirical analyses. Also, I felt that more emphasis on party elites could have balanced the emphasis on mass partisan voting patterns. Finally, the editors did not achieve their objective of offering contributions from diverse disciplines and creating a dialogue between scholars and practitioners. Nevertheless, the level of scholarship is generally high and the editors and Sage are to be commended for discovering the important work of some younger emerging scholars. It is refreshing to find a volume that is not overloaded with the mediocre work of established scholars included solely for its potential commercial appeal.

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Politics and Class Formation in Uganda. By Mahmood Mamdani. (New York and London: Monthly Review Press, 1976. Pp. 339. $16.50.)

Class Struggles in Tanzania. By Issa G. Shivji. (New York and London: Monthly Review Press, 1976. Pp. 182. $9.50.)

These two books belong to the new genre of political economy, being Marxist or neo-Marxist in methodology and in style of analysis. But unlike the authors of much of the literature on political economy emerging from the Third World in recent times, Shivji and Mamdani are not primarily interested in dependency and imperialism. They have gone back to the older Marxist preoccupations of *internal* class analysis and the dynamics of capitalism.

Since the countries the authors are looking at (Tanzania and Uganda) have both been colonies, issues of dependency are indeed often fundamental. But the focus in these two books is on social forces at work within African countries, rather than on world capitalist forces working themselves out in an African setting. Shivji and Mamdani are thus refreshingly different from the cosmic dimensions of Samir Amin's work on West Africa or Colin Ley's work on Kenya.

According to Mamdani, direct colonial rule had indeed "reduced all contradictions between classes *within* the colony to secondary significance" (p. 221). But after independence this "muted" domestic class struggle broke out in the open—"the underdeveloped economy is now supervised by an indigenous ruling class" (p. 313). Shivji and Mamdani allow Africa to produce its own "villians" instead of reducing all Africans into puppets of international capitalism, as other radical analysts have tended to do.

Mamdani's book is more scholarly in a "bourgeois" sense that Shivji's, having more documentation and greater academic rigor. (This is not surprising since Mamdani's book is based on a doctoral dissertation for that most bourgeois of all the universities of the western hemisphere, Harvard.)

On the other hand, Shivji's analysis has already had more of an impact in Africa. Certainly in East Africa Shivji generated considerable debate with his earlier paper, *The Silent Class Struggle in Tanzania.* A large proportion of the 1973 annual conference on East African social scientists was taken up with debating Shivji's thesis about the nature of class structures in "socialist" Tanzania and the dynamics of continuing class struggle. This book is partly an elaboration of Shivji's earlier arguments, and partly a response to the criticism of his fellow leftists. For the time being, Shivji's work is probably still the most important critique from the left of Julius Nyerere's policies and strategies.

Both books are naive in their failure to understand how western *culture* (and not merely western capitalism) changed the system of stratification in both Tanzania and Uganda. Shivji discusses the "bureaucratic bourgeoisie" of Tanzania, but fails to examine why the top sector of that bourgeoisie consisted disproportionately of westernized or semi-westernized

Tanzanians in a country where the majority were either "illiterate" or very distant from western culture.

Perhaps partly because they are Marxists, neither Shivji nor Mamdani see the cultural dimension in class formation in twentieth-century Africa. Under the colonial impact, western education acquired considerable salience in the class struggle in Africa. Instead of status based on, say, age or property, there emerged status based on literacy itself. Instead of classes emerging from the question "who owns what?" class formation in colonial Uganda and Tanganyika began to respond to the question "who knows what?" The knowledge was often merely literary, but the colonial impact refused to conform to either Marxist presuppositions of economic primacy or African traditional presuppositions of primordial congruence. The very process of acquiring aspects of the imperial culture came to open the doors first of influence and later of affluence itself. By the last two decades of colonial rule, at least twice as many people became economically better off by first acquiring a western education, as acquired access to western education because of economic advantage. That is, more people were getting richer by being westernized than were getting westernized by being rich. Neither Shivji nor Mamdani show the slightest evidence of sensitivity to the cultural factor in colonial class formation. Have they not read their Frantz Fanon? Mamdani seems aware (p. 221) of Fanon's concept of the "intermediate class" in a colonial state, but he seems unaware that the intermediary class of natives was groomed through a new *culture*, rather than directly through capitalist apprenticeship.

But even within the realm of economic criteria of class Mamdani is either simplistic or rash. He describes the working class in Uganda soon after World War II as a "propertyless class" (see p. 189, for example). This is a highly Eurocentric view of the working class. In Uganda in the 1940s what constituted owning or not owning property? Many workers were also part-time peasants with some land of their own. On the other hand, some of the most powerful figures in the land had their power measured more in terms of how much *communal* land they *controlled* than in terms of how much private land they themselves owned. One of the dangers of using European categories of social analysis lies precisely in the kind of conceptual misapplications and analytical distortions of which Mamdani is often guilty.

As for the suggestion that Uganda after World War II first experienced the rise of a united proletariat, and then the emergence of a new petty bourgeoisie, it is difficult to view this as a serious interpretation of the country's history. It sounds more like an exercise in terminological gymnastics. We get no nearer to reality when Mamdani assures us that "the eclipse of working class politics and the rise of petty bourgeois politics were in fact two sides of the same coin" (p. 192). More verbal games.

Shivji is more interesting when he attempts to understand the relationship between workers and peasants in African conditions. He takes the "labor aristocracy" theorists to task for writing off the African proletariat too rashly in favor of the African peasantry. It is true that African workers have benefited more in *distributive* terms from the colonial situation than the peasants. But Shivji reminds us that "the revolutionary potential of the African working class is not based on their income but on the role they play in the process of production" (p. 116). Shivji is inclined to put his bet on the working class as the probable revolutionary vanguard—echoing at times some of Cabral's reasoning on the same subject (Shivji, p. 120).

Both Shivji and Mamdani are weak in their understanding of the dynamics of the *political process*. I suppose this arises partly out of the reductionist tendency inherent in class analysis—reducing all political behavior to relations between social classes or within a ruling class. Since Mamdani is ostensibly a political scientist, he sounds particularly incredible when he states: "Politics in early colonial Uganda was the exclusive preserve of the pre-colonial ruling classes" (p. 171). What sense of "politics" could this possibly be? Surely these pre-colonial ruling classes had *political* interaction with other Ugandan social classes. Surely they had interaction of a political nature with the colonial administrators as well. How then could politics be their "exclusive preserve"? The political reductionism in Mamdani's analysis is breathtaking. Everything has to be reduced to class proportions. The fact that thousands of Ugandans have died for ethnic or religious reasons, that many have prospered for similar reasons, counts for very little to such a school of analysis. In some curious manner Mahmood Mamdani is both a vulgar Marxist and a brilliant innovator. He is a vulgar Marxist because of his tendency to carry economic determinism to its extremities, and to reduce all primordial loyalties and emotions to class interests. He is a brilliant innovator because he is groping for a way to make a European baggage of analytical tools (Marxism) relevant to the understanding of non-European peoples. Ugandans are paraded before us as either "Kulaks" or "petty bourgeoisie," but they are rarely identified as

Baganda versus Banyoro, Bantu versus Nilotes, southerners versus northerners, Acholi versus Lugbara, or Nubi versus the rest! One seldom learns from this book about the dynamics of dying for one's religion in Uganda, or the force of living for one's tribe.

Both Shivji and Mamdani have depth of insight, but not breadth of vision. They are brilliant within the narrow confines of class analysis—but they do not see much beyond class interests. Like Jeremy Bentham, they see far in a straight line—but like Bentham each is analytically one-eyed. Alas!

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Youth and Politics in Japan. By Joseph A. Massey. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. xx + 233. $18.50.)

Youth and Politics in Japan is a report of a study of political socialization carried out in Japan in 1968 and 1969. The study draws on the data gathered by means of questionnaires administered to a sample of 942 Japanese junior and senior high school students and their parents. The study examines such topics as youth's attitudes toward the prime minister and other institutions, youth's perceptions of and attitudes toward democracy and peace, the inheritance and growth of partisanship, the development of political ideology, the social and psychological influences on youth's political character, and the role of the family in political learning. All these topics are of course standard items in political socialization research today.

Youth and Politics in Japan is remarkably up-to-date in terms of its bibliographic survey and statistical techniques. Joseph Massey has carried out a thorough review of all the relevant literature on political socialization not only in Japan but also in the United States and elsewhere, and for this reason this study may be considered comparative. Also Massey's mastery of statistical techniques is excellent. The study presents a large variety of informative bivariate statistics as well as results of such advanced statistical methods as factor analysis and multivatiate causal path analysis.

The manner in which Massey interprets his data is similarly outstanding. He carefully reevaluates the implications in the Japanese context of the Easton-Dennis thesis linking attitudes toward the president and the stability of the American political system. With respect to the role of the family in political socialization, Massey again reassesses and revises the prevailing impressionistic view in Japan that it may be minimal.

In addition, *Youth and Politics in Japan* is well-written and pleasantly readable. Unlike most books in political behavior which are often marred by esoteric terminology and excessively involved modes of description, Massey's book is eminently straightforward in its data presentation, data interpretation, and theoretical discussion, and his polished style certainly adds to the intrinsic scientific value of his socialization research.

Yet it would be less than frank to say that this is an unconditionally successful study. Let me illustrate a few minor weaknesses of this book. First, it gives a misleading impression on the representativeness of its sample (p. 196). Aside from the fact that its primary sampling units were not probabilistically drawn, the sample was not exactly representative of Japanese youth of junior and senior high school age; only approximately half of the Japanese youth of senior high school age attend school.

Second, the quality of responses obtained by Massey's survey tends to be more elitist than that obtained by a typical cross-section survey in Japan. Yet this characteristic does not seem to be adequately taken into account in interpreting the data employed in this study. For example, according to Massey's survey, 51 percent of the student sample and 60 percent of the parent sample are highly interested in politics, and 60 percent of the student sample and 73 percent of the parent sample often read articles about politics in the newspaper (p. 136). But according to a 1967 Japanese cross-section survey in which I was involved, only 10.4 percent and 38.1 percent of the sample are interested or a little interested in politics, and merely 24.4 percent of the sample follow news of politics in the newspaper. The unusually high rates in the Massey study may or may not be related to the fact that the study used self-administered questionnaires rather than face-to-face verbal interviews and to the fact that its field work was carried out through the Japanese school system where the emphasis on the activist model of democracy is markedly stronger than elsewhere.

Third, *Youth and Politics in Japan* relies on *gamma* as its principle bivariate correlation coefficient, which generally yields a considerably larger numerical value for a given relationship than other bivariate correlation coefficients, e.g., Kendall's *tau* and Pearson's *r*. Although *gamma* is a perfectly legitimate statistical measure and should certainly be used in social research, it would have been less misleading had the study more explicitly pointed out *gamma*'s unique characteristics for the benefit of those readers unfamiliar with statistics.

Finally, there are a few theoretical ambiguities in *Youth and Politics in Japan;* examples are the link between the benevolent leader and the system's stability and the role of urbanization in political development. Although Massey apparently prefers to support the Easton-Dennis thesis, isn't it intellectually more straightforward to reject it altogether in view of his clearly negative results presented in chapter 2 of his book? Is it theoretically defensible to cite Japan's historical uniqueness and continue to support the Easton-Dennis thesis as Massey does? Similarly, isn't it rather sweeping to single out urbanization as a very powerful source of modernization (pp. 12, 127, 186–88, 190) when Massey's own analysis yields fairly mixed results (pp. 134, 137–38, 150, 178)? Can't we simply argue that the role of urbanization in political development is either inconsistent or moderate?

It should, however, be emphasized that these weaknesses are relatively minor vis-à-vis the fundamentally outstanding quality of this study. We must remember that no humanly conducted study is absolutely perfect. Virtually any study contains some weaknesses, and what varies from critic to critic is whether one overlooks or identifies these weaknesses or sometimes exaggerates them out of proportions. As I have said, *Youth and Politics in Japan* is an extremely sophisticated behavioral study of Japanese politics, and in technical terms it may even be favorably compared to some of the advanced behavioral studies of American politics. There is little question that Massey's study represents one of the most important contributions toward the general trend of the increasing use of the modern behavioral science techniques in the rather "traditionally-oriented" contemporary area studies.

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Policymaking in the German Federal Bureaucracy. By Renate Mayntz and Fritz W. Scharpf. (New York: Elsevier, 1975. Pp. 184. $12.50.)

This volume by two German social scientists who build on extensive experience in administrative and organizational research constitutes a welcome and highly readable addition to the literature on contemporary European bureaucracies. For students of West German politics it continues a trend which is rapidly correcting the previous imbalance between studies of parties and interest groups, and those of bureaucracies and administration. British writers like Johnson and Dyson, as well as American ones like Armstrong and Putnam have also contributed to making the German bureaucracy more comprehensible for English-reading students. The consequence may not trigger a scholarly gold rush towards the *siebengebirge,* the Rhenish hills where many of the Bonn bureaucrats reside. But it should engender a much better appreciation of the crucial respects in which the West German bureaucracy differs from the better-known British and French ones.

What makes this volume particularly accessible to foreign readers is that it employs generalized descriptions of structure and process, with a circumscribed analysis focused on identifying the ways in which the system meets or fails to meet the prerequisites of active policy making. The scope is comprehensive for the federal level, but kept mercifully brief. The more familiar materials, such as those bearing on civil service structure, are presented in chapters of some dozen pages; and even where more details are provided to illuminate less familiar ground, length does not exceed two dozen pages. The manuscript is uncluttered by tables, complicated specific references, or too many lengthy footnotes, or repeated reference to the structural-functional constructs which constitutes the authors' loose framework.

In attempting to explain why the bureaucracy has been so difficult to direct from above, the authors tend to emphasize that the system is much more decentralized than many credit, allowing heterogeneous interests to maintain institutional veto positions invulnerable to executive direction. They trace the familiar picture of a chancellorship which "does not provide the unifying potential of the American presidency," and of ministers who resist the will of potential cabinet majorities. Within the departments, ministers and state secretaries do not fully and systematically control policy making either mainly because they lack substantial staff services, as does the next-lower echelon, that of the divisions. Because there is little institutionalized capacity for policy initiation elsewhere, responsibility for developing well-founded program proposals remains largely with the 1600 sections. These dynamic nuclear units, often made up of just 3 to 5 officials and sometimes without a full-time secretary, are portrayed with particular lucidity. The sections in turn are linked in a government-wide vertical matrix structure which handles routine problems efficiently, partly because it is insulated from outside contacts. (Thus outsiders are usually refused copies of ministries' tables of organization listing the section heads and their telephone numbers.)

In a very enlightening overview chapter the authors elucidate how these and other structural constraints have kept the bureaucracy from meeting the preconditions for active policy making. The two-stage budgetary process is found over-responsive to "bottom-up spending initiatives," and specialization within both the Finance Ministry and the legislative committees inhibits attempts to establish spending priorities across ministries. Especially for those ministries whose programs are implemented by other Federal or Land Agencies, feedback is a scarce commodity for which they are excessively reliant on organized interest groups with direct stakes in the program. Among the weaknesses identified under the rubric of "conflict" management is that the bureaucracy is not well-suited to deliberately mobilizing popular support, and especially "anticipatory mobilization of support for controversial decisions yet to be made." Here the authors call for PR expert in "impression management," who can advise governments as to how to persuade people about "what they should want and can legitimately expect to get" (p. 26).

Objections raised by this part of the analysis relate only in part to the more explicitly Machiavellian style. The problem is that unlike the Italian princes, Chancellor Brandt led a mass party, which certainly tried its hand at "impression management." Was its record so devastating that the author could seriously propose transferring traditional party functions to the bureaucracy? This example exemplifies a weakness which is a byproduct of the author's otherwise laudatory attempt to keep their analysis limited and abstract. Though the setting is very much identifiable as that of the Brandt era, explicit reference to particular policy reform initiatives of the SPD-FDP government are few. But insofar as the party component is largely excluded, the analysis remains incomplete. Certainly the SPD tried to insert various priority-shaping instruments into the bureaucracy. This volume could have added more to our understanding of why these attempts to bring party and bureaucratic efforts into tandem were so unsuccessful.

The first Brandt government did initiate a bold attempt to remedy some of the organizational shortcomings endemic in the Bonn bureaucracy by the creation of a planning machinery headquartered in the chancellery. Was it only space constraints that limited the authors to a bare one-page thumbnail sketch of how and why this attempt failed? The almost total lack of discussion of the Laender bureaucracies, which of course administer much of the federal legislation, means that a huge constraint syndrome is left unassessed. It also leaves the reader unconvinced that the authors could substantiate their claim (p. 171) that the institutional barriers to the developing political consensus are greater in the Federal Republic than in the U.S. It seems to me that there are more conflicts of interest in the U.S. resulting in decision-making deadlocks. It is at the periphery of their analysis, when relating the bureaucracy to other institutions and attempting cross-national comparisons, that the authors provide ground for criticism. But my overall evaluation is still very positive; although it contains an excessive number of typographical errors and lacks an index, I recommend this book to both undergraduate and graduate students because of its readability.

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Soldiers and Kinsmen in Uganda: The Making of a Military Ethnocracy. By Ali A. Mazrui. (Beverly Hills: Sage, 1975. Pp. 325. $15.00.)

Ali Mazrui's books on Africa are both impressive and elusive. They exhibit a solid knowledge of the continent's politics, a flair for generalization and an almost uncanny capacity to manipulate the English language. At the same time, one receives the consistent impression that Mazrui often relies upon a clever turn of phrase as a substitute for analysis; that his sweeping generalizations call out for more systematically detailed evidence and that greater use of relevant theoretical literature would help further sharpen the analysis. Perhaps more disappointing, he avoids basic or controversial questions while spending a substantial amount of time and space to restate already well-documented material.

Soldiers and Kinsmen in Uganda is, regrettably, no exception to this general evaluation. Assembled, basically, as a compilation and re-editing of previously published articles, this volume exhibits both the qualities and the faults of much of Mazrui's earlier work on the subject. It is knowledgeable, indeed occasionally insightful, about Uganda's contemporary political history, masterfully written (though a more careful editing to eliminate repetition would have been useful), and far-reaching in its interests and comparisons. It is also, in the end, both conceptually unsatisfying and disappointing in its failure to bring to light new factual materials.

The basic theme of *Soldiers and Kinsmen* concerns what Mazrui regards as the emergence of "ethnocracy" in Uganda. Mazrui contends that General Amin has created in Uganda a polity based on authentic African ethnicity and culture. Shorn of verbal embellishment, Mazrui's argument reduces to the proposition that the Ugandan military regime is a closer reflection of the deepest cultural roots and attitudes of the vast majority of the African people than any other African regime to date, military or civilian. Mazrui believes strongly that the sort of political leadership represented by deposed President Milton Obote—western-educated, cosmopolitan, ideologically sophisticated and equally at home in either English or a vernacular language—is, in reality, fundamentally alienated by these very characteristics from the population over which it presides. General Amin—of peasant stock, barely educated, rough and ready in personal manner and untutored in western political protocol—seems, to Mazrui, far closer to the innermost aspirations of the African nationalists than the more foppish generation of African nationalists who preceded him.

Mazrui's conception of Amin's Ugandan ethnocracy, then, is that it is purely and authentically African as no polity adulterated by an admixture of western practices and institutions could ever be. This makes it difficult for westerners to appreciate the basis of Amin's Ugandan popularity which, for Mazrui, amounts to a form of charisma based on his personal embodiment of the visceral values of sexual and military prowess. Amin is *macho* and, in Mazrui's view, this makes it possible for Ugandans to identify with him more than was ever possible with the generation of leaders educated at Oxford or Cambridge. At one point, Mazrui mocks African radicals whose militant antiwestern feelings are based, in his view, on the importation of an alien western ideology into Africa. By implication, Amin's brutal treatment of Asians is far closer to the political passions of the African people than the Marxist rhetoric of a westernized intelligentsia.

There are serious shortcomings in Mazrui's presentation of this position. Despite the fact that the book deals with ethnicity in African society, Mazrui makes no use of the extensive theoretical literature on this topic. Students of cultural pluralism and of the complex theoretical interplay between M. G. Smith, Hilda and Leo Kuper, Pierre Van der Berghe and other scholars of this subject will be deeply disappointed to learn that Mazrui does not use his Ugandan materials to make a contribution to, or even to comment on, theories of ethnicity in Africa. This is especially surprising in view of the fact that Mazrui was one of the contributors to the Leo Kuper-M. G. Smith volume on ethnicity, *Pluralism in Africa.*

It is even more regrettable that Mazrui was unable to integrate the work of other leading scholars of ethnicity in Uganda, most particularly Nelson Kasfir, whose early articles, which appeared before the publication of *Soldiers and Kinsmen,* must certainly be considered a baseline for any subsequent treatment of this subject. *Soldiers and Kinsmen* is laced with references to the importance of ethnicity in Ugandan society; for example, to the importance of the rivalry between the Baganda and other tribes, to the salience of Amin's own Kakwa background, to the role of the Acholi and Langi peoples in the army and party system under the previous regime. But without a systematic treatment of ethnicity such as that given by Kasfir, and in the absence of an articulated theory of cultural pluralism, these bits of information are simply parts of a politico-historical narrative not integrated into a systematic theoretical analysis of ethnicity in Uganda politics.

Soldiers and Kinsmen also sidesteps certain of the more painful and controversial issues concerning military rule in Uganda. For example, Mazrui completely omits any treatment of the charges contained in the International Commission of Jurists' report on *Violations of Human Rights and the Rule of Law in Uganda.* This report alleges that, by conservative estimate, nearly 100,000 Africans were massacred, at the behest of the regime, during the first four years of military rule. Mazrui's own silence on this issue is inexplicable since he reprints, as an appendix, a 1973 memorandum by Milton Obote to the Organization of African Unity which contains much the same allegation. Some treatment of this issue is crucial to Mazrui's argument. Given Mazrui's view that Amin enjoys a form of charismatic popularity, it is logically imperative that he respond to exiled Ugandan intellectuals such as Wanume Kibedi and Joshua Wakholi who believe that the Amin regime survives not because of popular support for the general, but, instead, only by the systematic slaughter of real and imagined political opponents.

Mazrui's chapter on the economic transformation of Uganda under Amin is similarly inadequate. The economics of military rule are dealt with almost entirely in terms of the expulsion of the Asian community and the symbolic importance of this episode to African feelings, a subject which is covered in earlier chapters. There is no material whatsoever on the economic impact of the Asian expulsion or, for that matter, more generally on the performance of the economy under Amin's leadership. Certainly most knowledgeable observers posit a direct relationship between Amin's rule and the collapse of a once-thriving economy.

Mazrui's failure to deal with the issue of systematic political repression combined with the absence of any treatment of the deterioration of the Ugandan economy suggests a bias against the inclusion of subjects which might compel a more sharply critical treatment of the present military regime, and betrays unspoken admiration for Amin's style of leadership. The value of *Soldiers and Kinsmen* lies in its presentation of an informed insider's view of and reaction to the major political events of Uganda during the 1960s and early 1970s. But the book offers little to social scientists concerned about the political or economic conditions of Ugandans under military rule. Nor does it contribute to our theoretical understanding of ethnicity in African politics.

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Presidentialism in Commonwealth Africa. By B. O. Nwabueze. (New York: St. Martin's, 1975. Pp. 442. $25.00.)

This book is an extended commentary on the formal and juridical aspects of presidential rule in Commonwealth Africa. As such it will be of interest to lawyers and anthropologists concerned with problems of legal acculturation, and perhaps also to those jurists and technical assistants in the pay of African governments upon whom has fallen the thankless task of drafting statutory rules and regulations, amending existing laws and elaborating new constitutions. For most political scientists born after World War I, however, Professor Nwabueze's quest for constitutionalism—i.e., for "appropriate restraints upon the powers of government in the interest of the protection of individual liberty" (p. vii)—is scarcely the kind of intellectual exercise most likely to capture their professional interests. Not that issues of human rights and individual liberties are less worthy of attention in Africa than elsewhere; the question is whether constitutional engineering can provide anything like a meaningful basis for the protection of such rights and liberties. The constitutional record of most independent African states is dismal. In a continent where constitution making has become a chronic aliment, and constitutional reversals an all-too-common occurrence, the quest for constitutionalism is not unlike the search for the Holy Grail—a noble but self-defeating undertaking.

As I picked my way through *Presidentialism in Commonwealth Africa,* I was reminded of my brief but traumatic acquaintance with the constitutional realities of the Republic of Chad under the late President Tombalbaye. Five years ago, while doing field work in eastern Chad, I ran afoul of Tombalbaye's political mafia and ended up at the police station in Fort Lamy (now Njemena). Subsequently my case was deferred to the *Ministère Public.* This is when I first met the *Procureur de la République,* Omar Mahamat. I went on trial in early August, 1971, on charges of illegal entry into Chad, and illegal circulation on the Chadian national territory (both of which charges being totally groundless). After having heard my plea, Mahamat took me to his office and tried to call the president to ascertain what penalties, if any, should be meted out. Tombalbaye, as it turned out, was on a trip abroad. I was again brought to trial a few days later. Same scenario. Tombalbaye could not be reached. The third time around, having at last consulted the presidential oracle, the *Procureur* informed me of his verdict: a 30,000 CFA frs. fine, plus 30 days in jail. Shortly thereafter Tombalbaye changed his mind and decided to release me for health reasons. As I was about to board my plane, I was told that the president had had second thoughts about my case and wanted me to stay. How I eventually managed to make it back to the U.S. is another story which bears its own telling. Suffice to say that my experience of presidentialism in Chad fully corroborates Nwabueze's assessment: "The President in Africa has power, outside the normal scope of emergency powers in the advanced democracies, to make regulations for the detention of persons or the restriction of their movements—both citizens and non-citizens—and for any other thing that appears to him strictly required by the exigencies of the situation in the country" (p. 266). He goes on to quote Chief Justice Tregold to the effect that "[presidential] power is thus virtually co-extensive with the ordinary legislative authority of the Territory" (p. 226). Mahamat put it somewhat more succinctly: "Ici c'est Tombalbaye qui fait la loi; moi je l'execute." ("Here it is Tombalbaye who makes the law; my job is to put it into effect.")

The gap between constitutional norms and executive power is explored at some length by

Nwabueze, as well as the postindependence shift from one set of constitutional norms to another. Much of his discussion centers around the transformation of the Westminster model, in which the constitutional authority of the Head of State is kept separate from that of the Head of Government, into a presidential system in which all powers are concentrated in the same hands. Among the factors cited to explain this transformation, reference is made to what Nwabueze calls "autochtony," expressing "the desire of former colonial countries to give their political system a distinct national character and form" (p. 59), "africanism," "the personality factor," "the clash of interests," etc. Unfortunately missing from his analysis, however, is an effort to relate developmental goals to constitutional engineering, possibly along the lines suggested by David Apter and Martin Doornbos in their article on "Development and the Political Process: A Plan for a Constitution" (see David Apter, *Political Change: Collected Essays* [London: Frank Cass, 1973], pp. 118–46); what Nwabueze fails to show is how developmental priorities can be inserted into a constitutional formula, and pursued more effectively through institutional arrangements designed to promote specific developmental goals. His discussion of human rights, on the other hand, especially as it relates to the "safeguards available to a detained or restricted person," I found to be sensitive and illuminating, though unfortunately too legalistic to convey a realistic picture of the situation existing in specific African states. Nonetheless, at a time when human rights are openly violated by some African governments, and quietly ignored by others, one would like to hope that Nwabueze's comments might become required reading for African heads of state; if nothing else his book might help them clarify the distinction between Zionism and racism.

RENÉ LEMARCHAND

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Agrarian Revolution: Social Movements and Export Agriculture in the Underdeveloped World. By Jeffery M. Paige. (New York: Free Press, Macmillan, 1975. Pp. xiv + 435. $15.95.)

With this book Jeffery Paige, a young political sociologist at Michigan, joins the company of such writers as Eric Hobsbawm, Barrington Moore, and Eric Wolf, all of whom have attempted to come to some general understanding of rural social movements. But where Hobsbawm, Moore, and Wolf offer the insights and impressions of historical sociology, Paige provides a global theory with quantifiable parameters. Thus Paige's hypotheses are susceptible of verification or disproof by statistical techniques. In this respect Paige's work marks a considerable advance over that of his colleagues and predecessors in the study of agrarian movements.

Paige's study is centered on agricultural export economies—i.e., on those underdeveloped societies that have been caught up in the political economy of export agriculture and in which an enclave of commercial export agriculture exists side by side with the older subsistence agriculture. Eric Wolf and others have noted the destabilizing effect of commercialization on rural class relations. For Paige, the development of agricultural export economies is important chiefly as a way of accounting for the rise of a variety of patterns of elite-cultivator relations; for it is these patterns that are the true subject of Paige's book.

Paige's analysis is essentially Marxist, in that he views different kinds of political outcomes—e.g., intransigence or flexibility on the part of elites, rebellion, reformism, or revolution on the part of cultivators—primarily as functions of rural class relationships. His fundamental explanatory principle is income-source. Rural elites depend either on land or return on capital for their income. Cultivators are paid either in land-rights or wages. Combining these two dichotomies yields four types of elite-cultivator relations, each with its characteristic political outcome. For example, the combination of a landed upper class with wage labor or sharecroppers typically engenders class conflict that eventuates in a revolutionary movement, while a commercialized elite seldom provokes such radical behavior.

In a long chapter titled "A Theory of Rural Class Conflict" Paige elaborates on this basic concept. He develops "ideal types" of elites and cultivators and formulates a series of hypotheses linking rural class structure to political outcome through such intervening variables as the economic power of the elite, socioeconomic characteristics of the labor supply, structural features of the reward system, and the social structure of the cultivating class. These intervening variables are perhaps more accurately described as logical extensions of the ideal types. Thus Paige in this chapter creates a model of agrarian society that, he asserts, can predict the form that rural social protest will take for any given pattern of rural class relations.

In the following chapter Paige tests his model via a "cross-sectional study of 135 agricultural export sectors in 70 independent states and colonies in the underdeveloped world." The object of this exercise is to determine whether there are significant correlations between different patterns of rural socioeconomic organization and different kinds of agrarian social and political movements. The difficulties in such a study stem largely from the variable quality of the data employed; nevertheless Paige has evidently been able to surmount the methodological problems. The correlations that emerge are notably unambiguous and supportive of the hypotheses in Paige's theoretical chapter. Other scholars will, of course, wish to examine Paige's data sets and coding procedures before giving unqualified assent to his conclusions.

Paige then presents three case studies of rural social movements in Peru, Angola, and Vietnam. These studies are remarkable for their lucidity and the volume of scholarly literature they digest. Each case, exemplifying a different pattern of rural class relations, appears to bear out Paige's hypotheses on the expected political outcome associated with each pattern.

Yet the skeptical non-Marxist, and even the skeptical Marxist, may wonder at the remarkable consistency of Paige's results. Paige's model seems oddly free from inner dialectical contradictions. For any given pattern of elite-cultivator relations, the result seems pre-ordained; all hypotheses reinforce each other. The ideal types themselves are not wholly satisfactory. Thus Paige's view of landed upper classes as using political means to preserve a static situation, and of the commercialized upper classes as relying on market economics to increase their wealth, seems excessively simplified. Why must a landed elite prefer inefficient labor to efficient labor, or be rigidly opposed to a market economy? In nineteenth-century Japan, for example, some landowners saw the advantages of commercialization. And did not United Fruit Company obtain and preserve its commanding position by political means? Political power is as much an economic resource as land or labor, just as wealth in turn has political consequences. Is it useful, then, to treat economic strength as always distinct from political power? Paige, of course, is aware of the complexity of real events, the contradictoriness of actual, as against idealized, behavior. And he is usually aware that real cases cannot be fully described by ideal types. Yet sometimes he seems to forget.

Paige's emphasis on class structure imparts a curiously static quality to his discussion of social movements. Other writers have suggested that such movements are often the consequence not so much of structure as of structural change. Charles Tilly, in *The Vendee,* discusses the varying consequences of "urbanization" for peasant behavior in France in 1793. Eric Wolf and Barrington Moore argue that agricultural commercialization introduces new elites into the countryside and suggests that it is the conflict between old and new elites—between, for example, landed gentry and rural entrepreneurs, with their respective political allies—that is the key to understanding modern rural social movements. But in Paige's theory, inter-elite or intra-elite conflict is pushed to the periphery of his concern. Shifts from one pattern of rural social organization to another, with all their disruptive and energizing consequences, tend to fall in the interstices between Paige's ideal types.

No doubt the structural characteristics of society account for some of the variance in the political behavior of cultivators and elites, and set limits on the range of possible political outcomes. In providing a way of assessing quantitatively the contribution of these structural characteristics to political outcomes in his global statistical survey, Paige, I believe, makes an extraordinary contribution. But as one descends from global surveys to individual cases, explanation must become increasingly *ad hoc,* centered on idiosyncratic factors. Structure is now itself the "globe," the arena in which events occur. Factors that in a cross-national survey appear too derivative, evanescent, or unresearchable, such as psychological attitudes, relative deprivation, personality of leaders, communications, organization, aspirations, ideological appeals, now become central to any causal explanation. Yet Paige, in seeking to account for differences in bhe behavior of Malayan rubber workers and Ceylonese tea workers, refers only to structural differences between the two plantation systems. Similarly, in his chapter on Vietnam, Paige is chiefly concerned to show the connection between patterns of land tenure and types of peasant political behavior. In both cases, communist organizational activity is not treated, as we would expect, as a key element in any ultimate explanation of these events, but merely as a function of the structural characteristics of the local economy. Paige is too zealous in his efforts to demonstrate the explanatory power of his fundamental hypotheses. Not even Marx would have attempted to explain so much with so little.

Despite these qualms and caveats, I believe this is in many ways a remarkable book. At its best it is brilliantly argued. It combines an elegantly presented theory with encyclopedic mastery of relevant scholarly literature and considerable methodological sophistication. It should take its place as one of the handful of really significant discussions of its subject since Marx's *The Eighteenth Brumaire of Louis Bonaparte.*

IRWIN J. SCHULMAN

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France at the Polls: The Presidential Election of 1974. Edited by Howard R. Penniman. (Washington, D.C.: American Enterprise Institute, 1975. Pp. 324. $4.50, paper.)

France at the Polls is a useful volume, containing perceptive analyses of the 1974 election for both students and practitioners. The eight separately authored chapters are much better integrated than in most similar works. Careful writing makes the work accessible to those without intimate knowledge of French politics. The uninitiated will be confused only as to the content and scope of Gaullism, particularly as it applies to the winner, Valéry Giscard d'Estaing. The editor, at one extreme, tells us that Giscard led a coalition "usually described as 'Gaullist'" (Preface). Jean Charlot is more accurate: "Giscard is without a doubt a pillar of the Fifth Republic, but he is by no means a 'Gaullist" (p. 96). The Left imposes cohabitation on the forces of "order," but the core of Gaullism, as represented by former Premier Debré and others, is sharply critical of Giscard's policies.

The initial chapter, Roy Pierce's historical overview, unfortunately opens with too much irrelevant detail on a remote past—for example, Grévy's resigning the presidency in 1887 because of his son-in-law's scandal. In happy contrast, the balance of the chapter is an excellent analysis of the Fifth Republic presidency prior to the 1974 elections.

Each focusing on one of the three major candidates, the next three chapters cogently portray the 1974 events. Serge Hurtig's account of the Left and its candidate, Francois Mitterand, is the best balanced. Jean Blondel tilts to the superman image of Giscard's victory as a surprising personal effort carried out with little organization. As Alain Lancelot indicates in a later chapter, polls conducted prior to President Pompidou's death showed that Giscard started with a better image than any of his opponents. He undoubtedly was well financed. And, as mentioned by Charlot in his chapter on Jacques Chaban-Delmas and the Gaullists, Giscard has devoted the previous eight years to building an organization that, if not on the scale of the Gaullist UDR, was far from negligible.

The two most important chapters in the book are those by Alfred Grosser and Lancelot. Both are "must" reading assignments for academic courses on France. Grosser presents a valuable introduction to "The Role of the Press, Radio, and Television in French Political Life." My only regret is the omission of the extreme-left publications which, despite trivial circulation, have a certain influence on the national press.

In his study of opinion polls, Lancelot discusses the polls' commercial organization, the issue of whether polls influence elections, and debate over government regulation of polling. His analysis of poll results indicates that the critical element in victory was the electorate's perceiving that Giscard had greater competence than the other candidates. This is more convincing than the policy emphasis found in Charlot's analysis. Charlot contends that Chaban erred in campaigning as a man of change whereas his basic electorate was conservative and nationalist. Close inspection of Charlot's data, however, suggests that his interpretation is questionable for several items and, in any event, the *average* policy differences between the Chaban electorate and the Giscard electorate were not substantial.

None of the authors highlight the *dispersion* of Giscard's electorate. Data on p. 193 show Giscard scoring relatively best in the Center, among voters refusing a Left-Right placement, and on the extreme Right. Chaban, in contrast, peaked in the Right category. The data attest to Giscard's ability to pull in the "outs" of the De Gaulle reign: in 1965, both the 16 percent that had voted for the Centrist Lecanuet and the 5 percent that had voted for the extreme Right candidate, Tixier-Vigancour.

Giscard's broadening the majority to include not only Lecanuet but also Servan-Schreiber represents the latest and perhaps final stage of the dilution of Gaullist rule that began when the "French Algeria" crowd was amnestied after the 1968 uprising and continued with the Duhamel Centrists' joining the Pompidou majority. Certainly this coalition-building process merited more discussion than it received. Charlot alone gives it more than passing attention.

Even with the enlarged coalition, Giscard just squeaked through. Have all the reservoirs been tapped, suggesting an inevitable victory of the Left? Or is the electorate of the most bourgeois of nations engaged in sophisticated signaling that both warns the Right and keeps the Communists from power? Examination of these questions, as a match to Charlot's excellent discussion of the future possibilities of the Gaullists, would have been welcome.

There is also an interesting chapter on the major candidates' use of television by Monica Charlot. This chapter, subject to the usual methodological reserves on counting in content analysis, also testifies to the skill and competence of Giscard.

The chapter on electoral geography by Marie-Therèse and Alain Lancelot is unfortunately the only one that has suffered in translation. Methodologically, complementing the maps by statistical analysis would have been preferable to the exposition of cartographic analysis methods, something American readers have already seen, if not in Siegfried, in Gosnell and Key. Substantively, the chapter shows the 1974 results fit a long historical geographic pattern for both Left-Right voting and participation rates.

France at the Polls concludes with an inadequate appendix of departmental statistics for the three Fifth Republic direct presidential elections. Richard Scammon failed to indicate the data sources, omitted (in contrast to the Lancelots' article) valuable data on registration and spoiled ballots, and neglected to state that the departmental boundaries of Rhone and Isère were significantly modified between the 1969 and 1974 elections.

Despite the reservations advanced here, the volume has generally met the meritorious objectives of the series of which it is a part. These goals are, in the editor's words, helping "readers to gain a better understanding of the operations of their own political institutions" and informing "those who are responsible for writing legislation to improve their nations' electoral machinery" (Preface).

With respect to the latter point, Americans might look closely at the French system. In the U.S., the primary, convention, and electoral college systems are all under persistent attack. Remedies, including proportional representation and quotas, have hardly led to a new consensus. Turnout is low, for better or for worse. Many object that the nomination process is frequently captured by those entranced with the purity of position rather than the stained compromise of election.

The two-ballot system described in *France at the Polls* has, in contrast, increasing legitimacy. (The increase in popular support can be found in poll results that were unfortunately not discussed in the book.) Partisans of a national primary and the abolition of the electoral college will take comfort both in the fact that turnout on the first ballot has been consistently high and in the calm public acceptance of the result of the 1974 election, despite the slim margin of 400,000 votes for Giscard.

Those opposed to the length of the American campaign will be pleased to note that the two ballots are separated by only two weeks, as against the three-month period between the conventions and the U.S. elections.

The key difference between the French and the American system lies in the voter choosing among *all* the candidates on the first ballot, with the top two then competing in the second ballot runoff. Clearly such a system—which does not preclude prior party meetings—would avoid throwing an election to the House. It may also—a topic for more serious research—be more likely to produce the Condorcet winner if one is present while, at the same time, allowing the McGoverns, Goldwaters, and Wallaces to have their say.

Expression of minority position is in fact encouraged, as pointed out by Monica Charlot, by the provision of equal and virtually free television and radio time to all first ballot candidates. Many felt that the presence of 12 candidates would lead to frivolous diversion; in this observer's view the Trotskyist Arlette Laguiller's demanding equality for women, the ecologist Rene Dumont's pleading for the environment, and the conservative Jean Royer's attacking the permissive society all added positively to the more muted presentations of the major candidates.

Both the French legislation regarding television and the electoral procedure deserve careful attention in proposals for reform elsewhere, despite the skepticism mandated by obvious differences in political culture and institutions.

HOWARD ROSENTHAL

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China's Modern Economy in Historical Perspective. Edited by Dwight H. Perkins. (Stanford, Calif.: Stanford University Press, 1975. Pp. 344. $13.85.)

The central theme of this volume is "the degree to which China's post-1949 economy was a product of the nation's own past." It is a welcome antidote to the commonly held view that the Communists made a clean break with tradition when they gained power in 1949.

Dwight Perkins, the editor of the volume, contributes the key essay, an analysis of the twentieth-century development of the Chinese economy. He shows that modern industry and transport had got a good start well before 1949.

Indeed, from 1913 to 1937, industrial output rose at the respectable average rate of 9 percent a year. The national product grew at a slower rate, and there was even a small increase in per capita income.

Development was interrupted by external aggression and civil war, but by 1957, per capita national product had attained a level about 20 percent higher than that of 1933. Consumption did not rise, however; the increase was in investment. Perkins argues that the widespread belief that the Chinese people were materially better off in the 1950s than they had been a quarter of a century earlier was due to the cessation of internal disorder and to a drastic redistribution of income. That there was redistribution is beyond question, but what is not so obvious is its quantitative significance for raising average living standards. Perkins' numerical example, in which a reduction of the income of the top 5 percent of the population to the average level could raise the bottom half by 50 percent, implies that in 1933, the latter group was living on a *per capita* annual income of something like $20 (at 1952 prices). This is hardly possible; assessing the impact of income redistribution is a tricky business, particularly where the data are so poor.

Perkins makes it clear that the virtual nonexistence of statistical data since 1958 makes estimation of subsequent developments a hazardous occupation. However, he concludes that from 1952 to 1971, the national product was growing at somewhere between 4.5 and 6 percent a year, figures that bracket the recent U.S. government estimate of 5.6 percent. (Derived from Congress of the United States, Joint Economic Committee, *China: A Reassessment of the Economy*, July 10, 1975, p. 23.) Since population was probably increasing at more than 2 percent a year, *per capita* income was rising on the order of 3 percent annually. It might be noted that other careful scholars have estimated lower national product growth rates, 2.3 percent and 4.1 percent (both for the period 1952-1970), which would imply little *per capita* income increase (Subramanian Swamy, *Economic Growth in China and India, 1952–1970*, Supplement to *Economic Development and Cultural Change*, July, 1973, p. 63; T. C. Liu and K. C. Yeh, "Chinese and Other Asian Economies," *American Economic Review*, May, 1973, p. 218).

There are a few points that struck this reviewer as odd. Perkins rejects the "official" indexes of Chinese industrial production on the grounds that they show "extraordinarily high" increases, yet he prefers "official" Chinese to U.S. government grain crop estimates because he finds it more reasonable to "use the estimates of trained statistical workers in China rather than those reconstructed from incomplete data from a distince of some 10,000 miles away." Are there no equally well-trained industrial statisticians? He characterizes the forced, permanent rusticiation of tens of millions of high school graduates, one of the more inhumane aspects of Chinese Communist policy, as just part of a government effort "to press urban residents into projects in the countryside." These matters aside, for anyone who wants a convenient, succinct summary of the facts of Chinese economic development during the present century, there is no better source.

Robert F. Dernberger contributes an excellent chapter on the role of foreign capital in China from 1840 to 1949. Rather surprisingly for one who has been brought up on a diet of unequal treaties, he concludes that neither direct investment nor foreign trade destroyed indigenous industry, but on the contrary, "made a positive *direct* contribution to the Chinese domestic economy." Thomas G. Rawski advances the thesis that pre-Communist manufacturing enterprises played a very substantial role in subsequent industrial growth. Kang Chao confirms the conclusions reached by Dernberger and Rawski through a case study of the cotton textile industry. He shows that modern spinning mills had an adverse effect on handicraft spinning, but that handweavers were able to compete because of the greater labor intensity of the operation and thanks also to the entrepreneurial talents of the cotton merchants, who organized an efficient putting-out system.

Carl Riskin asks whether there was a "surplus" available for investment in pre-Communist China, and after a bit of juggling with 1933 national accounting data, concludes that a substantial amount of resources could have been mobilized, which "would have involved very little opportunity cost, aside from the sacrifice of privileged consumption habits on the part of a relatively small portion of the population." This should be welcome news to the many contemporary less-developed countries which have come to a different conclusion, namely, that to achieve an investment rate of 20 to 30 percent, *everyone* has to pull in his belt, and that this is not so easy to accomplish where income is already low and people have options about where they work and how they spend their money.

Ramon H. Myers describes the similarities and differences in methods of rural cooperation in the traditional Chinese village and the modern collective farm. Peter Schran endeavors to show the impact of the Yenan experience on current Communist economic policy. There is a fascinating essay by Mark Elvin on the pattern of technical change in traditional China, though its relevance to the main theme of the book is not clear. The same might be said of John C. H. Fei's artistic mapping of William Skinner's notion of the "standard market" in China, but the essay should be of considerable interest to those concerned with location theory.

Lest some of the foregoing remarks give rise to misconceptions about the overall quality of *China's Modern Economy,* it is necessary to add that this volume contains some of the best recent scholarship in the field of Chinese economic studies. It is stimulating and provocative, and can be read with profit by the specialist and non-specialist alike.

WALTER GALENSON

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Urban Latin America: The Political Condition from Above and Below. By Alejandro Portes and John Walton. (Austin: University of Texas Press, 1976. Pp. x + 217. $13.95.)

For the past two decades or more, urban areas in the Third World have come under increasing scrutiny by social scientists of all persuasions. In Latin America, the earliest research that concerned political scientists in particular developed through the desire to submit some early generalizations to empirical test. Seeing urban areas confronted with growth rates that threatened to double every ten years or less due in large part to vast rural-origin migration, political developmentalists forecast rising levels of radicalism, anomie, and other pathological conditions as massive demand-making by newly politicized masses seemed certain to overwhelm governments with inadequate resources. Work by both Latin America and foreign observers, however, including Cornelius, Matos Mar, Morse, Nelson, Roberts, Robles, Perlman, and Ugalde, found such easy assumptions deficient because they are based on a wide variety of misconceptions.

But revisionism, as usual, produced a new generation of questions increasingly comprehensive and complex. As a result, contributions to the literature, both in series such as the annual Sage *Latin American Urban Research* volumes as well as in various case studies, have simultaneously gone off in many directions, often leaving theoretical and analytic coherence badly tangled.

Portes and Walton, both urban sociologists, attempt to bring together several strands of research with considerable success. By adopting a perspective that sees "political control and the mechanics of access to power" (p. 4) as fundamental, they synthesize existing work and suggest new areas for investigation. As their subtitle indicates, the authors feel that elites and masses must be considered joinrly, as they affect one another and as both influence economic, planning, and demographic problems that face Latin America's cities.

Rather than undertaking actual joint authorship, Portes and Walton each provide two chapters, apparently combining their efforts only for the conclusion. Portes' first contribution presents a structural-historical interpretation of Latin American urban development as primarily a creation of Iberic colonialism. The heritage of concentrating resources almost exclusively in cities, Portes argues, has combined with late-arriving dependent capitalism to generate today's contributions which have had to confront an enormous influx of urban-bound migrants who have come to realize that progress and change reside in the cities. In his second chapter, Portes gathers together much micro-level descriptive and survey data to examine how and why a variety of structural conditions influence the political behavior of the urban poor. He concludes that "the political importance of urban marginal groups is limited . . . by the fact that . . . circumstances are largely made for them and not by them" (p. 109), and that the conditions of urban poverty and elite politics together determine behavior patterns and structural changes within urban society.

Walton's two chapters deal with urban elites. Like Portes, he discusses largely new materials in one chapter while presenting an extensive summary and secondary analysis in the second. The new data compare elites and their varying linkages to urban development in Monterrey and Guadalajara (Mexico) and Medellín and Cali (Colombia), four cities purposely selected to maximize variation on some variables while controlling for others. This admittedly preliminary descriptive chapter examines urban planning and its very different impacts on all four cities, discusses elite compositions briefly, and ends by cautioning against simplifying complex realities for theoretical parsimony and fit. His second chapter tests several propositions that deal with elites and urban development throughout Latin America; he musters some twenty studies from seven countries as his data base, and finds the propositions with support ranging from strong to nil.

In their final chapter, Portes and Walton conclude that while elite dominance of urban social structures still holds sway, some possibly vulnerable points exist (internal factionalism, dependence, and increasingly sophisticated masses). They then suggest some directions for further research, indicating concerns both internal (e.g., land markets) and external (e.g., city-hinterland relationships and national urban hierarchies) to a particular urban context.

It is difficult in a review to indicate the substantive summary wealth, the methodological richness and variety, and the theoretical sensitivity contained in less than 180 pages of text. While the four major chapters are clearly separate works by two independent scholars, the book is nevertheless knit together with numerous cross-references and strong mutual understandings of one another's goals. And while values and theory preferences are present, they do not whipsaw the data or paper over conceptual difficulties.

What the book does not do (nor did it intend to do), of course, is to carry out its own major suggestions. It does not study elite/mass interactions within a single urban area, under a specified political system, nor does it demonstrate how the many levels and units of actors and social structure identified do in fact influence urban power and its distribution. Such detailed knowledge, and the theoretical bricklaying that must accompany it, can only derive from a series of case studies and comparative analyses. And from a broader perspective, the conclusions offered here need cross-regional testing with other Third World data as well. As a summary, however, and as a most suggestive research inventory, *Urban Latin America* offers a great deal.

HENRY A. DIETZ

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Mao Tse-tung: The Man in the Leader. By Lucian W. Pye. (New York: Basic Books, 1976. Pp. xv + 346. $12.95.)

In recent years psychoanalysts have increasingly focused their attention upon the preoedipal mother-child relationship, and upon the narcissistic trend in character development. The oedipal tragedy has been largely replaced by a far more primitive drama, in which a voracious child is both persecuted and sustained by its mother's breast, and in which (in certain circumstances) the child develops into a grandoise self stalking Tarquin-like through a world populated by its own mirror images. Because psychobiography derives its categories, concepts and techniques from psychoanalysis, it was to be expected that here, too, Oedipus would give way to Narcissus. Thus Lucian Pye's interpretation of Mao Tse-tung:

> The strong nurturing support of his mother gave him an enduring craving to recapture the bliss of omnipotence and thus turned his psychic forces inward onto his ego, thereby providing him with his sense of narcissism that became the basis of his charismatic appeal. Yet she also "abandoned him" [by giving birth to other children], and thus taught him thereafter to constantly guard his commitments of feelings, and never to direct his affect toward persons or objects close at hand, but rather to use idealization as a defense mechanism and to reserve his feelings for abstract and distant objects, such as utopian revolution, world political forces, and the movement of history (p. 306).

Pye argues that, although Mao's father played a significant role in his childhood, he was not a tyrant who drove his son into rebellion. Rather Mao's "sense of rage with the world came from having once experienced such security and supporting reassurance, which when it could not be sustained made him feel that he was forever being cheated" (p. 307). As such, he was a kindred spirit and natural leader of "the contemporary generation of Western radicals whose parents went to inordinate lengths to ensure that their infancy was free of all insecurities and frustrations ... but whose world could never live up to these implanted expectations" (p. 308). Maoism, we are asked to believe, is a narcissistic striving after infantile bliss, a search for an infinite supply of mother's milk and honey.

There is unquestionably a kind of truth in the theoretical trend from which Pye's study derives, and at least some truth in his characterization of Mao. He does identify recurrent patterns in Mao's behavior, for which he often provides plausible interpretations. But even leaving aside his rather careless handling of his raw material, he has not mastered the psychobiographer's trade. For example, he observes that Mao grew up in a "remote" and "isolated" small town. He then remarks that "these words capture not just the region but the personal character of Mao Tse-tung" (p. 71). If this comment is as seriously intended as its rhetorical phrasing suggests, then Pye means to say that the remoteness of the town contributed to the remoteness of Mao's character. But the town, which was filled with Mao's extended family, would not have been experienced by young Mao as remote or isolated. Or again, Pye infers that during his childhood Mao seldom had "the thrill of visiting a market town," and juxtaposes against this inference the claim that "as an adult he has never shown great interest in economic matters" (p. 70). Presumably Pye's point is that the lack of economic exposure in the first instance determined a lack of interest in economics in the second. But with no hypothesis as to why this should be so, no attempt to mediate between the childhood experience and the adult attitude, and no statistical data indicating that adult attitude and childhood experience typically are correlated in this manner, the juxtaposition is meaningless.

Another example of Pye's reasoning bears more closely upon his central argument. At one point in his childhood, Mao's father accused him of being lazy. Several years later the son wrote some comments condemning laziness. Pye states "not just the sentiment, but the very words seem to be those of his father. Could a boy who was deeply rebelling against his father on precisely this issue of laziness have uttered such thoughts only five years later?" (p. 123) The answer to this begged question is: of course. Not only could laziness mean something different to father and son, but also identification with the aggressor (taking over one or more of the attributes of, especially, a parental enemy) accompanies exactly the deepest of rebellions.

In short, there are reasons for skepticism concerning Pye's interpretation of Mao. But the more critical deficiencies involve the imputed relationship between Mao and his situation. First, Pye accepts an a priori distinction between the private and public Mao ("the man in the leader") which both distorts the picture we are given of the person and proves worthless for categorizing either interpretations or biographical data. For instance, Pye says that "as Mao rose in power during the Long March, . . . critics and supporters suddenly found him to be a man of decisive action, great energy and increasing aloofness" (p. 23). He also says that Mao apparently "trusts the world enough to feel he does not have to dominate to have his way" (p. 57). The latter statement presumably depicts the public Mao, the former statement the private one. . . . Alice's problems in distinguishing between Tweedledum and Tweedledee pale in comparison to this conundrum of double identity.

Pye then places his schizoid creation in an historically indeterminate world of psychological universals. But a universal experience such as the loss of infantile omnipotence cannot conceivably distinguish Mao and the Maoists from any other individual or group; and it is quite useless to postulate particularly intense reactions to the loss when, outside the therapeutic context, there is no standard for measuring such intensities. Which is to say, Pye has only established that Mao is human, and that there is an emotional dimension to human communication. One therefore wonders what conscious or unconscious purpose has been served by viewing Maoism as a kind of narcissism.

Finally, even if Pye had developed his argument with far greater skill, he still would have explained very little about Mao or Maoism. For the man and the movement cannot be understood outside of the particular historical situation which they occupied. That situation, in turn, cannot be understood in purely subjective terms. Rather in theory and in practice it is determined through the clash of conflicting class and national interests. Emotional factors play a part in this conflict, and it is a worthwhile enterprise to work toward their elucidation. But when psychological concepts swallow up objective reality, then one is assuredly trapped in a world of projected intentions and mirror images.

EUGENE VICTOR WOLFENSTEIN

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Iran's Foreign Policy 1941–1973: A Study of Foreign Policy in Modernizing Nations. By Rouhallah K. Ramazani. (Charlottesville: University Press of Virginia, 1975. Pp. 507. $20.00.)

A lengthy study of modern Iran's foreign policy might seem a dubious enterprise to those who question the existence of a truly independent Iranian policy after 1941. In this sequel to his earlier book, the *Foreign Policy of Iran, 1500–1941*, Rouhallah Ramazani provides some evidence that Iran's leaders retained a creditable freedom of action in foreign affairs even during the allied wartime occupation and through a decade of cold-war dependence on the United States. The author stresses the concept of "autonomy" to explain Iran's foreign policy in each of four phases—the third-power strategy, negative equilibrium, positive nationalism and independent national policy. He claims, moreover, that this urge to optimize freedom of action in an international system, prompted by the desire to construct or retain self-respect, offers a key to understanding foreign policy in *all* modernizing nations. In an introductory chapter, the author relates familiar concepts and categories in the development literature to the quest for autonomy, and he identifies several interesting empirical questions and a promising research framework. Yet when Ramazani turns to examine the Iranian case, his analytic designs are quickly put aside. Instead, he writes an essentially diplomatic history, ranging chronologically over the period 1941–1973.

This basically descriptive approach is well suited to display the author's impressive familiarity with his subject. With much objectivity, he details the major events and policy choices of the period and provides a frequently persuasive picture of the aims and assumptions that have underlain government decisions. The study is particularly insightful in finding how political motives were normally ascendant over economic and social factors. Far less successful, however, is the author's organization and discrimination in the use of his materials. Exhaustive descriptions of diplomatic exchanges, texts of agreements and public statements run on without clear purpose. Too often where the reader seeks a synthesis of arguments, one finds unnecessary repetition and recapitulation or, alternatively, an aborted discussion. Thus while benefiting from a detailed account of the policies that led to and followed the Soviet decision to quit Iran's northern provinces in 1946, the reader looks in vain for a summary analysis of the style and principles of strategy that guided Prime Minister Qavam, then the architect of Iran's policy. In another instance the author offers three conventional explanations for the fall of Prime Minister Musaddiq in 1953 without even the briefest assessment of the relative validity of these explanations. In these and other discussions, Ramazani makes use of valuable primary sources, but has chosen, rather oddly, to ignore a number of relevant secondary studies.

Ramazani also hesitates to probe behind the public record—that is, to consider the more covert but no less critical aspects of recent foreign relations. Absent, then, are enlightening discussions of the extent of Iran's post-1966 involvement in the Kurdish struggle with the Baghdad government, military assistance to the Sultan of Oman, strategic maneuvers in Iran's Baluchistan, the numerous trade and training agreements with Israel after 1956, and Iran's posture during the October, 1973 Middle East War. Indeed, the book is mainly an examination of Iran's relations with the United States and the Soviet Union and, to a lesser extent, Great Britain. Only in a penultimate chapter does Ramazani consider in any detail Iran's regional and small nation relations. (Compare this with the recent volume by Chubin and Zabih, *The Foreign Relations of Iran,* where more than half the text is devoted to this increasingly prominent aspect of policy.) Apart from a useful survey of issues and developments in the Persian Gulf, the dimensions of subsystem relations with Egypt, Turkey and Pakistan are not fully explored; and Iran's convergent interests and possible future political and economic competition with India are entirely ignored. The reader's understanding of the region is not aided, moroever, by the book's several nearly illegible maps and an appendix of marginal value.

The need to link foreign and domestic policy studies is a repeated, though hardly original, theme. Ramazani offers several good illustrations of how internal security concerns dictated policy choices. Much attention is devoted to Musaddiq's differences with Iran's Communists and their attempted exploitative role during the oil nationalization crisis of 1951–1953. At the same time, the influence on foreign policy options of Musaddiq's larger coalitional difficulties and the pressures of domestic economic deprivation are hardly noted. Also the way in which foreign policy has been used more recently to distract and disarm the regime's domestic opposition in less than adequately explained. Perhaps the more interesting assertion of linkage, that the "new look" toward the Soviet Union in 1972 helped prompt the Shah to launch his White Revolution, warrants more than circumstantial evidence. More important, throughout these discussions we learn little about the process of foreign policy making. Even if we accept the author's view, that the Shah has monopolized the formation of foreign policy since the mid-1950s, and that elites and institutions assume at best implementing roles, still a final chapter touches all too lightly on how these instrumentalities are employed by the monarch, how human and material resources may constrain his choices, and to what extent the Shah's style inhibits or facilitates an adaptable foreign policy.

In that Ramazani's study carefully documents and intelligently explains more than three decades of foreign relations, especially with the super-powers, the book has much to offer the student of modern Iran. But given its substantive limitations, its analytic diffuseness, and its failure to operationalize its own design, the volume is less than satisfactory as a systematic study of Iran's foreign policy or as the basis for conceptualizing policy in other developing states.

MARVIN G. WEINBAUM

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Politics in Argentina, 1890–1930: The Rise and Fall of Radicalism. By David Rock. (New York: Cambridge University Press, 1975. Pp. 315. $19.50.)

The history of Radicalism long has both intrigued and confused scholars and observers of the Argentine political scene. Formed in the 1890s as the *Unión Cívica Radical* (Radical Civic Union), the Radical party developed a mass base among the country's middle class and captured the Argentine presidency in 1916. Two Radical presidents, the enigmatic and charismatic Hipólito Yrigoyen (1916–22; 1928–30) and the aristocratic and amiable Marcelo T. de Alvear (1922–28), oversaw a 14-year period of UCR rule which finally ended with a military coup in September 1930. David Rock's analysis provides us with the best description and explanation yet published of this period and the role of Argentine Radicalism in it.

Rock's approach is to examine the interaction of four major groups in Argentine society—the landed and commercial elite, foreign capitalists (mostly British), the urban middle class, and the urban working class—and their relation to the evolution of the Radical movement. These groups, in turn, are related to the growth of Argentina's primary export economy, with particular focus on the impact of cyclical fluctuations on the political system.

Within this framework Rock traces the rise of Radicalism in the 1890 to 1916 period, arguing convincingly that the UCR originated as a protest movement among the elite seeking a new distribution of political power and economic benefits. Revising the standard interpretation, he shows that the party's leadership was largely from the upper-class landowning and commercial sectors, and remained so until the 1920s, when the middle-class groups attracted to the movement after the turn of the century began to assume control of the party. He also discusses Radical ideology, such as it was, a vague mixture of disparate ideas which stressed political reform, said little about social and economic change, and served well the party's intention to form a broad coalition of different class and regional interests.

Rock's main emphasis is on the Radical administrations, especially Yrigoyen's first term. The Radicals achieved and retained power, he argues, through the development of a political machine based upon local ward committees and political bosses. The machine recruited party adherents largely from the middle class, promising various forms of patronage, the most important being government jobs. Patronage depended upon state expenditures and government revenues, which tended to vary with overall economic conditions. Rock uses this relationship as a basis for explaining Yrigoyen's difficulties in his first term, the split within the party in 1924, and the ultimate disintegration of *yrigoyenismo* in 1930.

In addition to gaining middle-class support during his first administration, Yrigoyen also sought the backing of the urban working class, particularly in the city of Buenos Aires. Drawing upon a wide range of sources, Rock describes the connections between the government and the fledgling unions of the period and analyzes the effect of these connections upon the three other groups of his framework. While this is a fascinating account, it might be argued that the attention given to Radical-proletarian relations, about one-third of the book, somewhat distorts the importance of this issue in examining the overall development of Radicalism in these years. As the author notes, working-class voters were concentrated in the city of Buenos Aires and represented only a small portion of the total electorate. A more profitable approach to examining Radicalism as a national phenomenon might be a study of Radical activities in large agricultural provinces such as Buenos Aires, Córdoba, Entre Ríos, and Santa Fe, where the party received the bulk of its electoral support.

This caveat aside, Rock has masterfully traced the complex story of Argentine political developments over a 40-year period and constructed a stimulating analytical framework for examining political groups and forces. This is a book that all students of Argentina will want to consult and one which seems assured of being considered a landmark in the literature about Argentine political history.

RICHARD J. WALTER

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Dahomey: Between Tradition and Modernity. By Dov Ronen. (Ithaca, N.Y.: Cornell University Press, 1975. Pp. xi + 272. $16.50.)

This is the sixth volume in the *Africa in the Modern World* series, under the overall editorship of Gwendolyn Carter. As a country analysis, restricted to the progression from tradition to modernity, this volume warrants close attention by students of that aspect of political development in Africa. To suit his purpose, Romen has selected five major areas of inquiry: (1) the country's poverty, (2) "regionalism," (3) "the dichotomy between deeply rooted traditional and imported institutions," (4) the mass-elite conflict, and (5) the role of the military. A sixth area of inquiry has not been accorded the importance it deserves. The bureaucracy, or the "salariat," is noted but essentially, and critically, undervalued. But more of that later.

This work's consideration of Dahomey's poverty is information but unsatisfactory, for several reasons. Theories of imperialism or colonialism, or neocolonialism for that matter, are not utilized. Also, this part of the study lacks the extensive statistical support needed for poverty analysis.

The equation of regionalism with tribalism is curious. It can only be explained by the author's apparent decision not to avail himself of the rich literature on ethnicity in Africa, in West Africa in particular. To be sure, in the case of Dahomey, tribal and regional identity appear to overlap substantially. However, be that as it may, the two terms represent different concepts, convey substantially different meaning, have different causes and effects, and their equation, or confusion, must of necessity obscure the real bearing of either subject on the study of political or economic development.

Analysis of elections in Dahomey follows the traditional routes of inquiry, based mainly on official information. Romen's coverage is extensive, but given the current state of knowledge on political parties, elections, campaigning, and voting behavior in Africa, Ronen would have been well-advised to discount heavily French or Dahomean official versions of that entire subject. The author deserves credit, however, for his treatment of the core of much of Dahomey's political party game, namely the highly personalized conflict between a mere handful of contenders for power.

Analysis of the educated elite could have been strengthened by statistical support and broader perspective. The author's contention that the "adaptation of Western institutions by the political elite separated them from traditional values by which the majority of the country's citizens live" (p. 156), is not borne out by the evidence he himself presents, and that not too clearly. As is their inalienable right, the Dahomean elite have invented their own institutions. The author's contention that "the political bankruptcy to [sic] which Dahomey arrived and the resultant military interventions can be understood only in the light of the [separation of the educated and the political elite from the rest of society]" (p. 156), reflects a misperception of political development in Africa generally. What has never been functional or operational cannot become bankrupt. Dahomey never adopted, or adapted for that matter, any kind of western institutions, and its educated and political elites are separated from the broad masses for basically the same or similar reasons as may be observed throughout the world. If any institutional transfer took place, it never went beyond transfer of fragments, or of facades. Separation of an educated elite from the masses is primarily a socioeconomic matter. Military interventions occur because colonial rule favored the military. The civilian alternative was deliberately neglected.

Finally, there is the critical mass in Dahomean politics, the *salariat*. To be sure, Ronen is aware that an extraordinarily large share of the national revenue is claimed by that voracious segment of the Dahomean polity. But he does not seem to draw the appropriate conclusions from this central fact. The phenomenon is well known to students of Senegalese politics. There the national budget has been referred to as *état des soldes* or statement of pay, with predictable consequences in terms of regime stability in the event of a budgetary shortfall. Moreover, examination of the real or ultimate sources of the Dahomean budget, would have brought the author closer to a major theme of politics in French-speaking Africa, namely, the French connection.

HENRY L. BRETTON

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Grass Roots in an African City: Political Behavior in Nairobi. By Marc Howard Ross. (Cambridge, Mass.: MIT Press, 1975. Pp. 169. $19.50.)

This short, clearly written book is concerned with the relation between social structure and political behavior in a large African city. Most of the data is derived from a sample survey (carried out in 1967) of 498 adults in two African public housing estates—one a working-class estate, another an upper middle-class area. The survey material is supplemented by participant observation and a limited amount of documentary evidence. Departing from what has become the conventional approach to urban politics in Africa—an approach incorporating political history, administration and policy as central variables—Ross' book seeks explicitly to understand the influences on mass political attitudes and participation. The discussion which follows will deal in turn with the three central concerns of the book: social structure, ethnicity and political participation.

As in most other East and Central African cities, the population of Nairobi consists largely of first-generation migrants. To what extent, and in what ways this population has become "urban" has been a recurring theme of much of the now-classic anthropological literature on social change in Africa. Neatly summarizing the adaptive patterns of Nairobi's Africans to urban life through the concept of "life style," Ross discusses family structure, continuing ties with the rural areas, and urban social networks. His data shows that men with higher formal education (and thus, in a bureaucratically dominated economy, higher incomes) tend both to have urban centered families *and* to maintain a higher than average level of contact with their rural areas of origin. Contrary to the predictions of some, class differentiation in the urban areas does not seem to herald—at least for the time being—an increasing rural-urban conflict. Ross' material further shows that continuing investment in rural landholdings by the urban middle class relates both to pervasive attitudes about land as ultimate security, and to a pattern of social relationships based on rural district of origin. As he puts it, "Nairobi and the countryside are not seen by most people as two points on the rural-urban continuum. Rather, they are two locations within the same social system, and a person can move back and forth between them without severing his ties with either" (p. 131).

A related finding is the centrality of ethnicity in both social life and politics. In rejecting both the "melting pot" approach to ethnic assimilation and Daniel Lerner's argument that urbanization should lead to increasing empathy between groups, Ross suggests that urbanization in the context of scarce resources may lead to a "cultural pluralism" in which people are largely encapsulated by ethnic-bound networks and political attitudes. This is brought out clearly by a comparison of the Luo and Kikuyu groups in the sample. Within the ethnic networks, however, class still has an independent influence on the "life style" variables. This suggests to Ross the need to re-examine models of urban politics which posit an incompatibility between communal and class-based loyalties.

For political scientists, the most intriguing (if not surprising) finding in the book relates to the nature of political participation. In his survey, Ross asked respondents whether they followed the news about politics, talked about politics, attended political meetings, and whether they had ever (for example) voted, joined a political party, or seen a government official about a problem. Based on "yes" or "no" answers to 18 such items, factor analysis showed the responses could be grouped into two independent dimensions. One dimension reflected what Ross calls the "independence style," involving the active, mass-type politics characterizing the late period of nationalist mobilization; the other dimension reflected the "postindependence style," involving acquisition of political information and contact with decision makers. Pearson product-moment correlations revealed that income and formal education had the strongest relationship to these dimensions, varying negatively with the first and positively with the second. While the conclusions one can draw from this for the preindependence period are limited, the data does imply that the bureaucratic style of politics in the postindependence period, during which opposition and open political competition have been progressively more limited by the regime, has elicited a significantly more active response from the wealthy and the educated than from the poor. Social class is also related in the expected direction to perception of improvement in living conditions since independence, thus the survey data here bear out what has been documented about Kenyan politics in other studies (including Ross's own study of a Nairobi squatter community). Unfortunately, because of limitations in time and scope, Ross' material is silent on the patterns of polarization and even class tension that had become prominent among the Kikuyu people before independence, and have become increasingly important in recent years. To extend the general line of argument suggested by the approach to participation in this book, what we clearly need now are more specific studies of both the implementation and distributional impact of government policy, in order to understand the modes of elite manipulation of the political system as it is presently structured.

In general, this is a careful and imaginative study. Its conclusions not only shed light on some important aspects of Kenyan politics today, but have relevance for other third-world countries at a similar stage of urbanization and industrialization.

RICHARD E. STREN

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Spain in the 1970s: Economics, Social Structure, Foreign Policy. Edited by William T. Salisbury and James D. Theberge. (New York: Praeger, 1976. Pp. xv + 187. $20.00.)

The essays in this useful volume were prepared for a mid-1973 conference on "Spain in the Seventies," jointly sponsored by the Center for Strategic and International Studies, Georgetown University, and the Institute of International Studies of the University of South Carolina. The only exceptions are specially-commissioned papers on church-state relations and the prospects for succession by Tad Szulc and Vincente R. Pilapil.

Expecting Generallismo Francisco Franco to die before the end of the decade, many of the writers gave their views as to the probable pace of change under King Juan Carlos, the octagenerian dictator's hand-picked successor. The consensus was that sporadic violence would not give way to revolution, that the country's rapid economic growth in the 1960s obviated a Marxist upheaval, and that despite evidence of severe tension "the average Spaniard appears to want peaceful change" (p. ix). These conclusions have been supported by events since Franco's demise on November 20, 1975; for example, in late 1976 over 90 percent of the voters in a national referendum endorsed the establishment of a parliamentary system.

Although marred by typographical errors and uneven in style, content, and scope, the selections provide information vital to our understanding both the Franco regime and the current transitional government. Coeditor William T. Salisbury reveals how trade, investment, tourism, and emigration have drawn this Iberian nation of 35 million irresistibly toward Western Europe, even though venemous, ideologically motivated opposition from trade unions and socialist parties has barred its membership in NATO and the common market. James G. Holland and Gregory A. Raymond, in a jargon-laden piece, show how geography, agreements with the U.S., bilateral military ties with France, and the Iberian Pact of Friendship and Nonaggression with Portugal have made Spain more important to the defense of the NATO area than at least half of the members of the alliance. Tad Szulc cogently demonstrates how the heterogeneous church, which termed Franco's uprising at the outset of the Civil War a theologically justified crusade, had become an "economic hostage of his government" (p. 67).

In the only essay that extensively uses quantitative material and survey research, Stanley G. Payne illuminates the impact of the dictatorship and economic development on the regional nationalism of the Basques and Catalans. Observing the new momentum that regional nationalism exhibits among small ethnic groups throughout Western Europe, he notes that modernization has not produced secularization in Catalonia and the Basque country. To the contrary, "[u]rbanization has sharpened cultural tensions rather than merely transforming them, and the formation of political parties [has] tended to reproduce rather than cut across regional differences" (p. 98). Modernization, he argues, has "led to a combination of feelings of superiority and exploitation that has undermined the concept of a Spanish nation-state" (p. 98). This tendency was evident in the 1976 referendum in which nearly half of the voters in the Basque provinces of Vizcaya and Guipuzcua stayed away from the polls, while 20,000 Basque industrial workers went on strike to protest the election.

The best-written essay is by Benjamin Welles, a former Madrid correspondent for *The New York Times*. In discussing Spanish-American relations, he points out that detente, withdrawal from Vietnam, the SALT talks, discussions on Mutual Balanced Force Reductions, and the quietness of Russia's Mediterranean fleet have tended to "diminish Spain's strategic value" to the U.S. (p. 149). These conclusions were valid for the early seventies; however, the April 1974 coup in Portugal, the surge of the Communist parties in France and Italy, and the tensions that grip Yugoslavia during Tito's twilight years have shaken the Mediterranean rim, enhancing Spain's importance to western security.

As an illustration of desultory American callousness toward Spain, Welles reveals how Franco's proud foreign minister, Fernando Castiella, managed to obtain an appointment with Lyndon B. Johnson to discuss a treaty with the U.S. in 1968. The interview was a disaster: "On entering the Oval Office, the tall, courtly Spaniard found President Johnson seated, a telephone under his ear, his feet on his desk, and his ankle-length socks exposing his shanks. Johnson continued his conversation for a quarter-hour. Then hanging up, he peered at Castiella and barked, 'What do you want?' " Not even Dean Rusk's southern-coated diplomacy could mollify the diplomat's outrage.

If a major goal of the book is to help readers understand Spain in the post-Franco seventies, the editors should have broadened the range of essays. Selections on the impact of the Civil War on contemporary affairs, Spanish political thought, the military, political groups (including labor organizations), and political socialization would have been invaluable. Moreover, Salisbury and Theberge should have included a map to show the regional diversity within Spain, as well as its strategic position at the western entrance of the Mediterranean, astride major air routes, and behind the Pyrenees—a possible last line of defense should Soviet tanks roll across the North German plain.

GEORGE W. GRAYSON

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Proletarians and African Capitalism: The Kenya Case, 1960–1972. By Richard Sandbrook. (New York: Cambridge University Press, 1975. Pp. 222. $23.50.)

"This is one of the few books on African labour," claim the publishers, "which places the workers and their unions within the broad context of an evolving political economy," in contrast to "similar studies" which "have usually focused either on union-government relations or on internal union politics." The first chapter certainly gives promise of fulfilling this claim. The aim of the study is stated as the assessment of the validity of Fanon's sketch of the nonrevolutionary, economistic role of trade unions in dependent former colonies. Kenya's recent economic history is briefly described, with emphasis on the high growth rate, increasing inequalities, individualistic ethic and heavy reliance on foreign capital. Rejecting Marxist class categories as "somewhat artificial" (p. 12) in the African context, Sandbrook characterizes the emergent social structure as that of a predominantly peasant society, "governed by an elite advocating a capitalist economic strategy, closely associated with an external estate, intertwined with local business interests, and deeply involved in clientelist politics" (p. 23). The stage is set, it would seem, for the dynamic, political-economic analysis of the unions in their relationship with the national bourgeoisie that we have been promised.

Yet what comes next is curiously disconnected from the neo-Marxist framework of the first chapter. Since gaining independence in 1963, the ruling elite has sought to control the working class through its unions, demanding both political subordination and economic responsibility. The political part of this policy has been successful, argues Sandbrook, but the economic part has been less so—and he seeks the explanation for this relative lack of success in "the exigencies of internal union politics." The extent to which union leaders are responsive to pressures from government and the nature of the internal conflicts within unions are seen to depend largely on a number of features of their membership. Thus, the more concentrated is a union's membership and the higher the level of interaction between members, the less advantage to incumbent leaders deriving from their control of communications channels. Again, cleavage within unions is unlikely to follow occupational rather than ethnic lines unless (1) a high proportion of top posts is held by minority persons, (2) there are clear-cut status boundaries reflected in the distribution of privileges and (3) there is a high level of interaction within strata. Moreover, "the more intense the personal insecurity of trade unionists, the greater the incentive for them to adopt militant and uncompromising stands in industrial relations" (p. 143). Therefore, by encouraging factional politics within unions, and thereby making union leaders feel less secure, individual political leaders have been undermining the government's policy of encouraging economic responsibility. This is undeniably a neat piece of analysis, but we do seem by now to be on methodological ground perhaps closer to the "similar studies" of the publishers' blurb than to "the broad context of an evolving political economy."

It is in any case unclear what is meant by the "militant economism" with which the unions are credited. Perhaps because of the relative detachment of the analysis from the process of structural change in the economy and the labor market, there is a danger of confusing union leaders' rhetoric with "militant and uncompromising stands in industrial relations" (p. 143). As Sandbrook's data show (p. 158), the number of work days lost through strikes fell from 745,800 in 1962 to 49,500 in 1970. Although leaders make "skilful use of the strike threat," nearly all such threats are withdrawn "at the last minute 'in order to give the Ministry of Labour a final chance to resolve the dispute'" (p. 158). And can union participation in two tripartite agreements with the government and employers, involving a temporary wage freeze, be seen as consistent with militant economism? Certainly, urban wages rose during the sixties (although not as fast, particularly in relation to profits, as is generally assumed), but it is by no means clear that union militancy was a cause of this increase. Indeed Sandbrook himself suspects (p. 165) that strong unions emerged "in firms which were best placed economically and most willing to pay higher wages" rather than that strong unions forced "wage rates up in firms they organized."

In the final chapter the author returns to the Fanonist model of his first page. He agrees with Fanon's contention that workers are not committed to a radical critique of the capitalist political economy. He disagrees, however, with the contention that a sharp line should be drawn between the urban proletariat and other social strata, such as the peasantry and the urban unemployed. This leads him to the somewhat apocalyptic conclusion (p. 191) that "if the small political class in Kenya continues to enrich itself while adopting a policy of wage restraint and union restriction, workers will feel increasingly deprived relative to this elite. Any violent industrial and political action in the future might thus put a small section of 'haves,' supported perhaps by urban unemployed, against a much smaller group of 'have-mores.' " This conclusion may well be correct, but it can hardly be said to have flowed from the intervening analysis. Not that the intervening analysis and data are uninteresting. They are a useful addition to the growing body of literature on Kenya's trade unions. But the hopes raised by chapter one of an analytical breakthrough are not fulfilled.

MARTIN GODFREY

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Parties and Party Systems: A Framework for Analysis, Volume I. By Giovanni Sartori. (New York: Cambridge University Press, 1976. Pp. xiii + 370. $32.50, cloth; $10.95, paper.)

We have waited a long time for Sartori's major work on political parties. Judging by the distinguished scholarly book at hand, the first of two projected volumes, the work has been well worth the wait. Sartori has refined and extended a book-draft circulated in 1967, and he has also recast an important essay published during the last decade. He now provides, in volume one, a full, careful, and penetrating analysis of party systems. This analysis follows several rigorous definitional chapters establishing the frame of reference for the whole work. Promised for the second volume are party types, organization, and functions, plus the relationship of party to other variables and to the political system. Plainly, much of the subject-matter remains for the later publication. Nevertheless the first volume itself is a most significant addition to the field of comparative politics, and it should quickly be recognized as the leading study of the number, the ideological positioning, and the competitive relations of parties.

Now at Stanford, but long an Italian professor with an admirable international reputation, Sartori has a scholarly scope well beyond Western Europe and the United States. He is not content to write about parties only in Western democratic nations. Rather Sartori wants to develop a framework for understanding parties generally and everywhere. So he takes into account the experience of many non-Western and non-democratic nations. It is true that he refrains from claiming world-wide applicability for his typology of party systems; specifically, he believes that the "fluid polities" of certain African states do not so far have parties fitting categories derived mainly from Western experience. But whether it is to include or exclude from the typology, Sartori's discussion displays a staggering range as well as a depth of scholarship. His knowledge of the parties literature is massive. Although many national specialists can probably find grounds for disagreement, readily discernible mistakes seem so few and so minor that it would be ungenerous even to note them here.

Worth more attention is a difficulty inherent in the broad scope of the book, or in any book that attempts to integrate American experience in a perspective derived primarily from European parliamentary politics. Americans may wonder how our system's development is comprehended when the author emphasizes the transition from responsible parliamentary government to party government (pp. 19–21). Or we may think that Sartori is somehow excluding a separation-of-powers regime when he measures party strength in terms of parliamentary seats (pp. 122–23). But the author is aware of the terminological difficulty. Thus when he distinguishes two-partism as a system in which each major party can have enough seats to govern alone (p. 186). He adds in a footnote (p. 211) that the argument must be rephrased and adjusted for the American-type presidential system. In this as in other respects, there should be little doubt that Sartori understands American politics even if he starts from a perspective different from our own. That perspective, incidentally, leads to a proposed change in specifically American terminology. Sartori would not treat the historical patterns of southern state politics as one-party cases. Unlike a genuinely sovereign nation that has only one party, our old "one-party" states really belong, he argues, to his "predominant party" category (pp. 83–87).

The last point suggests the rigor with which Sartori defines and classified so as accurately to embrace the many cases in his large universe. The result, for some readers, could well be impatience especially with the first four chapters. But one should persevere. The definitions of faction, fraction, and party are conceptually important, and particularly for Sartori's purposes. Moreover, he reveals, in the course of defining, something about those purposes. For instance, his emphasis on the expressive function of parties relates to his belief that a single party cannot adequately replace a repressed interparty competition. And his basic definition of party excludes political groups incapable of electing candidates to public office.

Sartori's classification and typology of party systems, well-polished revisions of earlier work, are at the center of the present volume. Chapters 5, 6, and 7 are especially crucial and impressive. The number of parties is indeed important in Sartori's approach, and he uses an exacting numerical criterion to obtain seven classes rather than the traditional three. But for his typology, in contrast to classification, he adds an ideological criterion. Hence Sartori distinguishes "moderate pluralism" from "polarized pluralism" not just because the latter may have even more parties than the former but principally because a polarized system has greater ideological distance between its relevant parties. Such distance means a kind of competition that, while consistent with the existence of a center party, is so centrifugal that antisystem parties at opposite extremes of the ideological spectrum tend to gain at the expense of a center party and, at the same time, to be incapable of coalescing with each other. In other words, having a center party does not mean having a center tendency in electoral competition. A two-party system or a moderate pluralist system, without a large center party, is likelier to have a center tendency than is a polarized pluralism (of which contemporary Italy is an evident example). Understandably, in light of recent history, Sartori is by no means optimistic about the capacity of a polarized pluralism to endure. Nor is he at all certain that antisystem parties, like the Communist, can be absorbed into the political order. From his exposition, it is apparent that Sartori regards a noncompetitive system as the probable successor to a broken-down polarized pluralism. This still leaves a rich array of possibilities even among the one-party systems that he has separated from hegemonic-party patterns (as well as from predominant-party patterns, where competition remains). Sartori argues that there are meaningful differences, based on an ideological criterion, among totalitarian, authoritarian, and pragmatic patterns.

Neither the richness of Sartori's materials nor the elegance of his thought are adequately displayed by these capsulated illustrations. They do, however, suggest Sartori's analytical method. In this volume, as in earlier work, he treats parties principally as independent variables. Indeed, in substantial portions of the book, he treats the *number* of parties, or the party system, as the independent variable (pp. 191–92, 286–90). As he says, "from the vantage point of a science of politics—in its difference from a sociological explanation or reduction of politics—the question is precisely how the superstructure reacts upon the substructures" (p. 291). There can be no doubt about the brilliance with which Sartori supplies answers to that question. His refined typology and his analysis of the consequences of the several party systems are major intellectual achievements. They are undiminished by the observation that Sartori does not here tell us why one system rather than another has developed in certain nations. For example, he concentrates on the impact of polarized pluralism once it exists, and not on an explanation of the national circumstances that might have produced that party system in Italy or elsewhere. Perhaps we can look to the second volume for such an explanation.

LEON D. EPSTEIN

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Portugal's Struggle for Liberty. By Mário Soares. Translated by Mary Gawsworth. (London: George Allen & Unwin, 1975. Pp. 313. $21.00.)

Mário Soares is the Portuguese socialist leader, politician, and lawyer by profession. This book is his autobiography, begun in 1968 on the former Portuguese African island of São Tomé, to which the Salazar regime had deported him, and which he completed in exile in France. The author's life spans the years of dictatorship since military intervention in 1926, and appropriately his story covers that period, with concentration on the years after the Second World War up to April 1972 when he vowed to return to his country.

Soares' account is significant for two reasons. First, it provides a self-portrait of the leader of the social democratic movement in Portugal today, his convictions and principles, his struggles, his successes. It does not cover his conspicuous activity in the period after April 1974 when progressive forces within Portugal's military overturned the old regime, allowing Soares to become Minister of Foreign Affairs when final arrangements for the independence of the Portuguese African colonies were set in motion. Second, its analysis of contemporary politics in Portugal is especially useful in light of the dearth of materials on the subject—the consequence of nearly a half century of censorship and repression of intellectual life under dictator Salazar and his successor, Marcelo Caetano. This analysis focuses, first, on the author's early formation and perspectives of Salazar; second, on his opposition activities against Salazar; and, third, on his involvement in the period after Salazar's death in 1969. Let us briefly review each of these periods.

Soares' formative period was shaped by having been brought up in a liberal and republican family (his mother was "anti-clerical and extremely brave" and his father was "a furious Republican in spite of his own Catholic background . . . for ever engaged in one conspiracy or another" (p. 21). Three men influenced him during his years in secondary school: Alvaro Salema, a philosopher and unorthodox Marxist; Alvaro Cunhal, who taught him geography and today leader of the Portuguese Communist Party; and Agostinho da Silva, the head of an important cultural movement. Their intense hostility for the dictatorship prepared Soares for his political activities while a student in the University of Lisbon. During the war years his obsession with fascism turned him toward Marxism. He joined a small Communist group at the university and became active in the early antifascist Communist-backed front groups.

The period of his active opposition to the regime began with early manifestations of political struggle, leading to confrontation in 1958 and ending with the colonial wars after 1961 and their dramatic impact upon Portuguese society. The end of the world war nearly brought the demise of Salazarism with an attempted civilian and military coup in August 1945. The abortive coup, the ensuing electoral campaigns, and opposition politics are vividly described in detail not easily found elsewhere. Soares provides these details as well as reflections on the efforts of democratic forces at home in the face of European and U.S. support for Salazar, for him a disillusioning experience. The participation of the opposition in the presidential elections of 1958 did not lead to their usual withdrawal, and the opposition candidate received a fourth of the vote. Soares describes the "years of hope" which followed: involving abortive coups in 1969, 1961 and 1972; an aroused Catholic opposition; the seizure by a former colonial officer, Henrique Galvão, of a Portuguese luxury liner; and the efforts of the exiled opposition to form a united front. Soares pieces together well the threads of this opposition. Then he turns to the colonial wars in Africa and describes the tensions produced by Portuguese casualties, military defeat, and disruption of the economy.

His conspicuous involvement in the opposition politics and eventual deportation to São Tomé and exile parallel the decline and fall of Salazar, then Caetano. Soares analyzes the effort after 1966 to organize the non-Communist left, including Catholics, into a powerful Socialist Party. Its program would be to destroy fascism, restore political democracy; replace the corporative system by a policy of social justice and planned development on behalf of the workers; and to terminate the hostilities in Africa and bring about decolonization. In the end, Soares elaborates on his socialism: social democracy is the only real cure for the ills of Portugal. Admitting his admiration for Fidel Castro's Cuba, Soares prefers "the Chilean pattern that seems to me feasible" (p. 293). His socialism is humanistic "with personal freedom, with the independence of the individual to manage his own affairs" (p. 295).

This account, however, provides no analysis of the contradictions inherent in Allende's fall in Chile, a half year before the Portuguese "revolution." Knowing Soares' analysis might help us understand later events in Portugal. For example, the downfall of Allende was partly attributable to the manipulations of the Central Intelligence Agency—the facts are confirmed in congressional hearings. A turn away from socialism and a consolidation of authority on behalf of Soares occurred in Portugal during late 1975. At the same time Soares, once an admirer of the Marxist MPLA in Angla (p. 189), turned against that movement and toward U.S.-backed anti-Marxist Angolan forces. Both Soares and the anti-Marxist forces were heavily backed by the CIA—facts also confirmed in congressional hearings. Such contradictions in thought and action are not fully evident in Soares' personal account. His account, however, will stand as a contribution to an understanding of the dynamics of Portuguese politics in the period 1945 to 1972.

RONALD H. CHILCOTE

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A Revolution Is Not a Dinner Party: A Feast of Images of the Maoist Transformation of China. By Richard H. Solomon with Talbott W. Huey. (Garden City, N.Y.: Anchor, Doubleday, 1975. Pp. 208. $9.95.)

People may be fooled by the extraordinary pictures, beautiful publishing job and lack of scholarly apparatus into thinking that Richard Solomon has dashed off a merely popular book aimed at a commercial audience. If so, they would miss the serious intent of *A Revolution Is Not a Dinner Party*.

Solomon tries to carry a step further the psychocultural relativism of his earlier *Mao's Revolution and the Chinese Political Culture*.

His hope is to comprehend the meaning of Chinese symbols (such as the alleged preoccupation of the Chinese with eating), and therefore Chinese politics, by first critically apprehending American cultural biases which prevent Americans from understanding these symbols as the Chinese do.

However commendable his goal, Solomon's method is insufficient to achieve his objectives. This is manifest in the fundamental differences between his two books. The first, created during the years of America's military intervention in Indochina against China's alleged aggression by proxy, explained how Mao's relation to Chinese politics helped feed this supposed Chinese aggressiveness. The new book, created in a more normal atmosphere of adversary friendship, explains merely why China is different in its "desperate struggle for economic security, political unity, and national defense" (p. 28).

Solomon's perception has changed from seeing the Chinese as aggressive to seeing them as concerned with security, and yet the items he would have us focus on, such as your neighborhood Chinese restaurant, have not changed. American popular political images of China, including Solomon's, are shaped (Solomon to the contrary not withstanding) more by the differences in policies between Dean Rusk and Henry Kissinger than by the continuities of American cultural perceptions. One day China was a unique menace. The next day—or so it seems—it was an ordinary state with many humane features, a society even worth allying with against common enemies. Solomon's method does not comprehend these political perceptions, which are worlds apart and most decisive; his method looks away from the power of politics to hone the lenses which supposedly project our political vision.

The problem is that Solomon can't reveal much about Chinese political culture because he identifies too closely with some dominant themes in western political culture. It was the general tendency of eighteenth-century Europeans to admire the world's largest continuous imperial state and to inquire into the political mechanisms, such as civil service bureaucracy, which facilitated this unique state form; but this admiration gave way in the nineteenth century to contempt for China. Solomon explains this change in attitude as a pendulum swing away from enthusiasm toward a more informed view. With his own work an instance of his own thesis about cultural blinders, Solomon cannot take seriously the evidence that he should be aware of that this change in perception, by which China suddenly became contemptible, was not unrelated to a commercially expansionist Europe worried about an outflow of precious metal to China. Europeans suddenly favored ideas which sanctioned the unforgivable imposition of opium on China against the laws and wishes of China's rulers. There was no pendulum swing to a more informed view. Whether it is the Opium War or the Kissinger-Nixon switch, Solomon will not investigate how much western politics leads us to pick and choose within the rich variety of Chinese society for the means to justify very different western political projects.

Solomon's criticism of western cultural blinders, while welcome, is not enough. It may be fun to believe that criticizing cultural myths yields political understanding—whatever the reality of interests, institutions and related ideologies. The result of this harmless delusion which blames the "faddish enthusiasm of the American people" (p. 179) for the policies of groups in power is a pleasant enough book, with some exceptionally revealing pictures, incisive quotes from the Chinese and provocative psychocultural ideas presented in a delightful, easy-to-digest manner. The book is a pleasure to read. It just isn't persuasive.

EDWARD FRIEDMAN

University of Wisconsin, Madison

Social Classes in Agrarian Societies. By Rodolfo Stavenhagen. (Garden City, N.Y.: Anchor, Doubleday, 1975. Pp. xiv + 266. $3.50.)

This book is concerned with the formation and development of social classes and class relations in the agrarian sectors of countries where such sectors account for a very large share of the population and of production. The first part of the book, and the epilogue present an attempt to link theoretically international dependence, social classes and stratification, underdevelopment and processes of change. Two chapters in the first part generalize about agrarian structure and changes in Africa and in Latin America. The second part (about 40 pages) presents a case study of commercial farming and class relations in the Ivory Coast, especially in Agni country. The third part presents a case study of interethnic and class relations in the Maya highlands of Chiapas (Mexico) and Guatemala. "An earlier version of this book, with more of the scholarly trappings required by academia, was presented as a doctoral dissertation in the University of Paris in 1965. It was first published in Mexico in 1969. The present Anchor volume is a revised version of that edition." The revisions have been minor (p. xiii). The book is based on research done before 1965; post-1965 material appears in the first chapter and in the epilogue, both 18 pages each. The book depends on secondary literature for empirical data.

Stavenhagen is a learned, subtle and versatile scholar. This book integrates widely scattered pieces of secondary research, chiefly anthropological, providing an intelligible and intelligent picture of agrarian structure and change, especially in Africa and Latin America. The chief emphasis is sociological. Though Stavenhagen includes a discussion of power and power relations in his analysis of social structure, he curtails political analysis. For example, after a fascinating discussion of class formation and interethnic conflict among the Agni, Stavenhagen briefly notes the growth of associations of producers to defend member interests; farmers withheld produce from the market to boost prices. He then drops the subject: "An analysis of the political and economic action of these associations and a study of the efforts on the part of governments in each of the countries to organize production and stabilize prices are beyond the limits of this study" (pp. 154–55).

The author does not always come to grips with some central topics of his work. At times he contradicts himself. For example, "the Indian and the ladino are both integrated within a single economic system, in a single society. It is for this reason that inter-ethnic relations, insofar as commercial activities are concerned, bear the characteristics of class relations" (p. 189); versus "an Indian trader or landowner receives discriminatory treatment from ladinos who are in a socioeconomic situation inferior to his own, while Indian day laborers tend to receive smaller wages than ladinos who are in the same position" (p. 213). Thus the relationship between ethnicity and social class in commercial transactions is unclear. Does class characterize commerce, or is commerce impregnated with ethnicity, too? Another problem is the source of capitalism in underdeveloped societies: "Capitalism in underdeveloped countries does not grow from internal development but is superimposed on previously existing structures" (p. 40); yet later in the same chapter the author notes that "the existence of a relatively important commercial bourgeoisie in Madagascar (a class absent in most of Black Africa) antedates colonization by the French" (pp. 48–49). Is Madagascar the exception? Or does one find cultural variations in the propensity to generate capitalism internally among underdeveloped countries? Despite problems, Stavenhagen's analysis, on the whole, helps us to understand the complexity of these agrarian structures and, particularly, the variety of sources and manifestations of social stratification and social class.

The most disappointing part of the book is the possible link between the book's themes and international capitalism. Though Stavenhagen concludes that "as long as the majority of the countries in these two continents ... keep to a capitalist development strategy, it is likely that the main problems of the rural populations will not be solved, but will, on the contrary, be aggravated" (p. 232), his book discussed neither development strategies nor noncapitalist paths. He asserts that "industrialization undoubtedly figures among the most important processes that alter traditional class structures and provoke the development of new social classes, but it does so within the general framework of dependent and underdeveloped capitalism" (p. 61). And yet, industrialization is possible without capitalism; dependence is possible without capitalism; and, if industrialization has such wide-ranging impact, it is unclear in what way the country would still be underdeveloped or what difference its continued dependence would make. The first chapter, summarizing the "dependencia" school, is unpersuasive. Why do some countries in the metropolitan center (Spain) shift to dependency, while some formerly dependent countries (the United States) shift to the center (p. 8)? Why confuse capital formation with the balance of payments, and argue that there is decapitalization in Latin America only on the basis of balance of payments data (p. 9)? What does it mean that "both the development of the central countries and the underdevelopment of the peripheral nations are part and parcel of the history of the same worldwide system?" Obviously they are part of the same world history; but can one show if one caused the other? When Stavenhagen's book analyzes empirical links between agrarian structures and international economic centers, it suggests that Karl Marx, and nor many of his followers, was right. Referring to the English bourgeois impact on India, Marx rhetorically asked: "did any revolutionary party ever originate agrarian revolutions like those in Bengal, in Madras and in Bombay?" (*New York Daily Tribune*, August 8, 1853). One comes away from Stavenhagen's book with the view that international capitalism has substantially transformed agrarian structures studied, that it has led to social revolutions without previous parallel in these areas in Marx's sense, and that much remains to be done. A prescription for further change is in order. Should we accept the logical inference from Stavenhagen's analysis (more capitalism to accelerate drastic change), or his faith (less capitalism, because it may no longer do the trick)? The question remains open.

JORGE I. DOMINGUEZ

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Popular Uprisings in the Philippines, 1840–1940. By David R. Sturtevant. (Ithaca, N.Y.: Cornell University Press, 1976. Pp. 317. $17.50.)

David Sturtevant, who has been writing on the Philippines for nearly two decades, combines in his long-awaited *Popular Uprisings* new material and analysis with several of his articles. Using some archive records from the Philippines and the United States, a few interviews, several newspapers, and numerous secondary sources, Sturtevant focuses on three aspects of rural unrest between 1840 and 1940: the origins of 11 peasant-based movements, the government's actions against them, and the assessments of those movements by Philipino, Spanish, and American officials and contemporary analysts.

Sturtevant argues that peasant discontent belongs to a heritage of spiritual dissent that continues today. The causes of discontent are "serious cultural tensions" growing from a "clash between customary and modern tendencies" or "the stress between what Robert Redfield called the little and great traditions" (p. 17). Villagers who joined the various millenarian and revitalization movements were asserting their little tradition's vision of what could be (and, some believed, had already once been) the good life against the increasingly depersonalized and alien great tradition represented by western religions, laws and governments. Late in the nineteenth century and during the twentieth, these groups strongly advocated independence from Spanish and American rule, believing that liberation would permit the revival of the moral order their religions espoused. "As participants in the little tradition, they sought the re-creation of the simple clarities inherent in the defunct moral order" (p. 41).

The government bargained with leaders of these groups, getting some to join the official bureaucracy, arresting adherents, and doing battle with them. Those who analyzed the movements following the climactic events of each one frequently tried to dismiss the adherents as fanatics. Only in the decade or two prior to 1940 did some influential government officials and, especially, newspaper editors link these uprisings to harsh conditions in the countryside.

The book is certainly a contribution to scholarship on the Philippines. But, as the author warns, it is "neither the first nor the last word" on unrest during this century or on the groups it discusses. Indeed, the book will spark controversy among scholars and provoke additional research.

One of the book's limitations is vagueness regarding causes of unrest and the peasant's objectives. Concepts such as "cultural tension" and "little versus great tradition" are helpful, but these terms are not elaborated upon. For example, the first three chapters on the "sources of stress" provide valuable background but lack specificity and depth for the particular groups discussed. The author's argument would have become much clearer had he, for each group, described the social, economic, political and other cultural features of the provinces where the Cofradia, Guardia de Honor, Makobola, Colorum, Santa Iglesia, and others thrived.

Such analyses may also resolve an apparent contradiction. Sturtevant sometimes suggests that villagers sought the material benefits enjoyed by the upper classes. This is at odds with the thesis that villagers wanted a moral order that would reject and rise above such materialism.

A second limitation is the book's concentration on "leaders" of these groups rather than on less well-known participants. It emphasizes the same "charismatic" characters that the Manila newspapers and government officials emphasized, saying little about why these figures were attractive, what people's grievances were, and what people did. The book reveals scarcely anything about what went on inside these groups. Some of the glimpses offered (as in the marvelous interview with Colorum spokesman Pedro Calosa in an appendix) suggests that leaders were not necessarily followed or actually leading. Too often the groups remain mysterious for both reader and author (e.g., p. 144).

One cause of this limitation is Sturtevant's heavy reliance on government reports. Reynaldo C. Itelo, who has recently studied several of these groups, says more about what gook place within them and offers an analysis that, while not completely opposite, differs from Sutrtevant's. Itelo uses sources indigenous to those movements (in addition to many to Sturtevant's sources) and consciously seeks to interpret the groups from the participants' perspective ("Pasion and the Interpretation of

Change in Tagalog Society: 1840–1912," Ph.D. dissertation, Cornell University, 1975).

A third problem is the author's view that people in such uprisings are "unrealistic." (Sometimes he describes them as "mad" or "utopian.") Being other-worldly and weak, they inevitably fail, he argues, because peasant revolutionaries "are anachronisms." They seek to destroy "the depersonalized and incomprehensible world that has emerged around them.... But they also yearn to build a social universe that is basically anarchistic in style and content. Such a milieu, of course, has no place in the contemporary world" (p. 264). This belief colors Sturtevant's interpretation and probably accounts for the book's occasional cynical tone.

This is a distressing attitude in a book promising to "restore a degree of humanity to generations of defiant rebels..." (p. 18). Beginning with the belief that these rebels are unrealistic and possibly irrational, Sturtevant precludes an understanding of these people in their own terms. He gains knowledge about them, but does not come to know them. He also leaves, implicitly at least, "realism" and "rationality" to the elites–the Philippine Constabulary officers who killed them, affluent government officials who dismissed them as fanatics, and patrician critics who tried to steer them away from "blind" and "otherworldly" responses to frustration toward "rational" and "secular" movements. The more I learn about rebels and those who counter them, the more uneasy I am with such distinctions and characterizations.

Similarly, I do not agree that rebels inevitably fail. I suspect people who participated–and continue to do so–in those popular uprisings had other purposes than Sturtevant's standards could evaluate. "No uprising fails," Sakdal member Salud Algabre told Sturtevant in 1966. "Each one is a step in the right direction" (p. 296). The author leaves to others the job of researching the meaning of such statements.

BENEDICT J. KERKVLIET

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The Embattled Colossus: Economic Nationalism and United States Investors in Latin America. By Robert H. Swansbrough. (Gainesville: University Presses of Florida, 1976. Pp. 251. $12.50.)

The thesis of the book is simply stated: the rise of economic nationalism in Latin America since World War II has coincided and collided with the overwhelming economic presence there of U.S. corporations. The result has been a wave of expropriations, nationalizations, and restrictive policies which have elicited from the U.S. both foreign assistance and new forms of economic coercion.

Swansbrough approaches the subject from the perspectives of the three major actors: Latin American governments, U.S. foreign investors, and the U.S. government. Latin Americans have never esteemed private enterprise to the same extent as North Americans, many of whom believe that U.S. prosperity is the direct result of the private enterprise system. Latins, for cultural reasons, have long considered business and commerce to be lower-status occupations; and for governmental and political reasons, business has played a smaller role in the economic development of their countries, in part, because the state has played a much larger role.

Starting from such different perspectives, Latin America and the United States have often failed to communicate their respective needs and goals. Thus, Latin America continually requested public assistance in the years following the Second World War, while until 1960 the U.S. responded that the most effective channel for economic development was private investment, and the best way Latins could encourage its transfer was by creating a hospitable investment climate. Every president since Truman and every presidential advisory committee since the Gordon Grey committee of 1950 repeated the point that private enterprise is an essential part of the development effort and necessarily the major component of the U.S. foreign assistance program. Swansbrough does a commendable job in summarizing these reports on development assistance and noting their relevance to the issue of foreign investment.

America's professed reliance on foreign investment as a supplement to public assistance was always greatest when public assistance was least; conversely, when aid was greatest, for example, in the early years of the Alliance for Progress (1961–63), U.S. business almost felt excluded from the massive public campaign to foment economic development. By 1964, however, with the arrival of Thomas Mann as Assistant Secretary of State, private investment again assumed its former importance and various mechanisms were devised to promote private investment by minimizing the political risk through investment insurance, and to protect it through the manipulation of foreign assistance. It was in this manner that the American presence in Latin America grew and became more confrontationist just when economic nationalism in Latin America demanded less dependence from the North American colossus and more room for self-assertion.

Well-organized and clearly written, the book provides a good summary of the causes and consequences of sharply divergent perceptions between the U.S. and Latin America of the benefits and costs of private foreign investment in an era of economic nationalism and declining American hegemony. The breadth of the study and the comprehensive description of U.S. policy on foreign assistance as it relates to private investment in Latin America from 1945 to 1971 make the book most useful for students seeking an introduction to the subject. With the exception of one chapter and one note, there is unfortunately little which is original, and thus it is of less use to the scholar with some familiarity with the subject.

The two exceptions warrant mention. A survey of the business members of the Council of the Americas in 1970 makes the important point that while complaining, U.S. businesses were adapting to these new pressures, in some ways more quickly and efficiently than the U.S. government which still maintains, despite the Overseas Private Investment Corporation and the Hickenlooper and Gonzalez amendments, that its role is strictly one of "arm's-length" in all investment disputes. The author also has a short concluding note reminding Americans of their own economic nationalism, which was vented primarily at Great Britain throughout American history until the end of World War I when the U.S. became the creditor in the relationship.

Swansbrough's policy prescriptions—calling for more joint ventures, cautious repeal of the Hickenlooper amendment, multilateralizing investment insurance, and more attention to the region—would not have improved the investment picture if his analysis, which suggests Latin America's overriding need to reduce its dependency on the "colossus," is correct. Even if one were to accept these liberal recommendations as capable of narrowing interests and perceptions, so well described by the author, their relevance has diminished in the five years since he completed his research. His framework and analysis, however, remain useful.

ROBERT A. PASTOR

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Christians and Jews in Germany: Religion, Politics, and Ideology in the Second Reich, 1870–1914. By Uriel Tal. Translated by Noah Jonathan Jacobs. (Ithaca, N.Y.: Cornell University Press, 1975. Pp. 359. $19.50.)

When it first appeared in Hebrew in 1969, *Christians and Jews in Germany* was greeted as a seminal contribution to the historiography of German-Jewish relations in the Second Reich. Its translation into English by Noah Jonathan Jacobs explains the reasons for that enthusiasm. Based on enormous work on archival materials including a wide range of previously neglected popular, as well as scholarly, sources, *Christians and Jews* presents a rigorously conceptualized and finely nuanced analysis of the interaction between several sections of Germany's gentile and Jewish communities. More specifically, it analyzes the intellectual interaction, for although he calls his approach the "social history of ideas," Tal is less interested than earlier commentators, such as Paul Massing, have been in exploring the social roots of antisemitism. Nor is he as concerned as Ismar Schorsch in *Jewish Reactions to German Anti-Semitism, 1870–1914* to spell out the political struggles of Jewish defense organizations, although he says much about political issues. Tal also chooses to ignore left-wing attitudes towards the Jewish question, which have recently been studied for the Weimar period by Donald Niewyk and Hans Helmuth Knütter.

Tal studies with consummate skill the interaction of three groups in German society with the Jews: liberal intellectuals, conservative theoreticians, and anti-Christian, racially motivated antisemites. One needs to stress the word "interaction" because Tal is especially successful in uncovering the reciprocal dynamics of these groups with the Jewish community (more precisely, with its liberal, assimilated segment).

Tal's treatment of conservative and anti-Christian antisemitism deepens and extends what previous historians have already told us. He demonstrates the hostility of the conservative Christian state idea to assimilated Jews whose modernism and rationalism were seen as threats to the traditional order. He also shows that the conservatives were far less hostile to Zionism, which openly acknowledged the nationalist, and therefore unassimilable, component in the Jewish identity. Tal carefully distinguishes the conservative position from that of the anti-Christian brand of racial antisemitism, which was far less socially acceptable during the Second Reich because of its potentially radical character. Tracing the origins of this more modern antisemitism to Feuerbach's critique of religion and the growing influence of pseudo-scientific doctrines of racism, Tal shows how this more dangerous strain came to a head in Houston Stewart Chamberlain's system, "a nodal point of forces that had been slowly gathering momentum and that culminated in the racial legislation of the Third Reich" (pp. 280–81).

Tal's account of these developments has much to recommend it, but his basic contribution is his discussion of the liberal-Jewish relationship. Tal begins by positing a dual objective for Germany's recently emancipated Jews: integration into German society and preservation of a distinct Jewish identity. Although initially favoring both of these goals, increasingly gentile liberals came to see them as incompatible. While rejecting the conservative call for a Christian *state,* they nonetheless supported the goal of a homogeneous national *community* based on Christian principles, with the result that Jewish separateness, like that of the Poles or other minority groups, became intolerable. The price for integration, as even friends of the Jews such as Theodor Mommsen made clear, was the abandonment of a distinct Jewish character. Not surprisingly, the earlier Jewish faith in liberalism was consequently severely strained, a strain which Tal shows emerging during the *Kulturkampf* and worsening during the theological debates over the "essence of Christianity and Judaism" provoked by Adolf van Harnack's lectures of 1899–1900. Among Jewish intellectuals, this meant a gradual turning-away from the ethical neo-Kantianism of Hermann Cohen, whose failure to provide a rational ground for Judaism led to a new interest in historical, philological, and even anthropological orientations. Although Tal does not go beyond 1914, it is arguable that the so-called Jewish Renaissance of the early Weimar years centering around Buber and Rosenzweig can be understood in the context of this disenchantment with liberalism.

By demonstrating the liberals' inability to accommodate a Jewish identity in their nonpluralist *Kulturstaat,* Tal undercuts the assumption of earlier historians such as Fritz Stern that anti-modern "illiberalism" was the source of Germany's subsequent nightmare. Tal also helps strengthen the controversial argument of Arthur Hertzberg concerning the Enlightenment's contribution to antisemitism. His own work in turn has been corroborated by the more recent treatment by Schorsch of the gentile-dominated *Verein zur Abwehr des Antisemitismus* and Reinhard Rürup's study of the emancipation in Baden.

The missing piece in the puzzle, of course, is the left, whose attitude towards the Jews derived in part from the same Feuerbachian critique of religion that fed the anti-Christian antisemites, and in part from the same homogenizing tendency that crippled the liberal intelligentsia. An additional source of left-wing antisemitism was the identification of Jews with capitalism, which was expressed in Marx's controversial essay on the 1840s (and was rife among French socialists in the Proudhonist and Fourierist traditions). Although these stimuli contributed a certain amount of leftist antisemitism or at least to indifference to its importance elsewhere, the SPD was among the most vocal defenders of the Jews, at least in the Weimar era, if Niewyk is correct. The role of the SPD during the Second Reich is still somewhat clouded. Whether it was as false a friend of the Jews as Tal has shown the liberals to be remains to be seen. Tal would put us even more in his debt by extending his excellent analysis to this sector of German society in future work.

MARTIN JAY

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L'Algérie politique: histoire et société. By Jean-Claude Vatin. (Paris: Presses de la Fondation Nationale des Sciences Politiques, 1974. Pp. 312. Price not given.)

L'Algérie politique: institutions et régime. By Jean Leca and Jean-Claude Vatin. (Paris: Presses de la Fondation Nationale des Sciences Politiques, 1975. Pp. 501. Price not given.)

Concluding the project they initiated over a decade ago as French political science professors in Algiers "to examine the conditions of the genesis and development of Algerian political institutions," the co-authored volume is a monument of scholarly enterprise on two fronts. It combines a shrewd and nearly exhaustive analysis of official Algerian political structures and ideological declarations with a critique, mostly in footnotes, of the arsenal of American political science concepts they employed to interpret the Algerian political system. The display of erudition on both fronts is remarkable, though the authors confess in their conclusion that "the own of Minerva has some difficulty taking flight at this day's end. . . . Missing is that indispensable theoretical apparatus, that conceptual framework, preferably presented in the form of a cross-tabulation or vaguely resembling an econometric curve if it does not take the bizarre shape of a three-stage missile or transistor network, without which no analysis can merit the label claimed everywhere of being 'scientific' " (p. 463). This fit of professional masochism, however, is not quite justified. Despite their eclecticism, the authors present a theoretically sophisticated functionalist analysis of Algerian politics.

Taken together, the two volumes, each headed by the same co-authored preface and introduction, in fact constitute a trilogy: History and Society; Institutions; and Regime and Society. Though Vatin's book stands in its own right as an imaginative "rereading" of colonial historians' and Algerian responses to the colonial situation, the authors' ultimate intention of intellectually reconstructing Algeria's political system might have been better served by combining the history and the lengthy, rather juridical description of Algerian institutions that comprises almost half of the second volume into the analysis of Regime and Society. Such a reordering might have pressed the authors to work back from their portrait of the Algerian political system presented at the very end of the second volume to a more explicit theoretical design. Analyzing the interplay between the "social fabrication" of politics and the "political fabrication" of society (I, p. 11; II, p. 3) might in turn have encouraged them to supplement their secondary and official primary source material with some field work.

Leca and Vatin are at their best in their exegesis of official Algerian ideology, the significance of which as "a necessary artifice for building a political system" (p. 248) is rightly stressed, even though the very conditions of intensive colonization and war that gave rise to the ideology also militated against the acceptance of new "imported" and "cosmopolitan" ideas. From the authors' functionalist perspective, the really "scientific" question about ideology concerns neither its logical consistency nor its content but rather "which ideology permits the best accumulation of political capital" (p. 247, n. 39). Thus the "success" of an ideology lies in its "reception by the social groups that are indispensable to the functioning of the regime" (p. 248). The authors avoid ex post facto explanations by distinguishing "coherent" from "incoherent" pronouncements strictly on the basis of content. For instance, it is incoherent, though not logically inconsistent, to state, "A is definitely black" and "I will do everything possible to make A white." While hypothesizing that coherence "depends on a certain congruence between the discourse of the articulators and their possibilities of practical action" (pp. 273–74), the authors were able to analyze independently increases in ideological coherence and the narrowing of the regime's "core combination" of indispensable support to the "functional groups" of industrialists and a professionalized army. The substantive interpretation, of course, is hardly original and may already in 1976 be in need of certain revisions in light of the regime's search, amid growing economic pressures, for broader social bases.

It does not follow, however, that "official ideological discourse and administrative practice are relatively congruent with the political attitudes of the society" (p. 327). The "populist" political culture, which the authors admit is not "equally shared by all Algerian strata . . . independently of their structural position and cultural past" (p. 288), would have had to be derived, if not from survey research, then from a sustained analysis of the cultural fragments discussed by Vatin in the first volume. Nor is it clear that the cleavages between linguistic arabophiles and francophiles, religious conservatives and liberals, and Marxists and state capitalists (or technocrats) are as cross-cutting, hence vulnerable to manipulation by the "administrative state," as Vatin and authors dependent on more or less official sources make it appear (pp. 298, 468). In another context, in fact, the authors suggest that the educational system may be reinforcing these cleavages (p. 369).

Neither in Vatin's book nor in the jointly authored discussion of Regime and Society are social forces adequately examined. Vatin, for all his suggestive insights into colonial dialectic (esp. pp. 225 ff.), frankly admits that his "rereading" of the secondary sources did not enable him to delineate "which strata were able to benefit during different time periods from alliances with France," much less explain the social dynamics of Algerian nationalism (p. 312). Leca and Vatin, while insisting on their perspective of contemporary Algeria's "administrative state" rather than the Marxist view of a "state bourgeoisie"—"the difference is not just one of terminology" (p. 4)—finally excuse themselves from any theoretical discussion of an Algerian bourgeoisie on the ground that "the operational weakness of available methods becomes flagrant once we are dealing with a broken society, fractured by more than a century of foreign domination" (p. 462). Perhaps a rhetorical expression has little content worth analyzing (is not "state bourgeoisie" a polemical concept denoting the high officials and presumably their families of any industrializing regime not controlled by the polemicist's party or faction?), but the concept of an "administrative state" requires more elaboration in light of the growing literature, not cited in this volume, on authoritarian systems. Given the party's "chronic convalescence" (p. 93), is the regime really as legitimate as the authors claim (p. 488), and if so, for whom? Who gains from the covert political bargaining that occurs, for lack of legitimate channels in the party or elsewhere, in the administrative apparatus of the state? To what extent have administrators (or their families) exploited their political influence for more durable economic resources, notably those spoils of independence, the *biens vacants*, which were so much more extensive in Algeria than in other newly independent countries? How might these processes relate to the conspicuous consumption—"a sort of permanent *potlatch*" (p. 437)—observed by at least one of the authors in Algiers? Outside the capital, if local officials elected to the communes and governorate councils are so dependent on the central administration—as "recruits of the regime" rather than the representatives of social forces—why did the National Economic and Social Council try (unsuccessfully) to neutralize their role in the agrarian reform? To what extent and by what mechanisms ("clientelism"?) does the "administrative state" deviate from the ideals of Algerian socialism and what may be the political consequences, amid predictable economic difficulties, of its efforts to "fabricate society"?

Despite their obvious mastery of the documents and political events of recent Algerian history, as well as of much of the recent comparative politics literature, the authors do not move beyond a "rereading" of the documents and other academic interpretations to answer such questions. In this sense their approach is more restrictive than the Anglo-Saxon elite analysis and Marxist class analysis they have so intelligently criticized. Yet their study was well worth the "rereading" and will be of interest to any student of ideology or third world politics.

CLEMENT HENRY MOORE

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The Politics of Decolonization: Kenya Europeans and the Land Issue, 1960–1965. By Gary Wasserman. (London: Cambridge University Press, 1976. Pp. v + 208. $22.50.)

The Politics of Decolonization: Kenya Europeans and the Land Issue 1960–1965 reflects themes developed in several earlier works on decolonization and/or post-independence development in Kenya. These earlier works include Bennett and Rosberg (Oxford, 1961), Harbeson (Northwestern, 1973), Rothchild (Oxford, 1973) and Leys (California, 1974). Collectively they portray: the active participation of European politicians in establishing the terms of independence, the uses of constitutional negotiations and electoral politics to frustrate the reemergence of Kenya nationalism after Mau Mau, the deployment of land resettlement schemes to defuse rural unrest in order to limit the resurgence of Kenya nationalism, the effects of the land resettlement schemes in entrenching rural African middle classes whose emergence had helped to spark Mau Mau, the accommodation of Kenya's new ruling African cadres within these strengthened rural middle classes, the participation of international finance in shaping these developments, and the resulting emergence of a Kenya exemplifying the benefits and burdens of dependency upon the neocolonial forces to which she became accommodated during the transfer of power. *The Politics of Decolonization* does not add to or subtract from the major conclusions of the earlier works. Somewhat ironically, by viewing the events of decolonization "through a European looking glass" (p. 3), the author states conclusions very similar to those of earlier authors less synpathetic to the European positions.

The major strength of *The Politics of Decolonization* is its painstaking and detailed portrayal of the activities of European politicians in Kenya based on extensive personal records, correspondence, and interviews. The author has recognized the importance of detailing the motives, activities, and perceptions of the European politicians, and his work is timely because it was undertaken while the principal actors were still close enough to the events in question to recall and talk about them vividly. Although at pains in the book to emphasize differences within the European community over goals, strategy, and tactics, the author nonetheless concludes that "the goals of the two groups ["liberals" and "conservatives"] were similar, although their priorities differed. The goals could be summed up as the safeguarding of (a) the colonial system and (b) European agricultural assets" (p. 166). He argues that the "liberals" gave priority to the first objective while safeguarding the second, the conservatives the reverse.

The strength of the book begets its principal weakness. The author cautions at the outset that "the reader should be consistently conscious of the subjectivity involved" in viewing the decolonization processes exclusively through European eyes. The book's major problem is that the author's own analyses frequently do not take into account this very subjectivity in the data. They sometimes extend to matters beyond the focus of the book and the limitations of the data base, creating the impression of objective evaluation of decolonization as a whole when the data yield only the subjective responses of one set of actors to the events that transpired. For example, the author quarrels with the argument that the settlement schemes were a response to European farmer pressure, preferring to view the process as typifying the removal of colonial authority and the entrenchment of an African landed elite, amidst struggles for power involving numerous diverse interests (p. 129). The trouble is that some European politicians and, especially, some key British officials in the retiring colonial administration were anxious to *perceive* the process in these terms.

Moreover, the whole story told by the book details precisely how deeply involved were the European parties in negotiating the settlement schemes; significantly, there is a paucity of evidence that the major African leaders influenced European or colonial officials' thinking during the negotiation and sealing of the "bargain." For a further example the author questions what African interests were and whether the settlement schemes served them. In the first place the "liberals" were very skeptical of the settlement schemes' value to Africans, though they helped to secure them, precisely because they perceived the matter as the author does. In the second place, African interests, like European interests, were often a matter of the subjective perceptions of the various parties themselves. By omission *The Politics of Decolonization* illustrates vividly that African perceptions of African interests were scarcely arbitrated on equal terms with European perceptions in arriving at what the author therefore misleadingly regards as a "bargain." The author himself concedes, rather, that Africans were considered the "objects" of the process (p. 129) and Leys (*Underdevelopment in Kenya: The Politics of NeoColonialism*) explains what this augured for the future of independent Kenya.

JOHN W. HARBESON

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Power, Politics and Progress: Social Change in Rural Peru. By William Foote Whyte and Giorgio Alberti. (New York and Amsterdam: Elsevier, 1976. Pp. xxii + 307. $15.00.)

This book describes a large-scale collaborative research project conducted by the Instituto de Estudios Peruanos and Cornell University. The project, begun in 1964, continued through the mid-1970s, involved a large team of Peruvian and North American scholars drawn from anthropology, sociology and other social science disciplines.

The goal of the book is to explain sociopolitical change in Peruvian peasant society by linking together a wide range of studies conducted at the micro- and macro-level. In particular, the researchers attempt to link survey research, anthropological community studies and historical data (both local and national).

Although not explicitly stated as such, the two central dependent variables in the book are conflict and cooperation. These became the central variables in the investigation because, early in the research effort, the team evolved the now well-known model of peasant society as analogous to a triangle without a base. This model depicts peasants as subordinates in a hierarchically structured society in which they are linked economically, politically and socially to powerful patrons, but not to one another; hence the triangle without a base. The researchers focused on ways to close the triangle (i.e., instances where peasants cooperate).

Initially the research team saw conflict and cooperation as two poles of a single continuum. Later, as evidence began to come in from the field it became clear that conflict and cooperation were not mutually exclusive. This caused the researchers to evolve a two-dimensional typology of peasant society in which four basic combinations of conflict and cooperation are possible (p. 235). The low conflict/high cooperation village is one which is moving toward progress, whereas the low conflict/low cooperation village is moving nowhere. The other two combinations are intermediate. High conflict and high cooperation are characteristic of villages which, despite divisions, are making progress although the benefits are not equally shared. High conflict/low cooperation villages are ones in which efforts are being made to achieve progress but conflicts are too deep to permit success. In chapter 19 each of the 12 villages studied is placed into the typology. Since data from comparable surveys is available for two time periods (1964 and 1969), comparisons are made between those communities which have shifted their position in the typology and those which have remained unchanged. The important conclusion drawn from the two-dimensional analysis is that, contrary to the researchers' initial expectations, levels of modernity are of little help in predicting where a given village falls on the continuum. Rather, the critical variable is found to be cleavages (i.e., divisions along lines of wealth, age, occupation, culture, neighborhood, religion and party).

The book is divided into two parts. The first (Chapters 1–17) provides historical and contemporary information on the three major research sites (Cuzco, Junín and the Chancay Valley). The remainder of the book (chapters 8–22) presents theoretical material and a discussion of the interdisciplinary, micro-macro level methodology used in the research, emphasizing the limitations of using survey data alone or anthropological data alone.

The book is valuable for the reader unfamiliar with research on rural Peru. However, much of what appears has already been published (as the authors point out) and presents little that is new to the peasant specialist. The most disappointing aspect of the book is the treatment given survey data. Although the authors repeatedly emphasize the utility of their time-series data, they analyze very little of it in this book. In the section on Cuzco, for example, only 11 pages are devoted to the survey data, and even fewer are used in the discussion of Junín. Moreover, the data analysis which does appear leaves much to be desired. For example, reference is made to "a comparative study carried out in 1968" (p. 93) but it is not clear if this study is based on the 1964 sample data or on a new sample taken in 1968. The tables presented for the 1968 study do not contain the wording of the questions used, nor do they give significance levels or a test of strength of relationship. While other tables in this section make explicit reference to the 1964 and 1969 samples and do present the questions used, significance and correlation coefficients are still omitted. At one point (p. 94) it is stated that the respondents in one area "participated more actively in politics" but the table which is referred to has no political participation data included on it other than an indication as to whether the respondent possesses a voting card.

It is difficult to determine how the conclusions that are reached are drawn from the data presented. The reader would like to see, for example, the tables which permit the authors to reject the influence of modernity on determining community conflict and cooperation, especially since the authors explicitly state that they began with the assumption that modernity would be the critical variable. Certainly this evidence would be invaluable given the hot debate which is now raging over the role of modernity (e.g., Alex Inkeles vs. Alejandro Portes). While, the present reviewer is highly sympathetic to Whyte and Alberti's rejection of the role of modernity and notions of the passive peasant, he finds a complete presentation and analysis of the survey data which would support their conclusions sorely lacking. The absence of an index further mars the final product.

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International Politics

The Procedure of the UN Security Council. By Sydney D. Bailey. (New York: Oxford University Press, 1975. Pp. xiv + 424. $26.00.)

With this volume Sydney D. Bailey continues his series of highly specialized studies of the General Assembly and Security Council of the United Nations. This is his second major work on the Security Council and in his preface he announces his intention "to complete the trilogy with a further volume which will deal with aspects of the Council's primary responsibility for peace-making and peace-keeping." The present study analyzes in depth all aspects of the composition and procedure of the Security Council except voting, which is the subject matter of his previous book.

The author excels in his attention to detail, his meticulous documentation, and his logical organization of diverse subject material. The text is generously interspersed with tables and charts, most of which are of considerable value in supplementing and summarizing the themes of the material. The appendices provide additional reference data of special relevance to the Security Council procedures. In short, the work is impressive as a thoroughly researched study by a competent and established scholar who has based his findings on a careful review of the literature and the official documentation concerning the Security Council. The claim on the book jacket that "no other book . . . deals [as] comprehensively with the constitution and procedure of the Security Council" is fully justified.

In spite of the author's inclusion of a moderate account of anecdotal material, the book will not be of interest to a wide public. Even among scholars with a general interest in international organizations, only those who are willing to wrestle with the intricacies of procedural rules and practices will read the book from cover to cover. The tediousness of detail is relieved when the author departs from his usual pattern of reviewing (mainly on the basis of official documentation) the evolution of the rules and practices concerning such subjects as the order of speakers or the adjournment of a meeting. To this reviewer the character sketches, with analysis of traits which contributed to their effectiveness, of all heads of missions of the permanent members (pp. 107–33) were especially delightful and enlightening. For the most part, the hope expressed on the book jacket that the book would "be read and enjoyed rather than kept on a shelf and referred to" will not be realized. Yet, a definitive work for reference by other scholars has a special value.

This reviewer approached the last chapter of the book entitled "New Charter, New Rules, or New National Policies?" with anticipation that the author might offer a prescription for transforming the Security Council into a dynamic instrument for contributing to world order. After casting aside Charter amendments as unpromising and rules changes as marginal, the author informs his readers that "there is no panacea, nor ever likely to be" (p. 308). He then suggests that we "work for a more effective Security Council through changes in national attitudes and national policies." Analysis of his ten suggestions for these adjustments reveals that, for the most part, they comprise exhortations of members to take seriously their Charter obligations. If, at first, this seems to be a weak response to a major international set of needs, we need only remind ourselves of the inherent weaknesses of international intergovernmental organizations and their peripheral role in the implementation of the foreign policies of a majority of states. Perhaps then, as an honest scholar, Sydney Bailey has provided the only answer available to him.

A. LEROY BENNETT

University of Delaware

The Interaction of Economics and Foreign Policy. Edited by Robert A. Bauer. (Charlottesville: University Press of Virginia, 1975. Pp. 154. $10.00, cloth; $2.95, paper.)

This book consists of a series of six lectures delivered at the Public Affairs Forum Conference at Kenyon College in the spring of 1974. It is designed to contribute to an understanding of the relationships between economics and foreign policy. This is a subject about which much has been written, but very often in such broad and general terms as to be less than maximally useful to the teacher and student of either subject.

It is always easy to assert that all of foreign policy is shaped by economic considerations; or, conversely, that economics is wholly in the service of politics or strategy. Yet, all of us who grapple with the difficult interplay between these two areas know that neither assertion can be supported. It is therefore essential to look much more closely at both fields and at the linkages between them. The essays in this book throw additional light on this complex subject.

Though some of these papers are more impressive and compelling than others, there is no truly weak link in this compendium of six essays. The concluding essay by an Austrian financier and former government official is perhaps least useful, because its scope is too broad and because it tends to be more hortatory than analytical.

The initial essay by Richard J. Tretheway, an economist at Kenyon, presents a theoretical overview of the relationships between international economics and politics. Its purpose is "to provide the reader with a theoretical framework for distinguishing those aspects of international relations that are economic from those that are political" (p. 1). It is clearly and concisely written, and while it does not—indeed cannot—fully achieve its stated purpose, it provides the outlines of a theoretical framework based primarily on neoclassical economic analysis. It is regrettable that other types of economic analysis, leading to different conclusions, could not also be presented.

Other essays deal with economic factors influencing American, Soviet, Japanese, and Western European policies—and at the same time, dealing with the policies of these countries or areas which impact upon their economies. All are thoroughly researched and well written. The essay on American foreign policy, written by John Karlik of the Joint Economic Committee, presents an overview of political and economic relations with Europe, Canada, Japan, the Soviet Union, China, and the developing countries. Its tone is sober, factual, and restrained. Karlik concludes that in the next decade, "economic considerations will have a more significant impact on the formulation of U.S. foreign policy than was the case in the 1950s and 1960s" (p. 46).

The essay dealing with the Soviet Union, written by John P. Hardt of the Congressional Research Service and George Washington University, poses the question whether Soviet commercial relations with the industrial West will reduce or enhance the likelihood of change in resource allocation policy and economic reform, and answers that question by predicting that, in the interest of maximum economic performance, the prospects of change in Soviet policy will be enhanced. Young C. Kim, a political scientist at George Washington University, discusses the interplay between Japanese economic and foreign policies. His analysis centers around two case studies. One of these, Japanese behavior during the aftermath of the 1973 oil crisis, is too brief to present anything more thoroughly covered elsewhere. The other case, dealing with Japanese-Soviet relations, contains a detailed account and analysis which goes into considerable depth. However, Kim's effort to link economic relationships specifically with the dispute over the status of two disputed islands (Etorofu and Kunashiri) does not succeed; the case would have been clearer and cleaner if that aspect of the relationship had been omitted altogether.

Finally, an essay by Edward L. Morse on "The New Europe: A Unified Bloc or Blocked Unity" is a first-rate piece of analysis, linking political and economic concerns, both on the domestic and the international levels. His compelling argument raises important questions and suggests interesting hypotheses. One is that cooperation is more likely when the cooperating governments pursue a set of divergent but compatible goals rather than similar goals. The European "malaise" since 1973 is attributed not only to the obvious causes such as the 1973 Middle East War and the 1971 devaluation of the dollar, but also to domestic factors in the major European countries–factors which would have come into play even without the more dramatic events in the outside world.

Overall, this is a valuable contribution to a topic increasingly important to scholars and policy makers. In the academic realm, the book will be particularly useful in courses related to the old and honored discipline of political economy, but it can also profitably be infused into more specific foreign policy courses.

Fred A. Sondermann

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War in the Next Decade. Edited by Roger A. Beaumont and Martin Edmonds. (Lexington: University Press of Kentucky, 1975. Pp. 217. $11.00.)

The title of this book of essays is misleading. "War in the Next Decade" suggests, first, that a dominant motif will be a general, global consideration of the phenomenon of war. Second, it implies a futuristic analysis of international violence. The title does not promise that we shall go to the year 2000, but it does suggest, at the very least 1984.

These themes exist but are subordinate. A minority of the chapters deal with war in the longer vista of the future: the introduction on "the horizons of war: problems of projection," the conclusion on "polemology: promises and a problem" and one chapter on "science, technology, and the future of warfare."

The major part of the book focuses on organizational aspects of national defense and security and civil-military relations, seen from an Anglo-American perspective. Separate chapters deal with "the emergent military: reserve forces: mobilization demand in modern war," "the future of the reserve officer training corps," "youthful officer retirement: matrix for political action," "strategic ideas and defense policy: the organizational nexus," "militia in the seventies: a conflict paradigm," "the military bureaucracy: a case study of a civilian contribution."

Present-day structures, processes, and practices receive a good deal of attention; when the future is considered it is generally immediate and short-range. "War in the Next Decade" will be useful as a general introduction to such subjects.

Francis A. Beer

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The West Bank—Is It Viable? By Vivian A. Bull. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. 170. $14.00.)

One has to confess a certain humility when considering the impact of time on public policy. In any given area of politics what might seem outrageous or improbable today could appear quite plausible several years hence. While political science has put a great deal of emphasis on individual decision making and organizational constraints in historical time, it is important to note the significance for politics of *active* time, what is called "timing." The incarnation or materialization of ideas through historical action has a long pedigree in political theory from Augustine to Hegel, Marx and Plehkanov.

Almost by definition, political aspiration is actively progressive, futuristic. Such aspiration aims to write new history, to locate itself on the margin of things in contemporary terms but also on the horizon as a nascent force with which, "in the fullness of time," ordinary politics must eventually reckon. Revolutionary movements, for example, belong more to the future than present, working to create now the conditions for their fulfillment to come. Given this close, interactive relationship in political aspiration between past, present and future, it is fair to inquire into the current "viability"—practicability or feasibility—of movements that aim to establish themselves in a more routinized politics in the years ahead, a current viability determined not only by what they are now but by what they hope to become. By an ironic quirk of fate, the most "successful" political revolutions are generally seen as those which have established practical, stable new societies.

The passage of time in the Middle East has brought an increasingly radical self-consciousness among Arabs who now live in Israel, who dwell in the territories occupied by Israel in June 1967, and who left the land called "Palestine" before or after Israel's independence. This gathering, collective consciousness is known as "Palestinianism," partially represented by the Palestine Liberation Organization (PLO) but also going beyond this rather pluralistic national movement. Time has also worked something of a change in the perceptions of those outside the Palestinian movement, bringing a growing recognition that some sort of justice must be accorded the claims of Palestinianism to geographical nationhood within the old Palestine region. Whereas but a few years ago even the idea of a transition toward a Palestinian national entity might have appeared audacious, especially if voiced with subtle approval by high American officials, this is less true today and this idea is even expressed by a number of Israelis (in that unique and confusing domestic melange, the politics of Israel).

Without overdoing my emphasis on time and timing, one can say that it is "high time" that Palestinian political hopes be measured, inside and outside that movement, against the economic viability of a geographical entity that might somehow encompass these aspirations. A good deal of discussion about such an entity has centered on the West Bank territory along the Jordan River and on the Gaza Strip, both areas now occupied and administered by Israel.

Vivian A. Bull's study, *The West Bank—Is It Viable?*, is the first scholarly effort to deal with the economic viability of the West Bank in the context of slow movement toward a peace settlement in the Middle East that would encompass the Palestinians' claims to geographical justice. She by no means accepts the Palestinian claim to statehood on the West Bank, transitory or otherwise, but does recommend establishing a "Palestinian region" through a settlement between Jordan and Israel that would tie the West Bank to both and yet, in a sense, to neither in any full jurisdictional way—"an intermediary between Israel and the Arab world" (p. 152). The region might also include the Gaza Strip and other areas, with Jerusalem a part of this solution but also considered on its own. "The regional solution need not involve statehood of an area but could specify a semi-independent status" (p. 148), Bull says, but she seems to leave the door open for full independence at some future date; she sees such complete independence impractical "*in the short run*" (p. 143). In her estimation, justice would be served by this arrangement, if not fully then at least practically. Bull rejects solutions for federation with Israel or Jordan, as well as complete independence, since she sees significant drawbacks for Palestinians to federated status with either nation.

Basing her discussion of West Bank economic viability largely on Israeli data, Bull notes important forward strides in agricultural and human resource development in the territory since Israel's victory in June 1967. While some might cavil about her sources, I do not think this is a tremendous handicap in her analysis since she does not always approve of Israel's policies. Besides, after 1967 the obvious economic interests of Israel in the West Bank would necessitate the collection of accurate statistical information and its distribution to interested persons and groups.

What is perhaps more important for both the future of the West Bank and for peace in the Middle East than sources or methodologies in this book (some attention is paid to development theory) is the care with which Bull traces an evolving integration of the West Bank and Gaza Strip into the economy of Israel. In general economic terms this has involved continuous adjustments in the structure of production, trade in goods, services, and production factors, transfers of the means of payment, and prices. For agriculture this integration has meant changes in crop specialization and increasing involvement in Israel's export business, among other things. Industry and labor have also felt this gradual melding process. And of great significance for an eventual peace settlement has been what one might call "integration with the homeland," whereby a chain of Israeli settlements and highway/road connections have been established in Judea and Samaria, finding their contemporary political significance in, among other things, the Allon political-military plan for adjustment of territorial claims on the West Bank in the event of a full peace.

Bull argues that this integration, while not an unmixed blessing for Arab residents in the West Bank territory, has advanced their pace of development and has tied them closely enough to Israel that it would be unfeasible to return to their economic status before June 1967. In turn, Israel has benefited from this evolving integration. This evolution has raised a major question, however, for peace in the Middle East. The author ends by speculating that economic necessities may create political realities. But what kind of realities?

Frequently, arguments for and against return of the West Bank by Israel are naively put in strictly political and military terms. Bull adds another dimension to our thinking—the economic. After reading her study, however, one is left with several questions about so-called "realities." If a certain integration with Israel is vital for West Bank economic development, how important is this economy to Israel? Can Israel afford to lose the West Bank? Should a peace settlement materialize, will possible security guarantees have to include an economic plan designed to substitute in Israel's case for what it might lose should parts of its evolving, integrated economy be accorded semi-independent status? In some ways the present economic relations between Israel and the West Bank could be maintained, but surely not in all arrangements currently to the advantage of the occupying power.

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The Dynamics of the Arms Race. Edited by David Carlton and Carlo Schaerf. (New York: Halsted, 1975. Pp. 244. Price not given.)

The Dynamics of the Arms Race is a collection of lectures given at the 1972 International School on Disarmament and Research on Conflicts (ISODARCO, for short). Oddly enough, this crucial information about the origins of the work does not appear anywhere in the book. A few vague references are made to "the course and the symposium" and a list of "course participants" is given but that is all. The course (and presumably the symposium too) took place somewhere in Padua, Italy—this is evident from the thanks given for hospitality to an impressive list of Paduans. Determining the date of the event requires more detective work—it turns out to be mid-1972. What then is ISODARCO? And why the shyness in telling the reader?

ISODARCO is a biennial summer school at which about 50 students and lecturers (surprisingly, in about equal numbers) gather to discuss subjects related to disarmament, conflict, peace research and peace education. There have so far been five schools—including one in 1974—all in Italy. The organizer of the enterprise is Carlo Schaerf, Professor of Physics at Rome University. Schaerf, who does this rather thankless task as a hobby, spends a great deal of his time raising money for the school, mostly (it appears) from Italian and American sources. The original impetus for the school came from the Italian Pugwash group of scientists—including in particular the famous physicist Professor Edoardo Amaldi—but if there is still a connection with Pugwash it is a very tenuous one. But one can still spot a fair number of familiar Pugwash names among the participants. This number is, however, dwindling.

ISODARCO—because it is truly international and able to attract, for example, Soviet and East European participation—is unique. And the task of schooling young people in disarmament and peace research is an important and urgent one. There are, after all, precious few students interested in disarmament—perhaps the most vital issue so far as the future of mankind is concerned. If properly managed ISODARCO could be a major source of supply of future specialists in the field. It is, therefore, regrettable that its relative effectiveness is decreasing just when the need is increasing. A major reason for an apparent decline is that, whereas a decade ago a large proportion of those outside government service interested in disarmament were part-time amateurs, many full-time professionals are now involved. Standards have consequently risen considerably. Advocacy has largely been replaced by objective research.

On current standards, many of the papers in *The Dynamics of the Arms Race* are very poor. The collection is a jumble of the amateurish and the professional—mostly the former. Official views are mixed confusingly with non-official, and opinion with analysis. Add to this fact that the book is appearing three years after the event and one does not get a very useful product. The papers on the strategic arms limitations talks, for example, have long since been well overtaken by events, as have the contributions on the conflicts in South Africa and the Middle East. Among the remainder, there is a useful, but very incomplete, analysis of the origin of multiple independently-targetable re-entry vehicles (MIRVs) by Herbert

York; and a discussion of some of the problems of negotiating a prohibition of chemical weapons—interesting because the author, Oleg Reutov, is a Soviet scientist.

According to York the important decisions concerning MIRVs were technologically determined. Strategic considerations played virtually no part. Nor did political ones. In other words, the weapons were developed simply because the technology was available and, once developed, strong pressures built up for their deployment even though there was no strategic need for the weapons. York could have added that the same could be said for almost all strategic weapons and many tactical ones. But it is valuable to have an instance in which the process is so convincingly demonstrable.

In a paper on the political aspects of disarmament, Hans J. Morgenthau differentiates strongly between conventional and nuclear disarmament. The conventional arms race is, he says, politically motivated whereas the nuclear arms race is technologically conditioned. He is optimistic about the possibility of nuclear disarmament and arms control and pessimistic about the possibility of conventional disarmament and arms control—at least for as long as the political issues which gave rise to the arms race in the first place remain unsettled. Many would argue, however, that very far-reaching nuclear disarmament was impossible without some conventional disarmament. And one wonders whether Morgenthau is still optimistic about nuclear disarmament and arms control after the very slow progress made in limiting strategic arms.

Thanks to the sheer persistence, hard work and enthusiasm of Carlo Shaerf, ISODARCO is now well established. But the school must mature if it is to survive as a serious institution. One obvious step would be to limit the range of subjects covered. The 1972 school attempted to deal with the political and technological aspects of the nuclear arms race, various disarmament and arms control issues, chemical and biological warfare, the theory of conflict and some regional case studies. Inevitably, the treatment was very superficial. *The Dynamics of the Arms Race* shows this only too clearly.

We are now at a crucial stage in the arms race. The Soviet-American nuclear arms race will enter a very unstable phase unless military technology is soon controlled. And the proliferation of nuclear weapons to more countries is likely. The international trade in arms is spreading the most sophisticated weapons worldwide. Disarmament and arms control negotiations are getting nowhere. There is certainly no shortage of critical issues facing today's decision makers. And there should, therefore, be no difficulty about arranging the curriculum for the summer school centered on a relatively small number of topical issues which could be treated in some depth. Lecturers should be invited solely on the basis of their expertise in the topics chosen. If this is done, future proceedings of the school may become essential reading for those interested in disarmament issues. Unfortunately one cannot say as much for this volume.

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The Law of the Sea: Caracas and Beyond. Edited by Francis T. Christy, Jr., Thomas A. Clingan, Jr., John King Gamble, Jr., H. Gary Knight, and Edward Miles. (Cambridge, Mass.: Ballinger, 1975. Pp. 422. $15.00.)

There is no doubt that any person knowledgeable in the field of international law would agree that a proposal for a comprehensive new Law of the Sea (LOS) is desperately needed to bring order to an increasingly chaotic legal regime. Considering the apparent unqualified success of the 1958 conventions of the legal regime of the sea, it is a surprise to find such intense current pressure for change in the LOS. Even a decade ago various commentators suggested that revisions to the conventions were required only in the areas of setting a uniform width to the territorial sea between three and twelve miles and the need to resolve the issue of the maximum extent of exclusive coastal state fishing rights. These concerns are now only two of some 92 items currently being negotiated by 137 states with differing interests.

This volume evolved from the ninth annual conference of the Law of the Sea Institute in which Christy et al. have attempted to address the diversity of interests which presently make solutions by negotiation so difficult. The work is very comprehensive because it deals with most of the issues of the LOS, at least in the abstract. A major problem of this volume is that because the book is comprised of the proceedings of the conference, the quality of various contributors who gave papers at the conference and the dynamics of the panel discussions lose impact. For example, John Norton Moore, the Chairman of the U.S. Interagency Task Force on the Law of the Sea has an excellent commentary on the course of the negotiations at Caracas. He clearly sets forth the current problems of the ocean regime, and then succinctly outlines the principal issues dealt with at Caracas and provides a realistic appraisal of the forces involved pressing for one solution over another.

Jozef Goldblat's chapter on "Law of the Sea and the Security of Coastal States" is perhaps the most thought-provoking of the entire volume. Goldblat's view is that the main question facing the world community is no longer how to preserve the freedom of the seas, but how to restrict it to the mutual advantage of all. Thus, a new legal regime might be not only more complete but also more comprehensive. Those present at Caracas and at Geneva were engaged in controversies about the extent of future restrictions on traditional freedoms and not whether there was a need for them. Although many observers feel that military considerations have an excessive influence on the negotiations, the stark reality is that any final document must satisfy the major navies of the world before their respective governments will sign any treaty on the legal regime of the oceans. As a result, the fundamental concern when negotiating on articles such as pollution control is how these requirements will affect military security. Or, stated conversely, pollution controls are acceptable if they do not, in the view of the respective navies involved, significantly curtail their freedom of action.

One of the major influences on the negotiations has been concern over economic resources. The realities of scarcity and inflation have changed the views of many developed states. All now are concerned with the need to provide exclusive economic zones. Once again there are competing interests from landlocked and other "economically disadvantaged" states as to what is to be divided, and what is to be left for the "common heritage" of mankind and how will the latter be administered.

My major criticism of this volume is that its editors, apparently in an effort to get the book out before the session of the LOS conference in Geneva (March and April, 1975), did not edit out extraneous comments of various speakers, or irrelevant and redundant questions from the floor. This has the effect, on occasion, of raising triviality to an unwarranted level of respect. More careful editing of the book would have significantly reduced its length and greatly improved its quality.

For persons whose interest is of a general nature, this book would not be considered essential reading, since the Geneva session (of LOS) has moved considerably beyond the Caracas session. The volume does have definite value however, for those persons who are specialists in the law of the sea.

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The United States, China, and Arms Control. By Ralph N. Clough, A. Doak Barnett, Morton H. Halperin and Jerome H. Kahan, with Alton H. Quanbeck and Barry M. Blechman. (Washington, D.C.: The Brookings Institution, 1975. Pp. 153. $8.95, cloth; $2.95, paper.)

Most of the literature on the attitudes of the People's Republic of China toward the use and control of nuclear weapons was produced during the 1960s. Now, according to the authors of this policy-oriented study, a fresh assessment is necessitated by the dramatic change in the international environment in the last five to ten years. Clough et al. examine how Chinese leaders view this new international environment, predict the ways they are likely to view it in years to come, discern the implications of China's growing nuclear capability for U.S. security, and, on that basis, make policy recommendations for the U.S.

Concluding that the Chinese are not likely to involve themselves in serious arms control discussions in the near future, the authors still spell out areas of potential interest to the Chinese. These take the form of four arms control packages: (1) technical measures, which would provide means of preventing accidents or unauthorized launchings; (2) tension-reducing measures, which would be designed to alleviate Chinese fears of a U.S. nuclear first strike; (3) regional stability measures, which would stabilize volatile areas of interest to both sides; and (4) efforts to attract Chinese participation, whether formal or informal, in ongoing arms control forums and existing arms control arrangements. A U.S. no-first-use pledge and a proposal for arms control arrangements in Korea are then examined in depth as to the areas in which China and the U.S. are likely to find the greatest success.

These policy recommendations, and indeed the study as a whole, suffer from two sets of incorrect assumptions.

The first, those concerning the PRC, recalls the image of China employed by analysts in the middle and late 1960s. These scholars, most notably those at the Harvard East Asian Research Center, the Harvard Center for International Studies, and the Hoover Institute, concluded that China, once it had acquired nuclear status, would somehow conform to more "conventional" patterns of behavior than in the past. China would come to understand that the possession of nuclear weapons necessitated such Western strategic concepts as "deterrence," "mutual assured destruction," and "assured second-strike capabilities," and would recognize that bellicosity and intransigence were not consistent with its role as a nuclear-weapons state. It was assumed that these changing Chinese perceptions would be reflected in a growing body of Chinese statements on military and strategic policy. These expectations have not been fulfilled, however, and Chinese thinking on military/strategic matters in general and nuclear weapons/arms control issues in particular still remains largely a mystery. Yet, despite China's clear determination to go its own way, the authors of this study make little effort to place Chinese attitudes toward nuclear weapons within the context of what is known about Chinese foreign policy and their world view.

The authors seem to believe that either strategic parity or an "assured second-strike capability" would be both a necessary and sufficient condition for Chinese participation in international arms control measures. This error arises out of the assumption that the Chinese, in their foreign policy, seek only to maximize national security, defined in narrow military terms. While national security is certainly a major goal, there exists another objective, that of promoting China's revolutionary and developmental paradigms, to the extent applicable, in the Third World. This additional objective makes Chinese attitudes toward nuclear weapons resistant to traditional western modes of analysis. Because China does not desire to alienate the bulk of the Third World, its policies toward the use and control of nuclear weapons are dictated not only by its national security concerns, but also by the effects of those policies on less developed countries.

Thus, it may be true, as the authors state, that the PRC will certainly not participate in arms control arrangements as long as it remains "so far inferior to the U.S. and the USSR in nuclear weapons" (p. 62). But this does not mean, however, that they *will* participate once they overcome their inferiority. For if the Chinese continue to believe that the two superpowers collude and compete for world-wide hegemony, particularly in the Third World, and if they believe that arms control agreements, particularly those which seek to prevent smaller powers from acquiring nuclear weapons for self-defense, contribute to these grand designs, then the Chinese will probably continue to boycott such agreements, regardless of their strategic position vis-à-vis the superpowers.

The second set of erroneous assumptions on which the authors proceed concerns U.S. strategic policy and Chinese perceptions of that policy. For example, they advocate that the U.S. refrain from deploying weapons systems which would limit a Chinese second-strike capability (p. 77). In so doing, they are recommending a policy which would seem to contradict the "Schlesinger counterforce doctrine" which has gradually been adopted by the U.S. over the past few years, and which requires the capability to destroy Soviet military targets (including missile silos) in a tit-for-tat exchange. Because it is virtually impossible for the Chinese to distinguish between counterforce weapons aimed at Soviet targets and those destined for Chinese targets, the U.S., in implementing the approach envisaged by the authors, would have to discard a major targeting doctrine deemed necessary in its relationship with the Soviet Union, a relationship of much greater strategic importance.

In addition, the authors recommend that the U.S. express the view that the value of nuclear weapons is primarily, if not entirely, to deter nuclear attacks (p. 77). Again, they ignore the fact that the U.S. clearly relies on nuclear weapons to deter conventional attacks on Western Europe. Not only would the U.S. be forced to abandon this policy in order to make the above assurance to the PRC, but such a move would fly directly in the fact of Chinese desires for a strong U.S. deterrent to possible Soviet aggression in Europe. Finally, Soviet-American arms control agreements which set quantitative limits far above what is required for mere nuclear deterrence would probably be perceived by the Chinese as a contradiction of such a U.S. position.

As one reads *The U.S., China and Arms Control*, one cannot help thinking that the authors are describing the U.S. and China as they would prefer to see them, rather than as they really are. China is still analyzed on western terms, rather than on its own, and many current U.S. policies are largely ignored in the authors' efforts to advocate a U.S.-China dialogue on arms control issues. It is a disappointing effort on the part of four of the finest scholars in the field.

FRANK HAWKE

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Expropriation Politics. By Jessica Pernitz Einhorn. (Lexington, Mass.: Lexington Books, D.C. Heath, 1974. Pp. x + 148. $13.50.)

Jessica Einhorn's *Expropriation Politics* sheds light on two actions during President Richard Nixon's administration aimed at foreign countries which expropriate U.S. interests without compensation. The first case study examines why the administration deferred invoking legislative sanctions against the Peruvian revolutionary government after the 1969 seizure of International Petroleum Corporation. The second explores why the United States shifted its policy stance in 1972 to declare the "presumption" that future instances of nations expropriating U.S. investments without compensation will face the coercive economic policy tools avoided in 1969. The book applies the bureaucratic politics model developed by Graham Allison and Morton Halperin to explain these policy shifts.

Einhorn presents a wealth of detail, based largely on 55 interviews with unidentified executive branch officials, about the political process within the bureaucracies of the Departments of State and Treasury and the National Security Council. In this well-written book the author sets out to test on international economic issues the model Halperin used to analyze national policy in *Bureaucratic Politics and Foreign Policy*. The phrase "where you sit is where you stand," describes the book's core concept, which asserts that in inter-departmental struggles bureaucrats take positions on issues based upon the organizational norms of their department.

The author discusses in detail the legal complexities and political sensitivities surrounding the Peruvian junta's October 9, 1968 nationalization of the La Brea Y and Pariñas oil complex of I.P.C. She makes no judgment about the dispute, concentrating instead on describing the spectrum of opinion within the U.S. government as to who was responsible for handling the problem and how to deal with it. While noting that Embassy and State Department officials relied on I.P.C. representatives for facts and legal interpretations, Einhorn makes no effort to determine from those interviewed why the U.S. government did not make an independent study of this thorny issue.

By describing the shared images held by U.S. policy makers, Einhorn highlights the belief that Washington must respond to a foreign expropriation without compensation. The State Department and its N.S.C. allies avoided a major confrontation with the Peruvian government because neither the Congress nor the interested business community desired to impose legislative sanctions. The exclusion of the Treasury Department from the bureaucratic process facilitated the decisions to defer the application of the Hickenlooper Amendment. The diplomatic mission of John Irwin served as the "fig leaf" to explain why the sanctions were ignored. The choice to apply covert economic pressures against the nationalistic generals made the dovish approach more palatable to the president and State Department hardliners, identified as individuals who had been in Lima or were in the Peruvian office at the time of the expropriation.

The author's discussion of the State Department's rationale behind these "skillful, innovative pressure" tools (p. 59) proves most informative. It should be remembered that American diplomats flatly denied Peru's condemnations of U.S. "economic aggression" in the Organization of American States, a course of action dictated by the covert policy which requires no public admission of the pressures. One wonders, however, why the bureaucrats involved failed to recall how these same covert pressures, when employed against the democratic regime of Fernando Belaunde Terry, were unsuccessful in bringing about a favorable settlement of the I.P.C. dispute. Instead, these coercive economic tools brought the Peruvian generals to power, and they promptly expropriated I.P.C. The use of the bureaucratic model did not explain why bureaucrats seemed not to learn from past mistakes.

Secretary of the Treasury John Connally appeared as the key actor in the second case study. Connally's influence with President Nixon meant that State and the N.S.C. would be unable to ignore Connally's call for a firm U.S. policy against expropriation without compensation or to exclude Treasury from participating in the drafting of such a policy. A firm policy against nations which expropriate U.S. interests appeared foreordained, but the author points out how State's bureaucrats cut their losses by obtaining critical compromises in the language of the presumptive policy statement. Furthermore, State managed to gain control over the implementation of the presumptive policy, again with the cooperation of White House staff allies who resented Treasury's incursions into foreign policy.

Neither the author nor the reader concludes that bureaucratic political analysis yields profound insights into foreign economic policy making. Policy advisors desiring to toughen U.S. economic policy should recommend that the president appoint a strong Treasury secretary and minimize State's role in investment disputes. Those wishing a softer solution to expropriation should keep Treasury out of the political process and recommend the rotation of Embassy and State desk officers after a "revolutionary" change of power.

The bureaucratic methodological approach downplays the dynamics, popularity, and intensity of economic nationalism, as revealed in the author's observation that "the I.P.C. expropriation was the Peruvian government's birthright" (p. 45). Latin Americans gain little insight into how the shared images of U.S. bureaucrats are formed or how they can be modified to take into consideration the hemisphere's view of foreign investment. The author did not examine the fundamental clash of perceptions on the role of private enterprise because she utilized the bureaucratic model. However, the formulation of a United States policy toward expropriation requires a thorough understanding of these conflicting images.

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The Vietnam War and International Law—Volume 4—The Concluding Phase. Edited by Richard A. Falk. (Princeton, N.J.: Princeton University Press, 1976. Pp. vii + 1051. $35.00, cloth; $13.50, paper.)

Publication of the fourth and final volume in the series *The Vietnam War and International Law* brings to a close a unique contribution to the literature on the United States' role in the major political and military event of the past two decades. As the title indicates, the guiding assumption underlying the compilation of articles for this series is that in addition to the political and moral aspects of American actions in Vietnam, there are important legal questions as well. This is a position infrequently encountered in writings on the Vietnam conflict, which makes this compilation all the more valuable as a rare attempt to focus on some of the most fundamental unresolved issues of the war.

The earlier volumes concentrated on the origins of U.S. involvement in the Vietnam conflict, and the subsequent conduct of the war. This analysis is continued in the concluding volume to provide a context for the ultimate question of individual and collective responsibility for U.S. actions in Vietnam. A number of contributors attempt to delineate a standard of legal accountability for both the formulators and implementers of policy that stands in stark contrast to the efforts of the public officials involved to depict the Vietnam experience as a miscalculation and unique case, unlikely to be repeated, and devoid of any fundamental lessons for the future. The contrary position taken by editor Richard A. Falk and many of the contributors is that the U.S. actions regarding Vietnam were, worse than a blunder, a crime.

Volume 4 lists 28 contributors and is divided into eight major sections. Sections 1 through 5 are concerned with the role of international law and organization, the laws of war, war crimes and individual responsibility, the settlement of the Vietnam war, and the U.S. constitutional structure and war-making powers. Section 6 has a useful collection of documents related to the Paris Agreements (somewhat optimistically titled, "Settlement of the Indochina War") and section 7 contains "Some Principal Decisions by United States Courts and Legislative Action by the United States Congress on Indochina-Related Issues." A special supplement contains official statements of the various parties on the concluding phase of the war in Spring 1975, and reprints of contemporaneous analyses by Robert Tucker, Stanley Hoffmann and Richard Falk.

A central theme which integrates the wide-ranging contents of this volume is the connection between international law and U.S. domestic law and politics. John Norton Moore and Edwin Brown Firmage reject the "realist" assertion that there is an unresolvable contradiction between adherence to accepted norms of international behavior and "the national interest." As Firmage points out, the executive is not in the best position to be an exclusive impartial arbiter of either standards of international law or the national interest. As James Madison observed, "those who are to *conduct a war* cannot in the nature of things, be proper or safe judges, whether *a war ought to be commenced, continued or concluded*" (p. 37, emphasis in original). The wisdom of this observation is underlined by former Under Secretary of State Eugene Rostow's astounding statement that the 1973 Paris Agreement is "the same as the Korean settlement. . ." (p. 485). Perry L. Pickert analyzes American attitudes toward international law as reflected in *The Pentagon Papers* and concludes that, "the United States selected the part of the Geneva Accords which suited its purposes and protected those elements, while casting aside the other provisions" (p. 64). On the U.S. use of the SEATO treaty as an international legal justification for its Vietnam involvement, Pickert notes that "Since such treaties are drafted to have maximum domestic political effect and no international legal effect whatsoever, in order to keep American options open, American treaty commitments rest on the credulity of the audience rather than any legal obligations" (p. 69).

If the executive can have the latitude to choose the legal and moral norms which it will or will not respect, it is difficult to hold those entrusted with the actual conduct of hostilities to stricter account. L. C. Green explores the problem of the perception of legal accountability by the soldier and suggests replacing the concept of "manifest illegality or unlawfulness" with that of "obvious criminality" to place "the international law of war on the same level as ordinary criminal law" (p. 175). Presumably this might clarify the complexities of the accountability of soldiers for their actions and, if extended to the conduct of policy makers as well, might serve to define the inadmissible military conduct which Telford Taylor defines as involving "operational practices and tactics which *are* in accordance with directives laid down from the highest level and which are of dubious legality under the Hague and Geneva Conventions" (pp. 367–68, emphasis in original). The problem of enforcing a code of acceptable behavior in inter-domestic illegality has only begun to be grasped. The major contribution of this volume is its attempt to clarify and underscore this vital linkage.

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The International Court of Justice: An Analysis of a Failure. By John King Gamble, Jr. and Dana D. Fischer. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. v + 157. $15.00.)

The United Nations has been the focus of sharp attack from its inception, but rarely in the past has this criticism come from the academic community. In recent years, however, this pattern of scholarly defense has begun to alter in dramatic ways, and John Gamble and Dana Fischer's book is a further addition to the growing literature which argues that the United Nations system has been characterized by major failures in the past and has limited prospects for changing those patterns in the future. Gamble and Fischer's study is especially devastating because it focuses upon a particularly weak link in the institutional arrangements of the UN system, the International Court of Justice.

Gamble and Fischer begin with a review of a substantial body of literature on the activities of the ICJ, concluding that much of the literature is "pedantic, too narrowly focused, and of little practical use" (p. 28) because the research has had a pro-court bias, has failed to place the role of the court in a realistic political perspective, and has been based upon unsystematic research methods. The authors set out to remedy these shortcomings by taking an analytical, objective approach to studying the court, systematically gathering and analyzing data on the activities of the court from 1946 to August of 1975 for the purposes of discerning "what the Court has done, how countries relate to it, and ultimately what role it can be expected to play in the future" (p. 5).

The initial concern of the empirical research is to survey the work of the court, with primary emphasis given to an analysis of two criteria for evaluating its work: (1) the judgments of the court and their effect upon the resolution of international disputes and (2) the contribution to the development of international law and the effect on the international system of potential court action. Regarding the first criterion, the court's record is found to be far from impressive. The court has handled relatively few disputes, and its success ratio not only has been low but also limited to localized conflicts peripheral to major international issues. The court does not fare much better against the second criterion because of the small number of decisions it has made and its limited jurisdiction in expanding areas of international law, such as international economic relations, international administration, and human rights.

Having surveyed the work of the court, the authors then shift their attention to an analysis of states' relationships to the court. As a data base, the authors collected information on 18 variables for 126 states. The first of two major questions posed by the authors in this section is, "How do states of the world support the ICJ?" The most important result of this analysis is that support for the court is clearly highest among Western European and English-speaking democracies; the Latin American countries have a middle level of support, but all other regional groups show only minimal support for the court. The second question—"How does support for the court relate to other characteristics?"—is analyzed through multivariate techniques, leading to the general conclusion that support for the court is most highly related to the national characteristics of high GNP per capita, lengthy service of national judges on the court, and association with large numbers of treaties with dispute settlement clauses.

As a final concern, Gamble and Fischer seek to develop a model to bring together the results of the earlier analyses and to offer predictions concerning the use of the court for contentious cases. The additive model, based upon eight independent variables which had been previously tested, is moderately successful in "postdicting" cases, but it is clear that the model does not have the power to account fully for the variables which determine the use of the court. In regard to prediction of future use of the court, the authors suggest that the model may have some utility but that further testing and refinement is necessary.

Overall, the book makes a valuable contribution to our understanding of the ICJ. It is an example of innovative behavioral research applied to a subject largely devoid of behavioral studies, and the authors are able to challenge "conventional wisdom" at several points. For example, they establish that the court is far from laggardly in disposing of cases and that acceptance of the optional clause of the court is not significantly related to actual appearances before the court. The authors are well aware of the major weaknesses of their study and acknowledge these when they discuss the methodological foundations of their research.

Although the book is well worth reading, one cannot help wondering who will pay much attention to the book except for journal reviewers. The book does not lend itself to typical classroom use because of its specialized focus and methodological sophistication. Furthermore, the relatively few scholars interested in the court generally are likely to be unreceptive to the book because of their bias against quantitatively oriented research. The book deserves a better fate than it is likely to receive.

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U.S. Power and the Multinational Corporation: The Political Economy of Foreign Direct Investment. By Robert Gilpin. (New York: Basic Books, 1975. Pp. xii + 291. $10.95.)

Too many books are saddled with pretentious titles that promise much more than the author intends or is able to deliver. But sometimes there occurs deception of a contrary kind. Gilpin's *U.S. Power and the Multinational Corporation* is much more than its title suggests. The book contains not only an analysis of U.S. foreign economic policy as it pertains to foreign direct investment and its principal allocator, the transnational corporation; but also it describes patterns of "reciprocal interaction" between international politics and international economics, and their relationship to important world outcomes. Thus the book has two main foci; one is specifically policy-oriented, whereas the other presents a conceptual framework for understanding the intersection of economics and politics in international relations. Is Gilpin successful regarding both these objectives? At the risk of sounding more like a movie critic than an *APSR* reviewer, let me directly state that I believe *U.S. Power and the Multinational Corporation* to be one of the most significant contributions to the international relations literature to appear in many years.

In the first few chapters, Gilpin provides the reader with an overview of the study of international political economy through descriptions and critical analyses of three "prevailing conceptions" of the nature of economic relations, actors and activity, and the relationship between economics and politics; the three conceptions are "liberalism," "Marxism," and "mercantilism." This rather concise survey prepares the way for Gilpin's development of his "own perspective on political economy" which, at least to this reader, incorporates mainly mercantilist thought (the subordination of economics to politics), but in an amended analytical version that also borrows from both liberalism and Marxism (especially in terminology through the generous use of such terms as "center and periphery"). The thrust of Gilpin's viewpoint is that there is a fundamental difference in emphasis between economics (which emphasizes, in its dominant liberal version, absolute gains where all participants may benefit, though perhaps to different degrees) and politics (which emphasizes relative gains where the distribution of value occurs among participants caught in a zero-sum game). Moreover, economics cannot be divorced from inter-state politics; in fact "the primary determinants of the role played by these (economic) actors are the larger configurations of power among nation-states" (p. 39). Thus, "the modern world economy has evolved through the emergence of great national economies that have successively become dominant" (p. 40). And finally, "every economic system rests on a particular political order, its nature cannot be understood aside from politics" (pp. 40–41).

What then accounts for change in the international system? That is, why do dominant powers decline and thereby allow for the possibility of the imposition of a new political-economic order? "Although nation-states, as mercantilists suggest, do seek to control economic and technological forces and channel them to their own advantage, this is impossible over the long run. The spread of economic growth and industrialization cannot be prevented. In time the diffusion of industry and technology undermines the position of the dominant power" (p. 42).

Gilpin subsequently illustrates his provocative thesis through two case studies; one is devoted to "the British strategy of portfolio investment" in the nineteenth century, while the other, and far more extended treatment, investigates the contemporary example of the "American strategy of direct investment," and its main instrument, the transnational corporation.

The concluding chapters are more public policy-oriented and examine the costs and benefits to the United States of the foreign direct investment (FDI) strategy it has evolved. It should be noted that Gilpin is virtually alone in directing attention to the importance of FDI to the home country (i.e., the United States). Nearly all other analysts have been so concerned with the effects of FDI on host countries (especially developing ones) that we almost have lost sight of the fact that the consequences of such international political-economic activity may be serious (and perhaps even more serious) for the home country of the majority of these great business enterprises. In this vein, Gilpin's evaluation is that on balance the strategy of FDI "has had a detrimental effect on the economic and industrial development of the United States" and that its acceleration "sometime in the 1950's or early 1960's . . . became decreasingly a sign of industrial strength and increasingly one of relative industrial decline" (p. 189).

Taking a position similar to but not the same as the Burke-Hartke sentiment that recently characterized AFL-CIO thinking on the subject, Gilpin concludes that the presently dominant state economy has followed an increasingly dangerous course. "What the United States has been doing is exporting or trading away its comparative advantages (technology, technical know-how, and management) and potential productivity gains in exchange for future foreign earnings" (p. 198). The analysis ends with Gilpin advocating a drastic reform of "the American industrial structure, capital market, and taxation system in order to channel new productive capacity and resources into the underinvested sector of the American economy and society. This solution would mean a national industrial strategy which emphasized increased domestic competition and the development of new technologies and industrial processes rather than defensive investment abroad" (p. 206). "The American national interest dictates an emphasis on trade and the reinvigoration of the American economy rather than on foreign direct investment" (p. 214).

Gilpin unfortunately does not go on to describe the nature and content of this drastically reformed American political-economic strategy, nor does he indicate how this is to be adopted (as with so many other advocates, the question of the political feasibility of what should be done is not explicitly addressed). There are of course other weaknesses in this otherwise excellent and interesting work. By way of example, Gilpin often speaks of "dominant economies" and of "configurations of power" but, perhaps because of a bias regarding quantification, never indicates how judgments are to be made here (i.e., how do we in fact know when Britain was dominant and, later, in decline?). If the rate of portfolio investment is suggested, then we are at a loss as to whether this is to be treated as an indicator of dominance or its instrument or its effect. Since Gilpin makes so much of the political context within which economics takes place, it is crucial that it be analytically and operationally distinguished from data pertaining to economic exchange. Another problem concerns his various depictions of the relationship between U.S. government (and national interest) and business leaders (and corporate interest). At times Gilpin appears to argue that they "share an overlapping and complementary set of interests," but he also follows this by explicitly distinguishing the two groups and documenting divisions between them (pp. 141–44). The relationship, whichever it is, should be clarified since it has profound implications for the accomplishment of the reform that Gilpin so strongly recommends.

BERNARD MENNIS

Temple University

The League of Arab States and Regional Disputes: A Study of Middle East Conflicts. By Hussein A. Hassouna. (Dobbs Ferry, N.Y.: Oceana, 1975. Co-published with A. W. Sijthoff, Leiden, The Netherlands. Pp. xxi + 512. $25.00.)

In the past decade or so scholars have paid increasing attention to the phenomenon of regional organization. One particular area of interest has been the role of regional organizations as instruments for the management of local conflicts. Among such organizations, attention has tended to focus on the OAS and OAU while the Arab League has been relatively neglected, in part because of linguistic barriers and in part because of the greater secretiveness of Arab authorities.

Previous studies have dealt almost entirely with the League's institutional structure and legal framework. Very little attention has been paid to the actual performance of League machinery, particularly in the crucial area of conflict management. The present book is a serious attempt to fill this gap. The author, Hussein Hassouna, was particularly well placed to carry out such a study. As the son of longtime Arab League Secretary-General Abdel Khalek Hassouna (1952–1972), he was able to gain access not merely to Arab League documents frequently unavailable to western scholars but also to invaluable information and insights through his father.

Hassouna seeks to analyze the conflict-related activities of the League in two spheres—the management of conflicts within the Arab world (its internal, regulatory function) and the defence of Arab interests in conflicts with outsiders (its external, alliance function). In the latter sphere, he concentrates almost entirely on the Arab-Israeli conflict. This is the least useful portion of the book from the viewpoint of the student of international organization since the focus is largely on the activities of Arab governments and the extent of their legitimacy in international law rather than on the role and performance of the League in promoting joint action vis-à-vis Israel.

The main contribution of the book lies in its examination of the Arab League's handling of inter-Arab conflicts. Using League documents, Hassouna records and analyzes the organization's conflict-management activities in nine conflict situations between 1948 and 1973, including such major disturbances as the Lebanese crisis of 1958, the Iraq-Kuwait and Algerian-Moroccan conflicts and the Yemen war. In his analysis, the author has two main concerns. The first, flowing from the fact that this study was initially prepared as a Ph.D. dissertation in international law, is to establish and evaluate the legal basis for the activities of League organs in the course of these disturbances. As a corollary, he seeks to assess the amount of autonomy which the United Nations accorded the League in the handling of these conflicts in order to determine the effective division of jurisdiction which has developed between the two levels of organization.

The author's second major concern is to analyze the pattern of the League's activity and performance in the management of conflict. Here he emphasizes the expansion of the role of the secretary-general, the development of techniques of fact-finding and observation and the tendency to resort to procedures of mediation rather than rendering judgments. With respect to performance, Hassouna emphasizes that the League has been much more successful in persuading member states to freeze or de-escalate their conflicts than in settling the substance of these conflicts or taking effective measures to end hostilities. He concludes with certain suggestions for the improvement of League machinery and procedures which he feels will enhance its ability to deal with inter-Arab conflicts.

By providing a reasonably detailed record of the Arab League's conflict-management activities and identifying the basic patterns and trends discernible therein, Hussein Hassouna has performed a useful service. This study fills a genuine gap in our knowledge and will undoubtedly constitute a helpful reference work for students of regional organizations. Its value for the political scientist, however, is limited somewhat by the author's almost total neglect of political factors in his analysis. This is evident first of all in his analysis of the pattern of institutional activity itself. Here Hassouna indicates which bodies and techniques tend to be used by the League for the management of conflict and with what degree of success. No attempt is made, however, to determine whether the pattern of League activity or performance varies according to the type of disturbance with which it is confronted. In all probability, the amount of success achieved, and even the approach adopted, will depend on the nature of the parties to the conflict (e.g., major or minor members of the system), the type of issue involved (e.g., territorial, status, ideological or other issues), as well as the form of the conflict (e.g., civil wars or traditional interstate conflicts).

A more serious manifestation of the neglect of political factors, however, is to be found in the author's limitation of his analysis to a *description* of the pattern of League activity and the failure to provide any systematic *explanation* of its performance either in individual cases or overall. This reflects an implicit assumption that an international organization's success or failure can be explained largely in terms of the strengths or weaknesses of its own machinery and powers. International organizations are essentially superstructural phenomena, however. Consequently, their conflict management performance will depend primarily on the characteristics of the regional political system within which they function. In particular, it will depend on the goals and policies of the major actors in the system, the level and distribution of capabilities and the pattern of alliances among system members, as well as on the pattern, type and level of conflict with which the regional organization is confronted. It is only through an effort to link institutional behavior to the political framework of a system that we will gain a full understanding of the conflict-management performance and potential of a regional organization.

PAUL C. NOBLE

McGill University

The Irish Triangle: Conflict in Northern Ireland. By Roger H. Hull. (Princeton, N.J.: Princeton University Press, 1976. Pp. ix + 312. $15.00.)

Recent civil conflicts in Ulster, Cyprus, and Lebanon seem to constitute renewed testimony to the irrelevance of international law in the control of internal strife. Ulster itself presents a good example of the legal problem posed by internal conflict. This volume by a professional international lawyer attempts to examine the relevance of international law to the control of the internal conflict in Ulster and to formulate a set of policy relevant proposals for managing that conflict. Relying on a wide variety of sources, including recent government documents, the author describes the views of "Dublin," "Belfast," and "London" on the history and constitutional position of Ulster, and the applicability to the conflict in Ulster of international law relating to civil strife, the conduct of war, and the protection of human rights. In addition, he articulates and defends his own conclusions on each of these subjects.

Following a concise but thorough assessment of the dispute on Britain's authority to determine Ulster's fate, Mr. Hull places both the legal and political responsibility for Ulster squarely on the British doorstep. He also finds that the Republic of Ireland has a responsibility under existing international law, which it has clearly not met, to suppress activities in the republic in support of rebellion in Ulster. Readers will also find here thoughtful analysis of the relevance of the contemporary law of war to the Ulster conflict. Although the author's treatment of the issues here is generally capable, he unfortunately does not consider the current debate raging in the Conference on Humanitarian Law over the revision of the 1949 Geneva Conventions. Mr. Hull's analysis is valuable, however, insofar as it illuminates the unused potential of the existing European Conventions on Human Rights to mitigate abuses of human rights in the Ulster conflict. But while the analysis throughout this volume reveals how the law can be applied to manage the Ulster conflict, whether it will be applied effectively is, of course, another question to which the author does not give as careful attention.

There is a propensity in the western approach to international law to view international law as a set of principles reflecting the reason of the community of states rather than as the outcome of a baser political struggle. By not considering the political foundations of law the western law specialist often fails to come to terms with its ineffectiveness. Mr. Hull's effort to view the Ulster conflict in relation to international law, while generally a probing analysis, does not escape this criticism entirely. Thus, as if the law were handed down by wise men, he writes: "In order to facilitate the distinction between indigenous and exported conflict and to balance countervailing forces that abound in civil struggles, various principles of law have been developed" (p. 125). The process which established the principles to which the author refers was somewhat less rational and certainly more political than is suggested. Much of contemporary international law reflects less the inherited wisdom of the ages than the political weight of the major powers in the law-making process. The recent ineffectiveness of this law in Ulster and elsewhere reflects both the changing nature of warfare and changes in the global power structure. Under these circumstances, to insist, for example, as the author does, on the continuing general relevance of the traditional insurgency-belligerency doctrine and the requirement that insurgents conform with the laws of war in order to qualify as belligerents is to avoid the issue of why these traditional standards are increasingly irrelevant and ineffective.

In the final chapter Mr. Hull attempts "to cut the Gordian Knot" of the Ulster conflict by advancing "workable" proposals for ending the strife in Northern Ireland. The extension of full equality to the Catholic minority in Northern Ireland is identified as a necessary first step in an overall solution. As a second step, Mr. Hull proposes the introduction at Britain's request of a peacekeeping force organized under the auspices of the U.N., NATO, or preferably the EEC, to replace the British Army presence in Ulster. He also proposes that no change be effected in the status of Northern Ireland "unless and until" the people of the Six Counties decide by democratic means to alter that status. Finally, he proposes as an essential part of a solution to the Ulster problem an end to the segregation in the schools of Northern Ireland. The workability of this solution, however, can be questioned. The problem of achieving social justice in Northern Ireland is, of course, fundamental and therefore not easily resolvable as a "first step." An EEC peacekeeping force raises political and legal questions with which the author unfortunately does not deal. Further, democratic determination of the fate of Ulster is a solution which, one may argue, oversimplifies the problem of a conflict involving a struggle for the rights of a minority. Finally, the intuitively appealing notion that the integration of the schools of Northern Ireland will help shape the social environment in a direction conducive to the reconciliation of Catholic and Protestant communities is challenged by Rose's finding that mixed educational experiences do not have such an effect (R. Rose, *Governing Without Consensus: An Irish Perspective,* pp. 335–36).

The fact, however, that the conflict in Ulster may defy these solutions does not diminish the value of the analysis of the timely and important legal issues posed by the Ulster strife presented in this volume.

WILLIAM D. JACKSON

Miami University of Ohio

Perception and Misperception in International Politics. By Robert Jervis. (Princeton, N.J.: Princeton University Press, 1976. Pp. xii + 445. $22.50, cloth; $11.50, paper.)

As its title suggests, this volume is a further manifestation of Robert Jervis' interest in the evolution and impact of perceptions in international decision making: "What are the causes and consequences of misperceptions? . . . How are beliefs about politics and images of other actors formed and altered?"

The volume's argument is developed in four sections. In the first and perhaps least satisfactory of the sections, Jervis lays out what would appear to be a framework for the subsequent discussion. Chapter 1 considers the question of the appropriate level of analysis for the study of decision processes. Considering the external or international system, the internal or domestic setting and finally the bureaucratic focus, Jervis shows how each perspective is inadequate in explaining international decision making. While some arguments are good, others appear contrived and the reader has the feeling that it really was unnecessary to show the inadequacy of the other approaches for the purposes of concluding that perception is a significant if not crucial component of the decision process. Thus, the purpose of the first chapter is to provide the rationale for focusing on perceptions and images.

The intentions of chapters 2 and 3, however, are somewhat less clear-cut. Chapter 2 would appear to be concerned with how actors attempt to understand and predict the acts of others and the extent to which the various levels discussed in chapter 1 provide this type of information. There is some difficulty, however, in linking the thrust of this chapter with the more general argument made in chapter 1 and the argument subsequently presented in chapter 3. "Deterrence, the Spiral Model and Intentions of the Adversary," chapter 3, presents a thoughtful and illuminating critique of two models used to explain the significance of perceptions of intentions in the decision process. Jervis cogently contrasts the deterrence model—having so great a preponderance as to convince the adversary that you mean what you say—with the spiral model—having just enough to stay ahead—and shows the implications and explanatory inadequacies of each. While not all will agree with his characterization of these two models and the implications he draws, the analysis is thought-provoking. The difficulty with chapter 3, as with chapter 2, is its unclear relationship to the earlier and subsequent chapters of the volume.

Unquestionably, the best part of the volume is found in Part II: "Processes of Perception." The analysis presented in the four chapters of this section, together with the extraordinary range of documentation, provide an insightful and highly instructive argument for how decision makers come to acquire their perceptions and, thus, where and how misperceptions can occur. Chapter 4 is concerned with cognitive consistency and suggests that ignoring and altering certain aspects of incoming information is a necessary component of rational decision making. Contrasting this aspect of the decision process with what normally takes place in scientific discovery, Jervis produces a convincing argument for why decision makers cannot process all available facts and why the facts chosen are those most conducive to fitting current perceptions. But while the need for constituency is a necessary component of rational decision making, Jervis points to a related problem that he argues is not required and indeed dangerous to the decision process: irrational consistency, the inclination to generalize that while a policy is good (or bad) in one area it must necessarily be good (or bad) in others. Chapter 5 continues the line of reasoning by considering the importance of the context within which information is received. Most recent events, Jervis argues, set the stage for the interpretation of the next pieces of information. Chapter 6 is a discussion of the significance of history for decision makers. The argument here is that decision makers typically use history for drawing generalizations that can be applied to current policy issues, not, as some have suggested, to rationalize policy decisions. Jervis shows the types of events from which decision makers appear to learn the most and the kinds of lessons gleaned from these analyses. Chapter 7 then picks up on the theme of chapter 4. If consistency is an important component of rational decision making, then how do decision makers change their attitudes? How do they eventually recognize the fact that they have received information that disagrees with previously established images and sets? The purpose of the chapter seems less that of answering these questions than that of illustrating the devices by which old and outmoded perceptions are retained.

Part III of the volume considers various forms of misperceptions. Chapter 8 suggests that a common misperception is to see others as being more centralized, more planned and hence more devious than is in fact the case. There is thus a tendency to believe that the acts of others are not conditioned by accidents but are always part of a larger plan of attack. Chapter 9 proposes that another common fallacy is to exaggerate the degree to which one is a factor in another's policies, to believe that one's importance and influence are greater than is the case. Chapter 10 considers the influence of desires and fears on perception and concludes that wishful thinking is not typically a part of international political decision making. Chapter 11 is a discussion of cognitive dissonance in its relationship to misperception and consistency. The concluding section, Part IV, contains a single chapter devoted to what might be termed policy proposals. On the basis of the analysis presented in the previous 11 chapters, Jervis suggests some ways in which misperceptions might be minimized.

One of the most impressive features of this volume, particularly in Part II, is the documentation that Jervis accumulates to illustrate and substantiate his points. A vast array of historical examples ranging all over the time horizon is provided to demonstrate how, for example, the need for consistency operates in the decision process. But in addition to these informative and contrasting pieces of evidence from history, Jervis weaves in a variety of other forms of illustrative material from psychology, physics, medicine, and philosophy of science. The impact of this type of analysis can be seen in the glowing reviews of several social scientists given on the dust jacket of the volume. Thus, for example, Glenn Snyder concludes: "it is the finest piece of inter-disciplinary research that I, at least, have ever seen, truly a model for that sort of enterprise."

DINA A. ZINNES

Indiana University

Disjoined Partners: Austria and Germany since 1815. By Peter J. Katzenstein. (Berkeley and London: University of California Press, 1976. Pp. xv + 263. $13.50.)

This work appears at a time of declining belief in the inevitability of European integration. It is a case study of Austro-German relations, an outgrowth of a Harvard doctoral dissertation. The book is well researched, documented, and lucidly written. There are, however, numerous typographical errors.

The lack of political integration of the German-speaking people has been traditionally explained by the presence of various international constraints. In the Austro-German case, the Treaties of Versailles and St. Germain, as well as opposition from France, were responsible for preventing *Anschluss* until 1938. Peter Katzenstein has deliberately downgraded these influences. Instead, he has maintained that there is no "natural trend toward political unification of culturally cohesive regions" (p. 3). Rather than copressures and integration, "counterpressures and autonomy are the distinctive features of relations among disjoined partners" (p. 14).

Katzenstein has divided the period since 1815 into six phases: (1) the aristocratic (1815–1848), (2) the conflictual (1848–1870), (3) the hierarchical (1870–1918), (4) the voluntarist (1918–1938), (5) the structural (1938–1945), and (6) the pluralistic (1945–1970). While some of these classifications are debatable, especially the voluntaristic one, he has ably demonstrated that in each phase there were both integrative and counter-integrative pressures.

In the aristocratic phase the overriding consideration for the German states was the suppresion of anti-dynastic, liberal, and national movements. There was partial political integration for this purpose, but a marked absence of socioeconomic and cultural interstate institutions and relations. The Habsburgs imposed strict controls on communication.

During the conflictual phase the opposite occurred—conflict at the political elite level and considerable cooperation and interdependence in the socioeconomic sphere. Prussia and the Hapsburgs competed for control of Germany. In the hierarchical phase the Austrians, in their conflicts with the non-German nationalities, came to rely increasingly on political support of the Second Reich. Pan-Germanism became a powerful movement to which Katzenstein unfortunately does not pay sufficient attention.

After the collapse of the two dynastic empires in 1918 there was vast support in Austria for *Anschluss,* but as Katzenstein has correctly argued, the Christian Social Party became the dominant political force since 1920; it had vested interests in maintaining Austrian political independence. *Anschluss* was finally achieved by conquest in 1938, although there was probably majority support for it. After World War II a distinct national consciousness arose; traditional and social cleavages diminished. Austria was also at last economically *lebensfähig.* It, unlike Germany, had its own government in control of all of the country. Just as Hitler had restructured much of the Austrian economy, he was also responsible for instilling a large measure of national consciousness. It was ironic that a period of intense integration fostered independence. Katzenstein could have dealt with this at greater length.

Katzenstein has succeeded in demonstrating that high degrees of cultural and socioeconomic interdependence need not lead to political integration. It is difficult to disagree with his conclusion that the "fragmentation of central Europe looks more permanent today than at any previous time since 1815" (p. 225). The greatest value of the book lies in its questioning of much of the prevailing wisdom about the factors leading to integration. Integration theorists should pay close attention to Katzenstein's findings.

JOHN DREIJMANIS

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The Other Arms Race: New Technologies and Non-Nuclear Conflict. Edited by Geoffrey Kemp, Robert L. Pfaltzgraff, Jr. and Uri Ra'anan. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. 218. Price not given.)

There has been much speculation about the impact of some of the major technological innovations in weaponry that have been or will soon be introduced into the arsenals of large and small nations, and which in the future may become available to non-state actors. *The Other Arms Race* is an effort to examine some issues related to non-nuclear conflict that have been raised by these innovations. This book is the outgrowth of a 1974 conference conducted by Fletcher's International Security Studies Program; more than half chapters are revised papers from that conference. Both the quality and usefulness of the chapters vary considerably. However, because of the importance of the subject and the quality of most of the chapters, this is a valuable volume. Developments in weapons technology that are examined include: precision-guided, air-delivered munitions; remotely piloted vehicles; weaponized lazars and the numerous new surface-to-surface missiles for both maritime and land warfare. The implications of complex, improved command, control and communications systems, as well as surveillance and computational capabilities, are also examined.

No clear "answers" regarding the implications of these new technologies are contained in this volume. Rather, this work is an exploration of possible implications of the new technologies. As reflected in this volume, one of the striking features of this technological revolution is that considerable disagreement exists among the experts regarding its effects. There is a general view, however, that the new weapons are likely to give an advantage to nations possessing them because of the possibility of substituting highly accurate weapon systems for manpower. The increased recent manpower costs in the U.S. and certain other countries is seen by most of this book's contributors as requiring a reassessment of the options available through the substitution of sophisticated, but in many cases relatively inexpensive, new weapons systems.

The first part of the book contains a discussion of the technologies themselves and comments on their anticipated effects on the conduct of warfare. The second examines the impact of these technologies on the Middle East and European theaters and on prospects for low-level violence. Topics covered are: "Precision-Guided Munitions," by James F. Digby; "Tactical Air Systems and the New Technologies," by John E. Ralph; "The Role of SAMs in Tactical Warfare," by Mike W. Fossier; "The Future of the Battle Tank" by Richard M. Ogorkiewicz; "U.S. Naval Ship Design—One Viewpoint," by Reuven Leopold; "The New Technologies and the Middle East; " 'Lessons' of the Yom Kippur War and and Anticipated

Development," by Uri Ra'anan; "High Technology Terrorism and Surrogate War: The Impact of New Technology on Low-Level Violence," by Brian M. Jenkins; "New Technology and the European Theater," by Kenneth Hunt; "New Technologies and the Emerging Geo-Strategic Environment," by Geoffrey Kemp and Robert L. Pfaltzgraff, Jr.

Digby's discussion of capabilities and consequences of precision-guided munitions (PGMs) is concise, clear and useful as an introduction to further study of this subject. He is more firmly convinced than other contributors that PGMs are likely to be advantageous for defense and might lead to greater stability, particularly in Europe.

For those inclined to write off the battle tank as a vulnerable dinosaur in an age of inexpensive PGMs, Ogorkiewicz's paper suggests useful caution. While he concludes that tanks still constitute the most effective equipment available to ground forces for fighting tanks and other armored vehicles, his presentation is balanced and informative.

One of the most interesting papers in this volume is Ra'anan's analysis of some of the "lessons" of the 1973 Yom Kippur War, which takes issue with the following widespread views: that PGMs dominated the battlefield and rendered tanks practically obsolescent; that PGMs in that war dominated the skies to the extent of rendering aircraft almost obsolescent; that as a result, "defense" in the Middle East has regained the upper hand over "offense" and thus the significance of preemption and lightning strikes by air, plus armored thrust by land, has diminished and the qualitative gap previously favoring the Israeli side has been closed. Ra'anan's analysis is relatively short, but carefully reasoned.

Jenkins' discussion of the probable implications of the new technology on low-level violence is interesting and provocative. His first major point is not controversial: The new technologies, together with increasing vulnerabilities of society, are presenting guerrilla warfare and terrorist groups with a growing capacity for disruption. His second basic idea, however, is more provocative. He argues that, for a variety of reasons, modern conventional warfare as an instrument of political pressure is becoming more unattractive and this will lead some nations to adopt terrorist groups or terrorist techniques as a means of surrogate warfare. He includes as part of the new technologies not merely weapons but the mass media, and argues that this will enhance the effectiveness of terrorists by expanding the effect of any given action. The general thought he develops is, as he notes, "an idea that owes as much to Marshall McLuhan as it does to Mao-tse-Tung" (p. 93). He is not sanguine about the capacity of at least part of the international system to survive in this new environment. He argues that, as the power to destroy becomes more diffuse, political power may also become more diffuse, as it must be shared with others who have the capacity to disrupt. This could mean the creation of many more nations. "The resultant international system is likely to resemble the political complexity of Renaissance Italy" (p. 106).

Hunt's analysis of the impact on the European theater and Kemp's and Pfaltzgraff's overview of the impact on the emerging geostrategic environment are useful and balanced papers which underscore the tentativeness of the conclusions reached.

Lest one be misled by the title, this book does not, except in the most casual way, deal with problems of arms control for this particular arms race. Indeed, the emphasis here is on the need for continued research and development, with little attention to the implications of this for arms control efforts or to costs for rapidly changing military arsenals.

The 61 pages of appendices contain a useful inventory of the new weapons systems and a glossary of technical terms.

LAWRENCE D. WEILER

Stanford University

Technology and International Politics: The Crisis of Wishing. By William R. Kintner and Harvey Sicherman. (Lexington, Mass.: D.C. Heath, 1975. Pp. 177. $14.00.)

This book is a product of the Foreign Policy Research Institute, University of Pennsylvania, with which both authors are associated. It is addressed to selected antagonistic interactions between technological innovations and the international order. Following chapters on the history and characteristics of modern technology, four topics are given special attention: competitive nuclear weaponry, European integration, technology transfers to the LDCs, and the "energy crisis." These topics are considered in the context of the hardy vitality of the nation-state system; disparities among nations of military power, technical knowledge, and economic development; population growth; and determination within the LDCs to acquire a larger share of the world's wealth by whatever means.

The authors deploy the dialectical method of posing paradoxes and dilemmas. It is effective here in part because of the tendency of technological innovations to produce unintended "spin-offs," many of which are dysfunctional, and a few monstrously threatening. In particular, innovations have multiplied interdependencies, while concomitantly fostering uncertainties that limit or nullify intended benefits from the innovations in question.

Western Europe exhibits such a paradox. The authors hypothesize that "prospects for [Europe] becoming a center of power in its own right will become increasingly uncertain the longer it takes the Europeans to devise efficient collaborative organizations for technological innovation" (p. 90).

Regarding competitive weaponry, it "does not fashion a clear or stable strategic balance; rather . . . an uncertain and fluctuating strategic balance" (p. 64). The authors query whether such a system "can tolerate a commensurately rapid increase in uncertainty, and what its members are prepared to do about technology . . . [if] this uncertainty undermines their sense of security and well-being" (p. 39).

Regarding technology transfers to the LDCs and their economic development in general: If the Forrester-Meadows "projections are accurate . . . the developing countries will never be able to secure the comforts of the advanced technological societies without provoking world catastrophe" (p. 135). Without "fundamental innovations" to restore energy abundance, increase food supplies, and regulate inner and outer space, "the prospects for international peace and the preservation of the earth's ecology will be dangerously uncertain" (p. 137).

Final conclusion: "Technology's impact" of proliferating interdependencies and sharpening competition "may yet force men to recognize that they must move beyond the artificial barriers of the nation-state if their civilizations are to survive" (p. 144).

One speculates that the authors chose the dialectical format in order to shake people out of some traditional ruts of political science. If so, they will scarcely be surprised if their book evokes many caveats. Several occur to this reviewer, though limited space permits consideration of only one.

A recurring theme is that innovative technology can extricate humankind from the predicaments that technological innovations have contributed to. This theme comes through strongly in passages on the energy crisis. New energy sources will provide the wherewithal to produce more food to relieve hunger and tensions in the LDCs, while sustaining the affluence of the economically advanced societies. Maybe so; but energy development projects are themselves voracious consumers of energy.

Potentialities of such projects will be tested not only in laboratories and pilot plants but also in the balance-sheets of corporations and governments. How many watts will it take to generate an additional kilowatt by nuclear reactors or other means? Some estimates suggest that the energy costs of producing oil from shale, electricity from fission or fusion and other projects may consume most of the additional energy produced—if all capital and operating costs are included. And in the case of nuclear reactors, how much will it cost to protect people against formidable risks—assuming protection can be purchased at any price? What additional costs to cope with international blackmail, terrorism, accidents from human error and technical malfunction, leakage of lethal residues, etc.? Physicist Weinberg proposes a "Faustian bargain"—limitless energy for centuries—but with a crucial requisite: a degree of worldwide political stability never even remotely approached in human history—a poor bargain in this reviewer's judgment.

The book reflects wide reading, and, on military technology and strategy, Kintner is well known for professional competence. Documentation is adequate, though numerous important contributions to the dilemmas and paradoxes discussed somehow got overlooked. Many of these are listed in *Environment: A Bibliography of Social Science and Related Literature,* ed. by D. E. Morrison, especially pp. 816–20 (Government Printing Office, 1973), and in the indexes of *Science, Bulletin of the Atomic Scientists,* and other relevant journals.

Despite these and other caveats, the book deserves good marks on several grounds. Most importantly, it clarifies dilemmas destined to bedevil international statecraft indefinitely. The book is replete with sharp critical assessments—within the authors' chosen frame of analysis. If $14.00 seems a lot to pay for 144 pages of reading, the book's merits come closer to justifying the price than many do. This is definitely not a textbook, though its provocative format and rhetoric may commend it to some teachers as well as to productive scholars and a wider general audience of lay readers.

HAROLD SPROUT

Princeton University

Cromwell and the New Model of Foreign Policy: England's Policy Toward France, 1649–1658. By Charles P. Korr. (Berkeley: University of California Press, 1975. Pp. 282. $12.50.)

Students of international politics agree that the Peace of Westphalia (1648) was the critical and initial phase of a European state-system, yet the decade which followed was one of profound instability in diplomatic relations. No regime more thoroughly characterized the seeming confusion in diplomacy than the revolutionary regicides of England's Commonwealth and their successor, the Protectorate of Oliver Cromwell. In this revised and expanded version of his UCLA doctoral dissertation, Charles Korr has attempted "to build upon the framework established" by Christopher Hill and Michael Roberts "to show how Cromwell's perception of events caused him to construct a foreign policy that represented the priorities he had established for his rule in England" (pp. 2–3). The subtitle reflects the focus of the study, but Korr demonstrates a mastery of the primary and secondary literature on England's overall foreign affairs.

Historians have long been plagued by the lack of material on policy making in these years, a problem which Korr attempts to solve by analyzing Cromwell's policy as it was viewed by foreign observers. Unfortunately, portions of some early chapters reflect "the tremendous confusion surrounding" Cromwell's intentions which typified the reports of various envoys. The focus on France also tends to neglect the impact of Baltic affairs on Cromwell's policies, but Korr does provide new insights. He argues that Cromwell's Western Design—usually treated as an unmitigated disaster—did not compel England into a French alliance. Cromwell retained a great deal of autonomy in his diplomacy, and exploited the New Model Army and Blake's naval supremacy as effective bargaining instruments, even after 1655. Similarly, Korr rejects the frequent analogies historians have made which compared Cromwell's policies to those of Elizabeth I. Instead, he concludes that Cromwell's skillful "use of alliances and diplomacy to insure the survival of a new regime" was closer to the Tudor dynasty's founder, Henry VII (p. 211). Korr's incisive footnote commentary is an integral part of his thesis, and it is unfortunate that the publisher has buried the notes at the end of the book.

Highly interpretative in his approach, Korr does not always provide adequate evidence for his perceptive summaries of Cromwell's intentions and objectives. However, the scholarly audience for which the book was intended will find this a rewarding study, particularly Korr's historiographical essay in the concluding chapter. As the head of a revolutionary state in an age of dynastic monarchies, Cromwell provides students of diplomacy and international politics a rewarding model of *real-politik* in Early Modern Europe. If some of Korr's interpretations can be challenged, he has also offered us a challenging study which deserves the consideration of seventeenth-century specialists.

MICHAEL F. FOLEY

Georgetown University

The Politics of Race and International Sport: The Case of South Africa. By Richard E. Lapchick. (Westport, Conn.: Greenwood Press, 1975. Pp. xxx + 268. $13.95.)

The Olympic Games ideal of pristine international sports unfettered by politics has, of course, never been attained. From Hitler's famous exploitation of the Berlin games in 1936 to the violence of Munich in 1972, politics have played a major, perhaps increasingly major, role in international athletics of all types. This volume details one of the most long-standing and blatant episodes in this stained history.

Lapchick is a political scientist at Virginia Wesleyan College and the son of Joe Lapchick of legendary basketball fame. His book constitutes the first to be issued in a series of "Studies in Human Rights" sponsored by the Center on International Race Relations of the University of Denver.

Lapchick begins with a brief review of the history of racial segregation in South African sports, most of it enforced not by the government but by sports officials themselves. But he concentrates his attention on the 1959–1970 period when the Nationalist governments explicitly took control and the issue attracted worldwide attention. Sympathetic descriptions are provided of the growth of protest against "apartheid sport," protest both within South Africa and around the globe.

The central thesis put forward concerns which protest strategy is the most effective: a compromise policy of moderation versus an uncompromising policy of excluding South Africa from international competition. The author unreservedly supports the latter uncompromising strategy and the responses of the South African government over the years bear out his contentions. Moderate protests in the 1960s led to window-dressing statements and marginal changes that left untouched the basic apartheid structure. But the near-complete athletic isolation of South Africa in more recent years has led to some structural changes, modest in their sweep to be sure yet nonetheless significant when set in the broader perspective of nationalist thought. Lapchick is correct, I believe, when he argues how critically important international sports are to South Africa. I recall two decades ago being in South Africa during a football test match with England; the American madness over baseball's "World Series" and football's "Superbowl" pales in comparison to the virtual paralysis of South African society accompanying a major athletic contest.

Unfortunately, the arguments put forward in favor of militant protest represent the nearest thing to analysis that this narrow book of detailed description has to offer. Lapchick presents almost no insights into South African perspectives; his sources include largely works on sports to the exclusion of the vast scholarly literature on both South Africa and the politics of modern protest movements. His specific contentions could have been considerably strengthened had he placed his problem in its broader social and political contexts.

Consequently, *The Politics of Race and International Sport* may well prove of interest to specialists who would value a careful recording of the details of race and South African sports. But it will not prove helpful to those who seek a significant new political analysis of South Africa.

THOMAS FRASER PETTIGREW

Harvard University

Religion and Nation: The Kwangsi Clique in Chinese Politics, 1925- 1937. By Diana Lary. (New York: Cambridge University Press, 1975. Pp. 276. $19.50.)

This is a carefully written, meticulously researched, and thoroughly perceptive book. It recounts and analyzes the history of one of the more important of the regional military regimes which dominated China during the so-called "warlord period" from 1911 to 1937. In an especially incisive introductory chapter the author formulates a number of provocative generalizations concerning the nature of such regimes. For example, she suggests that instead of comprising a decentralizing force, as assumed by most scholars, the warlords actually prevented China from fragmenting altogether by preserving a considerable degree of unity at the regional level. She also believes that at least those warlords who acquired some education, either at military schools or abroad, were genuinely "new" men, whose concept of China was much affected by modern nationalism and, therefore, fundamentally different from that of the Confucian-minded mandarins whom they displaced. "The regionalists would apply themselves practically, in the regions they controlled, with the aim of serving the nation," she says. "Regionalism was thus elevated from a *faute-de-mieux* to a positive policy, justified by the high nationalist cause which it served" (p. 19). Other chapters describe Kwangsi's unusual poverty and wretchedness—"a province without even its own cuisine"—(p. 27) and the ways in which the Kwangsi generals fashioned out of this environment an army which by the late 1920s was one of the best in China. With this army and while acting in alliance with the Kuomintang, the Kwangsi generals built what the author calls a "Kwangsi Empire," encompassing many of the provinces of southern and central China, only to have it shattered in 1929 by the growing power of Chiang Kai-shek, whose relations with the Kwangsi clique the book examines in considerable detail. Especially interesting are the chapters dealing with the period following this defeat when the Kwangsi generals withdrew into their own province and tried to create there an entirely new society structured along military lines. Their aim was to modernize Kwangsi's economy and in this way not only strengthen themselves but also embarrass Chiang Kai-shek, whose own efforts to achieve economic modernization appeared to be floundering. Yet, although they succeeded in imposing an unprecedented degree of control on the population and even implemented a remarkably effective mass literacy movement, Kwangsi's economy remained extremely backward. This occurred because the Kwangsi generals refused to alter the basic structure of that economy, meaning the oppressive network of rents and interest rates which discouraged the peasantry from innovating or even producing much more than a subsistence crop. Nevertheless, there were set in motion forces which must have had a revolutionary impact on the society of Kwangsi, again suggesting that in China so-called "warlordism" was an important, probably unavoidable step in the direction of radical change and modernization.

Still another contribution of this book is a detailed examination of the role played by ideology in the history of the Kwangsi clique. Like Feng Yü-hsiang, Yen Hsi-shan, and many other warlords, the Kwangsi generals tried almost desperately to devise a set of ideas or values which could be used to arouse popular enthusiasm for their objectives. Ultimately, they settled on a hodge-podge of notions borrowed from such diverse sources as Sun Yat-sen and European fascism; however, by far the most dominant motif in their ideology was militarism, with its emphasis on authority and obedience. Such an ideology might have been acceptable if accompanied by changes which affected profoundly the livelihood of Kwangsi's largely impoverished inhabitants, but in the absence of such changes any ideology was bound to be meaningless to the vast majority of the population and, therefore, ineffectual. Nevertheless, a concluding chapter dealing with the Kwangsi clique's attempts to oust Chiang Kai-shek from the leadership of the Kuomintang, first in 1936 and again in 1949, leaves the reader with at least some respect for the Kwangsi generals' efforts to create a noncommunist alternative to Chiang's rule. This book is an important contribution to our knowledge of the history of China during the Republican era.

DONALD G. GILLIN

Vassar College

Intervention: External Involvement in Civil Wars. By Richard Little. (Totowa, N.J.: Rowman and Littlefield, 1975. Pp. xii + 236. $18.75.)

This is a creative contribution to the development of a data-based theory on intervention in civil wars, a subject generally slighted by students of international relations. The title promises a bit more than Richard Little delivers since he deals only with military interventions in a restricted number of cases. He supports the construction of a model of the process by which governments decide to intervene in or remain aloof from civil wars through analysis of four British responses: a unilateral interventionary response to the Portuguese Civil War in 1826, a collective noninterventionary response to the American Civil War, a collective interventionary response to the Russian Revolution, and a unilateral noninterventionary response to the Spanish Civil War.

Usually, international relations scholars who employ historical materials rely upon monographs at best and textbooks at worst. Thus, they must be content with only that evidence historians think important enough to publish. Eschewing ready-made data, Little painstakingly worked through the British Foreign Office archives as well as private manuscript collections. His venture—rare for a social scientist—pays off in the intelligent way he is able to use dusty drafts of memoranda and other unpublished material to illuminate the decision-making process. Of course, as Little acknowledges, such an approach is not only incredibly time-consuming, but is dependent upon the availability of archival sources.

Among the subjects he considers in his comparison of behaviors in interventionary and noninterventionary situations are: the differences between the psychological and operational milieus of the decision makers, the ways in which actors seek internal and external legitimation, domestic consensus building and its ramifications, how dissonance is handled, and eventually, the process of decommitment. Little classifies his approach to model building as neither deduction nor induction but retroduction where "the same body of data is used to derive and test a set of hypotheses" (p. x). Thus, he launched his investigation with a preliminary model (which curiously is not introduced until the appendix), gathered data on the basis of that model, and then, drastically reformulated the model. Little notes that although his approach is rarely identified in published work, retroduction is the method employed by most social scientists.

His most intriguing finding, and the one that frames the enterprise, is that although there is both a prescriptive and behavioral norm proscribing intervention in civil wars, when intervention does occur, actors claim they have not violated the norms. That is, by defining a situation as something other than a genuine civil war, decision makers can violate the norms with a straight face and send their troops into someone else's revolution. For example, the British considered their intervention in the Portuguese Civil War in 1826 as the fulfillment of an alliance commitment to defend Lisbon against Spanish or even French invasion. Or similarly, in the Russian Revolution, British forces intervened to support their war effort against the Germans, to save Czech prisoners of war, and to protect Siberia from the Japanese. Further, for a time, the cabinet claimed a civil war could not exist since Russia was in anarchy and the Bolsheviks were not a government.

This admirable reworked dissertation is not without its flaws. Using Lewis Richardson's war data to demonstrate that non-intervention has been a behavioral norm, Little claims that "the probability that a state will activate an intervention response when confronted by an intervention stimulus is .02" (p. 29). The formula he employs to arrive at this figure allows all members of the international system (n=50) an equal chance to intervene in every civil war. But military intervention in civil wars, for a variety of reasons, has been almost exclusively a major power phenomenon. The probability of major power intervention in civil war is around .15.

More generally, one wonders how much this study contributes to a universal model of intervention, based as it is on only four British experiences. Although the cases selected meet Little's criteria—representative of the four possible types of interventionary responses as well as four different historical epochs—there is no explanation of why these four and not four others. Further, his distinction between the *collective* noninterventionary response in the American Civil War and the *unilateral* noninterventionary response in the Spanish Civil War may be drawn too sharply. The British coordinated some of their policies with France during the Spanish Civil War and often went it alone during the American Civil War, especially when they agreed to meet Confederate emissaries in 1861.

Finally, because Little deals with each case episodically as he examines the eight propositions that constitute his model in eight separate chapters, some of the historical context is obscured. It is not always apparent who is making what decisions at what point in time, information vital to the evaluation of many letters and minutes cited. Occasionally ignoring bureaucratic and idiosyncratic factors, Little tends to accept at face value documents others might treat more critically.

Shortcomings such as these are expected in ambitious works. Clearly, Little's model makes good sense for understanding the process by which the British decided to stay out, intervene, and withdraw from four major civil wars since the 1820s. He himself hedges on the value of his model for explaining contemporary interventions, but it certainly suggests a framework that will be of utility to others studying this important activity.

MELVIN SMALL

Wayne State University

The Web of World Politics: Nonstate Actors in the Global System. By Richard W. Mansbach, Yale H. Ferguson, and Donald E. Lampert. (Englewood Cliffs, N.J.: Prentice-Hall, 1976. Pp. x + 326. $8.95, paper.)

Through a descriptive study of three geographical regions over a 25-year period, the authors make a notable contribution to the study of nonstate actors in world politics. Although the authors stress their examination of nonstate actors (interstate governmental, interstate nongovernmental, governmental noncentral, intrastate nongovernmental, and individual actors), their volume also merits attention for its innovative treatment of issue areas and events data.

The book is organized into a series of relatively autonomous sections. Early chapters review a state-centric paradigm that emphasizes the interaction exclusively of sovereign nation-states. (Question: do serious advocates still contend that states only deal with other states and fail to acknowledge as actors such intergovernmental organizations as the United Nations?) As a more adequate characterization of contemporary world politics, the authors propose "a complex conglomerate system" that highlights the interaction and shifting alignments of various types of autonomous actors, including both nation-states and nonstate actors. The balance of the book is devoted to demonstrating that a substantial array of autonomous, influential, nonstate actors exist in the system.

After a chapter on the Jacobite movement (roughly 1688–1746) presented as a precursor of contemporary nonstate actors, the authors examine recent activity in three geographical regions—Middle East, Latin America, and Western Europe. One chapter about each region provides a study of a prominent nonstate actor; namely, the Palestine organizations in the Middle East, multinational corporations in Latin America, and various integrative economic institutions in Europe. The most distinctive qualities of the book, however, appear in three chapters designed to complement the case studies. The authors characterize each region through the presentation of events data collected on the regional actors, interactions, and issues from 1948 to 1972. The 25 years are divided into three periods. For each period the authors present and discuss systemic information on (a) the most active state and nonstate actors and recipients, (b) the ranking of the most salient regional issues, (c) the major alignments of the actors on the issues, and (d) the region's most disruptive and supportive actors.

The findings of this quantitative analysis will generate discussion and further research. Consider two of the conclusions suggested by the analysis: (1) Although nation-states appear in an average of 89 percent of all dyads in each region, nonstate actors occur in an average of 64 percent. Thus considerable interaction occurs between nation-states and other types of entities. (2) "Overall nonstate actors of one kind or another were involved in over three-fourths of all conflict dyads and over 90 percent of all violent dyads! . . . Thus, we may conclude that nation-states or groups composed of nation-states are less likely to initiate conflict or resort to violence than individuals or nongovernmental groups" (p. 281).

Regrettably, we must accept the authors' conclusions on faith, because their description of the methodological procedures (confined largely to an appendix) is too truncated to permit much insight. For example, throughout the book nonstate actors are aggregated into broader categories such as media, socialists, or business. A table in the appendix illustrates the kind of entities included in each category but does not list all the included entities or the number of units involved. Some tables in the text list the entities that comprise a particularly important composite nonstate actor. These tables are not necessarily reassuring. For example, one finds that Yemeni royalists and republicans have been included in the same grouping called "Intrastate Arabs." Therefore, we must be careful in considering results on the activities of an aggregated nonstate actor that may consist of numerous entities who do not coordinate their activities and indeed may be adversaries.

Another inadequate characterization of procedures concerns the factor analyses that provide the basis for much of the reported material. Nine analyses were performed but the factors are not displayed. If complete presentation was impossible, one illustration could have demonstrated how the various indicators were constructed. The appendix devotes less than a page to describing the procedure for each construct. In some cases, such as the explanation of how to calculate the variance explained by superissues, the account raises more questions than it answers. (The unpublished doctoral dissertation of the third author is cited as a source of additional information on methodological issues.)

Central to the study is the question: "Do actors other than nation states freely exercise influence in world politics?" (p. 5) A conceptual-operational concern arises because the authors appear to imply influence in the quantitative chapters by the frequency with which an actor initiates and receives action. Though a case could be made for that procedure, it also is plausible that a supplicant may be a frequent initiator and recipient.

These difficulties diminish but do not offset the book's creative treatment not only of nonstate actors but also issue areas and events data. Issue areas are constructed so as to reveal their dynamic qualities. One can see, for example, the emergence of the Palestine issue in the Middle East or the rise and decline of the Cold War in Europe. Moreover, the authors' examination of the co-occurrence of issues suggests when they become intertwined.

In addition to demonstrating that events data can be used to give operational meaning to the various constructs used in the book, the authors have developed the first major longitudinal data set for nonstate actors. Their plausible argument that their source—the *New York Times*—may under-report the involvement of nonstate actors in world affairs, adds significance to their finding of the extensive involvement of such actors.

In their conclusion the authors question whether nation-states are desirable. Given the disposition of many nonstate actors toward violence and their frequent dependency on states, one might ask whether many nonstate actors are desirable. A more productive question, however, might be under what conditions actors—either nation-states or nonstates—become effective problem solvers serving their constituents and agents for desired change without resorting to violence? By providing a systematic and comparative foundation, this book offers a valuable text on nonstate actors and serves as the point of departure for new investigations on this and related questions.

CHARLES F. HERMANN

Ohio State University

Emerging Forms of Transnational Community; Transnational Business Enterprises and Regional Integration. By Bernard Mennis and Karl P. Sauvant. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. xiv + 240. $18.50.)

The nation-state is declining as the principal actor in world politics and major engine of global political integration. Non-state actors—international pressure groups, transnational political parties, world churches, and multinational corporations, to mention the major types—are rapidly transcending the nation-state, perhaps more rapidly than most scholars and policy makers are prepared to acknowledge or deal with. Mennis and Sauvant not only acknowledge these emergent nonstate actors but also bring to their investigation thoughtful theory, solid empiricism, and a perceptive explanation of some of the relationships between the behavior of one type of nonstate actor, the transnational business enterprise, and the *process* of international political integration. Their book should be "must" reading for public policy makers dealing with global economic issues and for corporate executives of transnational businesses. The book's theory and empirical findings will help such readers better comprehend the developmental implications of their attitudes, daily routines, and policy behavior.

Mennis and Sauvant estimate that there currently exist about 10,000 multinational corporations with between 50,000–70,000 foreign affiliates—that is, parent enterprises with affiliates in one or more foreign countries, or, as they prefer to call them, transnational business enterprises. The great majority of the parent enterprises are headquartered in five countries: the United States, the United Kingdom, France, the Federal Republic of Germany (FRG), and Switzerland. A chapter of descriptive statistics on number of enterprises, amount of investment and trade, etc., convincingly establishes that transnational business enterprises have in the past quarter century become "the most important actors in international economic transactions. . ." (p. 163).

Within the constraints of their research resources, the authors limit their investigation to the case of the Federal Republic of Germany and the process of regional integration in Western Europe. More specifically, they investigate how the regional integration process is experienced, perceived, and anticipated by 2,766 managers in 38 transnational business enterprises headquartered in the FRG. Their 23-item questionnaire was sent to 5,500 West German corporate managers, with a response rate of about 50 percent. The questionnaire items were designed to test the authors' theory of the integration process set forth in their second chapter.

Mennis and Sauvant are concerned with identifying and measuring those specific transnational interactions (independent variables) that explain specific world political outcomes (dependent variables). How and to what extent does employment in a transnational enterprise change the perspectives of corporate managers on certain sociopolitical matters, particularly those relevant to regional integration? How do intracompany allocations of resources and division-of-labor policies affect various patterns of international economic inequality? To what extent do transnational enterprises encourage the emergence of transnational values and behaviors that reflect home-country assumptions and practices? The authors' principal theoretical contribution is to link the behavior of these transnational non-state actors to the integration process. Using factor-analytical techniques, they were able to identify nine forms of integration that could be grouped under three broad categories: governmental (institutional), social (behavioral), and production (economic) integration. In other words, integration is conceptualized as a multidimensional rather than a unidimensional process, with a complicated structure and uneven degrees of support from the non-state actors.

Some of the authors' findings are of more than passing interest. The sample of FRG managers supported European Community integration in general and strongly, but with great variation among the different forms of integration. The best predictor of positive support for integration was a factor called "external interest," that is, a manager's sense of personal well-being as associated with perceived beneficiality of European integration. This positive attitude is most strongly developed in managers with extensive transnational work experience and is stronger than such factors as certain types of value priorities, forms of transnational contact, or degrees of corporate internationalization identified by the authors. In general, behavioral integration arising out of work experience appears to be an important antecedent of economic and governmental integration.

On the other hand, Mennis and Sauvant found that the prevailing pattern in which corporate managers prefer to organize the transnational activities of their enterprises is ethnocentric, that is, imposing home-country standards, interests, and business practices, on their foreign affiliates, as contrasted with systemcentric modes of organization. The pro-integration attitudes and the ethnocentric intrafirm orientations are not related to each other, but the combination may, according to the authors, tend to accentuate the already existing asymmetrical relationships among nation-states that are "imperialistic" in nature. In other words, the habits of ethnocentrism may tend to defeat the integrationist hearts and minds of the transnational enterprise managers themselves. Thus delineated, the problem becomes one for global institution-builders and skilled world leadership, namely, how to achieve the objectives of heart and mind despite the burdens of habit.

There is, of course, much more among the findings and interpretations, all of which invite a reading of the book.

RALPH M. GOLDMAN

San Francisco State University

The Natural Law Tradition and the Theory of International Relations. By E. B. F. Midgley. (New York: Barnes & Noble, 1975. Pp. 588. $30.00.)

The study of the morality of international interactions has nowhere to go but up. We are beginning to realize that almost exactly the same time that Hans Morgenthau was proclaiming the new science of realism in international relations, the atomic bomb was heralding a quite different era when that same "realism," grounded in a Hobbesian perception of the need for national "security," was going to become progressively more uninteresting. Even in the United States Department of State normative policy is increasingly talked about in terms not of whether a foreign government is "friendly" to us militarily but whether it will promote internal freedom and liberty. (Of course, we're still just scratching the verbal surface; the most that is now done is talk against those regimes that are denying liberty to political prisoners or racial groups.) Perhaps in years to come we will even perceive that no military allies, in Morgenthau's sense, are needed to defend an over-nuclear-stockpiled United States, but rather our long-term quality of life in a shrinking world will depend on whether there are other lands beyond our borders that respect basic human values. Nevertheless, at the same time that ordinary military confrontations are becoming uninteresting in terms of national security, guidelines are needed in the event of a nuclear-war confrontation. Since such an event has never occurred—if it does occur the early days or hours will be fraught with uncertainty as to retaliation, escalation, and the like—the guidelines of morality even in that tense and potentially world-suicidal situation may become prominently important. Thus, the normative aspect of the study of international relations will probably assume increasing significance both for shaping the sort of peaceful world we want our children to live in, and for sorting out our contingency thinking in the event that some nation ignites what might quickly become a suicidal thermonuclear war. E. B. F. Midgley's impressive tome on natural law theory in the context of international relations may be a signpost of a new look at ancient wisdom. It's a hard-to-digest book and straddles an ambiguous fence between being an historical textbook and an essay for a natural-law perspective in nuclear-war strategy. As a textbook, I found it to be exceptionally valuable. The author knows natural law theory from the perspective of a Catholic education (the book even has an Imprimatur, which might render it slightly suspect; however, the author wears his cross on his sleeve, which at least tells us his bias). Not only has Midgley wrestled with and digested the likes of St. Thomas, Suarez, Althusius, Grotius, Pufendorf, Wolff, Vattel, and some moderns like Delos and De Solages, in what appears to be his reading of their texts in their original languages, but he also had read the leading commentaries on these writers (exception: a certain ignoring of Rousseau on Pufendorf), and his references are extremely good. The scope of the author's learning is not easy to come by, and many future readers will be indebted to him for writing it all down in clear prose.

The worth of this book makes me somewhat reluctant to criticize what I believe are important shortcomings, but perhaps the only way to build upon normative theory is to make these criticisms clear to those who may be quite unfamiliar with the scholarly tradition Midgley addresses. My major criticism is Midgley's unsophistication about law in general and international law in particular. The author's background includes service in the British Civil Service, Administrative Class, as well as a strong grounding in philosophy. He credits Clive Parry with reading a draft of chapter 9 (relating to current international law); this is the only background I could discern with respect to international law in the author's training. The lack of familiarity with international law shows throughout his discussion of natural law scholars who wrote primarily about international law. However, this is not as significant an indictment as it might appear at first glance, because Midgley is more concerned with a moral approach to problems of international politics than with the subject of "international law" as viewed by writers as disparate as Pufendorf and Wolff. Yet, there is a strong positivist sense of what "law" means in Midgley's writings—a not untypical perspective coming from England, the home of positivism (Bentham, Austin, Hart). The result is a certain misunderstanding of what the great natural-law theorists meant by "law," a misunderstanding common to those who view law positivistically which is impractical for me to delineate in this short space (though I have written on the subject elsewhere). Curiously, most *lawyers*, when they read the classic international law theorists, go much farther astray in their inability to appreciate the natural-law component than Midgley does in misinterpreting the international-law component.

My second major criticism is one that Midgley probably won't like at all, and if he is the only one who ever actually reads this review (I often wonder whether a review is just a private letter to the author of a book), I apologize now and ask him to note that I don't take back my earlier comment that this is an exceptionally valuable book. The fact is, in my opinion, that Midgley entirely fails to prove his thesis–if his book is more than a textbook. That is, his final chapter on nuclear preparation does not follow logically from all that went before; not merely does it fail to follow, it is merely an addendum. The final chapter itself fails on two internal counts. First, it is not sufficiently sensitive to the subtlety of the academic and State Department game of talking about nuclear deterrence theory. This "game" played by the likes of Kahn, Kissinger, Schlesinger, and Schelling consists of a combination of objective theorizing and real-world nuclear-deterrence communication to the "other side." Midgley seems lost in sorting out the differences that necessarily result. For example, talk of a countervalue strategy seems grossly immoral to Midgley, when perhaps it was brought up by the particular writer for the deterrence value conveyable to the other side in discussing the bombing of cities (or even the dialectical value in the internal argumentation between academicians and Pentagonians). Secondly, Midgley has not related his earlier discussions of the "just war" and all its variations to any plausible scenarios in nuclear deterrence theory; he seems quite unfamiliar with these scenarios and war games.

However, we the readers do not have to buy Midgley's concluding chapter nor hire him to direct the next war. All we ought to do is read his earlier historical-analytical chapters with a view toward working out our own relevances to scenarios that make sense to us. Hence, in this ultimate sense, Midgley's book *is* more a textbook than an essay. But as a text on the difficult subject of the moral approach to international relations examined in a vast scholarship through the ages on "natural law," the text is worthy of study and repeated reference.

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The Functional Theory of Politics. By David Mitrany. (New York: St. Martin's Press, 1975. Pp. xxv + 294. $18.95.)

David Mitrany's theory of functionalism has exerted a remarkably persistent impact on western thinking about international politics and organization since the publication of *A Working Peace System* in 1943. In that essay and in his other writings, Mitrany rejected both classical conceptions of "power politics" and federalist visions to articulate the notion of a "working peace system" in which specific areas of human activity, important to welfare, would be organized internationally and managed by experts. He argued for a "sharing of sovereignty" rather than for its surrender: on particular issues, governments would "pool their sovereign authority insofar as the good performance of the task demands it" (p. 129). This political decision having been made, experts would take charge, organizing activities and delivering services for the common good. Eventually, as Ernst B. Haas has summarized it, "an ever-widening mesh of task-oriented welfare agencies would come to pre-empt the work now done by some governments, leading eventually to the creation of a universal welfare orientation" (Ernst B. Haas, *Beyond the Nation-State* [Stanford, Calif.: Stanford University Press, 1975], p. 11). Formal sovereignty would not be altered, at least not immediately, but the content of sovereignty would be changed by partial transfers of authority to functional organs.

The volume under review contains essays and excerpts from Mitrany's writings over a period of more than 40 years. It includes his essay on "The Progress of International Government" (1932) and the introduction to the Fourth Edition of *A Working Peace System* (1946), in addition to a number of extracts from shorter and lesser-known essays. Most important to the reader familiar with Mitrany's work, however, are two essays apparently published for the first time in this volume. One, "The Making of the Functional Theory: A Memoir," is autobiographical; the other, "Retrospect and Prospect," assesses the continued relevance of functionalism, in Mitrany's view, and criticizes "scientific" analysts of international relations and theorists of regional integration for allegedly using "the tools of science to cover arbitrary inventions."

Functionalism was a major contribution to the understanding of world politics, since it was the first major non-Marxist theory of the twentieth century to look beyond the nation-state to analyze coalitions other than classic inter-state ones: for instance, of bureaucracies with similar functionally defined tasks. It provided the intellectual basis for "neo-functionalist" theories of political integration, developed by Haas and others, as well as the early functionalist ideology of specialized agencies of the United Nations. Indirectly, at least, it contributed to contemporary analyses of transnational and transgovernmental relations.

Functionalism attempts to separate power from welfare, and claims that functionally defined activities can somehow be insulated from political interference. This vision is the source of functionalism's attractiveness: to people concerned with "practical problems" (whether of social security and welfare, as in Mitrany's case, or environmental protection, as for many people today), it appears to offer a way out of a dreary round of political infighting that contaminates technical activity and obstructs progress toward common human goals. Yet these assumptions have been severely criticized, most notably by Haas, and events over the last 30 years have not been kind to the functionalist vision. As Cox and Jacobson have shown, the functionalist ideology in the United Nations system has given way to a stress on "developmentalism," which "stresses the coordination and coherence of economic policy at the national level," and "centralization of policy control and action at the international level." (*The Anatomy of Influence: Decision-Making in International Organizations,* ed. by Robert W. Cox and Harold K. Jacobson [New Haven and London: Yale University Press, 1973], p. 404.) Recent events in specialized agencies such as UNESCO, and at the Law of the Sea Conference held over the last several years, have further dramatized the politicization of functional agencies and the importance of politics at every stage of international action, where governments' interests are importantly affected.

In light of this negative but clear record, it is surprising and somewhat depressing to read Mitrany's reaffirmation of the functionalist faith. Rather than analyzing the political forces that hinder realization of the functionalist dream, Mitrany preaches against them and asks plaintively: "Is there something incurably unhinged about the political mind in the mass" (p. 46)? Yet alongside this pessimism, one finds an almost desperate inclination to believe that functional progress marches on: "nuclear power, the exploitation of the sea-bed, satellites and space travel, they all move on a new plane of international relations and organization, shaped not by any theory as to the political self-determination of the parties, but by the technological self-determination of each of the matters involved" (pp. 250–51). And it is assumed—against mounting evidence—that functional organization really does insulate international administration from politics: "technology and resources make equal participation impossible, but functional organization and control ensure equality in protection and benefits" (p. 250). Thus Mitrany concludes that "the functional idea seems adaptable enough as concept and practice to serve the several needs of the present time" (p. 265).

In its pure form, and on a global scale, functionalism is an idea whose time has passed. Yet its insights into the potentially fragmented nature of pluralistic states, and its suggestions for political strategies to ameliorate the worst pathologies of politicization, remain valid bases for analysis and planning. Cox, Haas, Jacobson, Lindberg and Nye, along with other scholars, have made some progress toward redefining the functionalist insights and adapting them to contemporary political reality. It is to be regretted that the father of functionalism, David Mitrany, has chosen to defend his ideas in their original form, rather than to display the creative, adaptive learning that functionalism reminds us is necessary if international organizations are to adapt and thrive.

ROBERT O. KEOHANE

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Chinese Policy toward Indonesia, 1949-1967. By David Mozingo. (Ithaca, N.Y.: Cornell University Press, 1976. Pp. 303. $14.50.)

This interesting and well-written book is broader in scope than its title suggests. It actually deals with three topics: Chinese foreign policy, Chinese policy toward Indonesia, and Indonesian politics and foreign policy.

On the first of these, David Mozingo shows the common tendency of American academic writers on Chinese foreign policy to take Peking's self-evaluation, as expressed in its propaganda, largely at face value and to give it the benefit of the doubt most of the time. This flies in the fact of the excellent advice given by the late Chou En-lai to more than one foreign visitor not to take Chinese propaganda too seriously. The analysis accordingly revolves around the central position assigned in Maoist theory and propaganda to the "contradictions" between "imperialism" on the one hand and the "socialist" (i.e., communist) and Third World countries on the other. Chinese foreign policy is seen as largely reactive in practice to a perceived threat first from the United States, and more recently from the Soviet Union. This is all right as far as it goes, but it does not go far enough. It leads to Mozingo's highly questionable main thesis, which is that in the case of Indonesia, Peking for some reason deviated from its usual approach and let itself be drawn into competition with the United States and the Soviet Union for influence befitting a great power, instead of following a more "principled" line (to use a favorite Chinese term).

In reality, Peking has always aspired to be a great power (although not necessarily a "superpower") and has sought constantly and sometimes opportunistically to advance toward that goal; its policy toward India is an excellent example. Seen in this light, Peking's Indonesian policy is not so much an aberration as perhaps the extreme—although not the only—case of Peking overestimating its capabilities in a particular situation for promoting simultaneously its own national interests, the cause of local revolution, and Peking's anti-Soviet stance within the international communist movement.

Mozingo's analysis of Chinese policy toward Indonesia and of Sino-Indonesian relations is generally excellent for the period down to the early 1960s. He has done important original work on this subject, including interviews with Indonesians who had held responsible official positions. The emergence by 1961 of a rather close partnership between Peking and the Sukarno government, in spite of the formidable obstacle posed by the complex question of Indonesian resentment of the economically powerful Chinese community in the island state, is well handled. Insufficient importance is perhaps assigned, in evaluating Peking's contribution to the emergence of the entente with Jakarta, to Chinese concern that otherwise the Soviet Union might make major diplomatic gains at the time of Khrushchev's visit to Indonesia in early 1960.

This underestimation of the anti-Soviet aspect of Peking's Indonesian policy is a feature of much of the book and characterizes the analysis (pp. 229–30) of P'eng Chen's important speech of May 25, 1965, at the Aliarcham Academy in Jakarta, in which P'eng in true Maoist fashion denounced Soviet "revisionism" as a "prop" of American "imperialism."

Mozingo's entire analysis, in fact, tends to deteriorate after the early 1960s. This is true in particular of his discussion of the famous coup of September 30-October 1, 1965. After some wavering in the course of which he largely excuses Peking, Sukarno, and even the Indonesian Communist Party (PKI) from any significant responsibility for the coup, he comes very close (pp. 239-40) to exhuming the thesis of the so-called "Cornell Paper," written shortly after the coup by some of Mozingo's colleagues, which in turn parroted the PKI's initial line that the coup had been an "internal affair of the army." The PKI itself soon abandoned this line and admitted its responsibility for the coup, as Mozingo points out, and the Cornell Paper has long since been annihilated by writers such as Guy Pauker and Arnold Brackman (see the spirited correspondence on this subject in *Problems of Communism,* May-June 1976). In reality, the role of Peking, and still more of Sukarno, as well as that of the PKI, was far more important than Mozingo indicates. He attributes Peking's rather muted response to its colossal disaster-by-proxy in Indonesia to its hope that Sukarno would somehow save the PKI and restore the situation; this is probably true in part, but an even more important consideration is likely to have been Peking's desire to avoid an overreaction that might make things even worse for the Chinese community in Indonesia.

The conclusion contains an interesting, highly leftist, analysis of the United States's new model "imperialism" in the Third World. Here as elsewhere in the book the United States is almost never shown as acting from worthy motives, whereas Peking usually is. There is a frequent use of gratuitously pejorative language with respect to American policy in the Far East, which whatever its faults deserves more objective treatment than Mozingo gives it.

The bibliography, although "selected," is long enough so that it seems justifiable to point out some serious omissions, notably Antonie Dake's study of the PKI, Stephen FitzGerald's work on Peking's policy toward the overseas Chinese, Jay Taylor's important *China and Southeast Asia,* Guy Pauker's articles in *Problems of Communism,* and former Ambassador Howard Jones's *Indonesia: The Possible Dream.*

HAROLD C. HINTON

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Stalinism and After. By Alec Nove. (London: George Allen & Unwin, and New York: Crane, Russak, 1975. Pp. 205. $7.50, paper.)

The word "Stalinism" disappeared from the official Soviet vocabulary after Stalin's death in 1953, but has retained its hold in much western writing. It has been so handy as a term of abuse that authors rarely bother to define what they mean by it or to use it with precision. In typical usage the term is applied to some but not all of the institutions and practices associated with Joseph Stalin in power; likewise, it is applied to some but not all phenomena occurring after Stalin's death. We know who Stalin was, but we don't really know what "Stalinism" is, because so much depends on the eye of the beholder.

In view of the need for an analytic work clarifying the nature of "Stalinism" it is disappointing to report that Alec Nove's latest book does not fit the bill. We are offered something much less necessary, another (O Lord! how many?) brief history of the Soviet Union from the revolution to the present, with 67 pages on the prewar period, 34 pages on the war and postwar years to Stalin's death, and 70 pages on the period since 1953. It is an "easy read," to use the vernacular, because the text is peppered with anecdotes and reminiscences in the style favored by the *New York Times Magazine*. In fact, Nove might have produced a less routine history by making his account even more autobiographical than it is.

Though the work does not, I think, come up to the high standard set in Nove's previous books, mostly on Soviet economic developments, *Stalinism and After* has its virtues. Nove writes well, and gives a thoughtful, unpolemical treatment of developments in the USSR and of Stalin's role in them. The book may be suitable for assignment to students needing a quick ride through 60 years of Soviet history. But the price of the ride will probably deter widespread adoption of the book for this purpose.

A few errors or dubious assertions have crept in. Nove's treatment of the seventeenth Party Congress in 1934 exaggerates the extent of opposition to Stalin manifested there. In particular, Stalin was not reduced at the Congress from "General Secretary" to plain "Secretary" of the party. (Something like this did happen at the nineteenth Congress in 1952, though it is not clear that the move was taken against Stalin's will.) It also stretches credulity to connect the later repression of a large number of seventeenth Congress delegates and Congress-elected Central Committee members with an alleged attempt on their part in 1934 to clip Stalin's wings. Among minor errors, Nove states that the proportion of women to men in the 20–40 year age groups at the end of World War II was 2 to 1; it was more like 1.5 to 1. And the first artificial satellite was launched in October 1957 not the spring of 1957; the American U–2 was shot down a month before the abortive summit conference of 1960, not while Khrushchev was meeting Eisenhower in Paris.

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Rhodesia: Racial Conflict or Coexistence? By Patrick O'Meara. (Ithaca, N.Y.: Cornell University Press, 1975. Pp. 217. $10.50.)

Gwendolen Carter, who writes the preface to O'Meara's book, states that the particular value of the work is that it goes beyond a mere description of Rhodesian "pressure politics" (many aspects of which have, incidentally, already been covered in several recent and not-so-recent publications) to "set them within a frame of analysis based on conflict theory" (p. ix). It is against this claim that the value of the book should be assessed.

In his introductory chapter, "Towards a Framework for the Analysis of Rhodesian Politics," the author draws distinctions between different forms of conflict. "Political competition" is applied to rivalry between personnel within or between parties operating in the context of constitutional politics–illustrated, for example, by the rivalry for the leadership of the black parliamentary opposition. "Political opposition" relates to the pursuit of different policy objectives where this "institutionalized form of conflict serves a constructive end by translating issues into objectives which can be achieved within the system" (p. xx). In the Rhodesian context there are a wide variety of political parties or groups which fall into this category. They include ultra-right groups, the Rhodesian Front establishment, the more moderate multiracial Centre Party and the white-led Rhodesia Party, and the various black parliamentary opposition groups. "Opposition of principle" is defined as a "challenge to the continued existence of the system itself" (p. xvii). In the face of white Rhodesian intransigence "the Rhodesian African nationalists have moved from system to anti-system operations, and finally to an extra-system position of conflict" (p. 198). Rhodesia's political and constitutional history–from the pioneer period in the 1890s to the intensification of guerrilla activity which took place after the Pearce Commission had returned a negative verdict on the 1971 Settlement Proposals in May 1972–is interpreted against the theory of conflict outlined in the introductory chapter.

The historical dimension is indispensable to any analysis of the Rhodesian conflict, although it is not clear that the account in this book adds much to previous works. The author's preoccupation with political organizations and constitutional developments leads him to neglect the social, economic, cultural and other sources of conflict which are equally important for any theoretical explanation of the Rhodesian experience. O'Meara notes that conflicts can be subdivided further according to "scale–the degree of threatened disruption; scope–the size of the units involved; pervasiveness–the number and kinds of relationships disrupted; duration; and, finally, manifest expression–the specific overt activities involved" (p. xxi). It would have been useful if the book had illustrated more thoroughly how these categories applied to the various aspects and levels of conflict in Rhodesia. In particular there is very little attention paid to the way in which guerrilla activities have developed. A comprehensive theory of social and political change should, surely, make use of such concepts and analytical techniques as alienation, communications theory and relative deprivation–this last factor is briefly noted (p. xxiii) but carried no further. There is some limited discussion of the objective conditions of discrimination in the economic, legal and political spheres, but there is little analysis of how these conditions relate to the subjective perceptions of the different groups and strata within either the black or the white communities. The chapter on the status and role of the chiefs is a partial exception to this criticism.

A few specific matters should be noted. It is wrong to refer to the famous six points on which the series of Anglo-Rhodesian independence negotiations were conducted as the "NIBMAR Principles" (p. 18). It was only after the Rhodesian rejection of the December 1966 *Tiger* Proposals that the British Labour government stated that "in future no grant of independence will be contemplated unless African majority rule is already an accomplished fact" (i.e., NIBMAR–No Independence Before Majority African Rule). This pledge was broken by the Labour government with the *Fearless* Proposals in October 1968 and by the Conservative government over the 1971 Settlement Proposals. It is also incorrect to say that Rhodesia's export markets were declining by 1971 (p. 41). In fact, exports rose rapidly between 1970 and 1974. While it is true, as O'Meara points out (p. 46) that Lord Pearce, the Chairman of the Pearce Commission, had no previous experience of Africa, it is also worth noting that Lord Pearce gave the only dissenting opinion in the Judicial Committee of the Privy Council's ruling on the "Madzimbamuto" case in July 1968 which confirmed the illegal status of the Rhodesian regime–a fact which inevitably, though in the event unjustifiably, gave rise to misgivings about the impartiality of the commission.

The most interesting and valuable part of the book is that dealing with the black opposition to parliamentary parties which have been somewhat ignored in other works. As O'Meara argues, they are significant insofar as they are used by the Rhodesian Front regime to provide a "safety valve" function and to confer a veneer of respectability on the parliamentary process. The major shortcoming of the book is not so much that it deals rather superficially with conflict at the formal level of organizations and constitutions as that it almost completely ignores the objective and subjective factors which account for collective behavior at the less formal level of the mass constituency, both black and white. There is certainly a need for theories of social and political change to be used in the any analysis of the Rhodesian situation and Patrick O'Meara has at least taken a tentative step in this direction.

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International Theory and European Integration. By Charles Pentland. (New York: Free Press, 1973. Pp. 283. $9.95.)

Seemingly unprecedented developments in European statecraft after World War II, culminating in the formation of the European Economic Community and subsequent efforts to transcend the nation-state with supranational institutions, ushered in a wave of theory in the field of international relations. Much of this literature selected the unfelicitous word "integration" as an organizing concept to find the proper preconditions for international cooperation which might lead to transnational institutions.

This book seeks to codify four strands of "integration" theory, with special reference to the interstate politics of Western Europe since the late 1940s. The book is divided into two parts. In the first part the author surveys the major assumptions and propositions of pluralism, functionalism, neofunctionalism, and federalism, citing respectively the works of Deutsch, Mitrany, E. Haas, Friedrich, and writers with similar preferences for each of these four alternative pathways toward international cooperation. In the second part the author uses Waltz' three levels of analysis (the individual, the state, and the international system) as categories to sort out alternative propositions for each of the four approaches.

Written in the style of Inis Claude and Stanley Hoffmann, the book is unquestionably a classic insofar as it pulls together the many strands of a vast literature, noting similarities and differences between and within each theoretical tradition. The strong point of the book is that it is an authoritative summary and intellectual history of an important focus in the field of international relations.

The deficiencies of the book are therefore representative of the difficulties in the literature surveyed, so we should perhaps observe that Pentland has undertaken to explicate the literature, and he could obviously not reconstruct it or test it in the light of empirical research, which he leaves to others. But "integration" theory is a soft underbelly of the discipline, and this volume well illustrates why.

The first problem is that the author has been unable to define "integration" in a consistent manner, variously referring to it as "process" (p. 190) or as "a property" (p. 192). Indeed, I have counted some 30 different definitions of the term throughout the literature, and I am appalled as I see it applied to so many disparate phenomena. The simple definition that "integration" refers cybernetically to a communication link (as opposed to a nonlink) makes sense in information theory; what has happened in political science is an obfuscation and imprecision which Pentland fails to correct. The term "international cooperation" seems to be a more appropriate unbrella concept, since there can be no self-delusion that it is anything but a generic term.

Pentland's skillful enumeration of four paradigms of "integration" theory has excluded a fifth approach, which will doubtless be addressed more vigorously in future research, namely, imperialism. The most obvious method for transcending the nation state, one which historically is most numerous, is for one state to overpower and to annex another. Whether we trace this theory to Hobbes, Mazzini, Simmel, or to Marx, we find that such academicians as Etzioni agree that unification is an extremely easy process for states with overwhelming power at their disposal. This may not be "international cooperation" but it is certainly "integration" within the meaning of several of theorists surveyed by Pentland. Indeed, according to Galtung, we may interpret the effort at supranationalism in Europe as motivated by the desires of bourgeois elements in Western Europe to carve up the markets of the world, to fulfill an ambition for economic imperialism after the failure of political imperialism.

By focusing on Europe, Pentland is unable to see the necessity for incorporating this fifth paradigm into his volume. Within Third World countries, however, it is no secret that the European Economic Community and related institutions are establishing economic control to the detriment of indigenous efforts to become developed economies. If there is anything that countries of Southeast Asia do not want it is "integration," because that term implies that they will be annexed economically by the multinational corporations of Japan, having sought by war to get out from under the Greater East Asia Co-Prosperity Sphere. For ethnic minorities within these same countries, the term "integration" means that the dominant majority wishes to impose assimilationist norms and to break up ethnic enclaves. In a world where China provides a model of autonomy, "integration" may be decreasingly useful as a rallying point for those wishing to bring prosperity closer to the people.

A final weakness in the book is that the discussion relies on institution and anecdotal situations rather than systematic evidence. A codification of research findings in this area of study not provided in the volume. *International Community,* written by Cobb and Elder in 1970, is one of the many studies which the author fails to review. If there is any way out of the abyss of verbal solitaire which "integration" theory represents, it will come through systematic research to test propositions. A codification of the findings of empirical research could then serve a useful purpose. Unfortunately, the author leaves the reader with the impression that no such research has produced clear findings that might shed light on the subject, and this is not the case.

Whatever the weaknesses of the book, Pentland is to be congratulated for providing a summary of pretheory that can serve as a clearly written introduction to theories of international cooperation. If there is any hope for the field of international relations, this volume will in due course be viewed in Comtian terms as the culmination of the metaphysical stage for the science of international relations, opening the way to progress through empirical and systematic research.

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United States Diplomats and Their Missions: A Profile of American Diplomatic Emissaries since 1778. By Elmer Plischke. (Washington: American Enterprise Institute for Public Policy Research, 1975. Pp. 201. $4.00, paper.)

This little volume by Elmer Plischke, of the

University of Maryland, should prove useful for the lecturer who deals with American foreign relations. It provides in quantified form considerable information about the 2,926 chiefs of resident diplomatic missions, chiefs of mission to international organizations, ambassadors at large, and "top-level Department of State officials" who served the nation from 1778 to March 31, 1973. In eight descriptive chapters, Plischke takes up the historical development of United States diplomatic representation, types of appointments and appointees, geographic origin, sex and age of diplomats, their ranks, titles and career status, their political ambitions, and their collateral interest as authors. Finally, in 16 appended tables, we are given the hard data upon which the eight chapters were based. As the author notes, almost all of the information in the tables and text was taken from the 1973 Department of State publication, *United States Chiefs of Mission, 1778–1973,* prepared by Richardson Dougall and Mary Patricia Chapman of the Historical Office of the Department of State.

As might be expected, Professor Plischke's interest in writing this book appears to have been twofold: he had information which he wished to convey, and he hoped someone might make some use of it. While his chapters do not bristle with didactic insights, he regularly draws attention to important trends or results which, if considered thoughtfully, could be used to improve the nation's representation abroad or, possibly, the management of its foreign relations. Occasionally his information is fairly startling; but, too often, the commonplace is given more attention than this reviewer feels is warranted by the space allotted to the publication. For instance, from Table A–10 ("Duration of Diplomatic Missions to Foreign Governments") we can learn that of 1,932 diplomats who served one or more years as a chief of mission, 409 lasted three years (or 21.17 percent of the total). On the other hand, of the 222 emissaries who served less than a year, three lasted only a month (1.35 percent) and 45 made it through eleven months (20.27 percent of the 222). Except for a display of dexterity with a calculator, it is hard to find any genuine value in such data.

More positively, though hardly earth-shattering, the following facts are useful to know: "Although represented at approximately 100 international conferences prior to 1900, averaging less than one per year, current United States conferences attendance amounts to an annual average of 350 to 400" (p. 21). For almost 200 years, American diplomatic appointments "generally ranged between 2.5 and 3.5, and averaged approximately three appointments for each country each decade" (p. 26; Tables A–4, A–5). From Table 2 ("Average Duration of Diplomatic Missions by Geographic Area") it can be seen that the average mission in Western Europe lasts 3.36 years but only 2.66 years in Sub-Saharan Africa (p. 54). Concerning geographic origins of diplomats, Plischke concludes that "factors other than geographic origin influence presidential appointment of diplomatic chiefs of mission" (p. 76). Also, "while fluctuations have occurred in the ratio of careerists, the trend since World War II has clearly been in favor of increasing the quantity of career diplomats, varying from two-thirds to three-fourths. The issue of careerism versus noncareerism appears to be of diminishing importance" (p. 105).

In chapter 6, "Presidential Aspirations and Congressional Service of Diplomats," Plischke expresses a sentiment with which many academics might enthusiastically agree: "This raises the issue as to whether the time is not at hand when the secretary of state, on appointment, should be considered primarily in terms of his qualifications in foreign affairs management and not because of his political importance or constituency—or, to put it another way, whether he should be essentially apolitical rather than a political candidate for high elective office" (p. 109). Of course, it has been a long time since Jimmy Byrnes was salted away in Foggy Bottom, but the advice is probably worth passing along.

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A Peace Denied: The United States, Vietnam and the Paris Agreement. By Gareth Porter. (Bloomington: Indiana University Press, 1975. Pp. ix + 357. $15.00.)

During the war in Indochina Gareth Porter was one of the most concerned of the Concerned Asian Scholars and one of the most resourceful at the Indochina Resource Center. Now he is first to produce a detailed historical account of what he considers "the failure of diplomacy to bring peace to Vietnam." He does so, as his preface indicates, with a desire to report previously obscured facts and to assess responsibility for the prolongation of warfare. The result is a book that abounds in valuable research but confounds with its constricted interpretations of American behavior. The book is more than a welcome antidote to previous distorted reporting of the diplomacy concerning Vietnam; it is a smooth, relentless lawyer's brief for the patient strategy pursued by Hanoi. As such it is a valuable work, but the subject can still profit from the efforts of a less engagé observer, as well as a more theoretically oriented one.

The first two chapters contrast the expedient use of negotiation by the Democratic Republic of Vietnam (DRV), from its origin in September 1945 to the war's turning point in 1968, with the United States' lack of interest in any negotiated solution. Three chapters then detail the four years of "talking while fighting" at Paris, and two are devoted to the non-implementation of the Paris Agreement. The final chapter describes the shifting of the military balance in favor of the Provisional Revolutionary Government (PRG) up to the eve of the 1975 offensive, which caught the author on the way to press with only a sketchy epilogue in hand.

The book has decided strengths. The author knows the Vietnamese language, uses untranslated sources well, and deals authoritatively with linguistic subtleties which caused controversy during the negotiations. Porter writes fluently, clearly, and sometimes even beguilingly. His summaries at the beginning of chapters are first-rate. He makes a major point, in refuting the U.S. government's interpretation of the Christmas '72 bombing of Hanoi. He calls it a defeat for Nixon in that he used up his last major bargaining chips to no avail and put himself under even greater pressure to reach agreement without delay. He notes that the U.S. justified its bombing in Cambodia after the Paris cease-fire agreement with an argument that Article 20 set no time limit for foreign withdrawals, yet demanded that the North Vietnamese presence be eliminated in accordance with the same article. He lays bare the sharp practice of the U.S. government in transferring ownership of bases and military equipment to the South Vietnamese to avoid the dismantling requirements of the agreement. He makes clear the secret commitments by Nixon to Thieu that the U.S. would react with "full force" to any North Vietnamese violations of the cease-fire. President Thieu's dissembling in interpreting the agreement to his people is spotlighted. Thieu's record is further put in question by reference to his government's preventative isolation of the PRG and DRV delegations in Saigon after the cease-fire, its unyielding interpretation of PRG zones of occupaion as only "temporary," its unwillingness to have rival local commanders even meet to perfect the cease-fire, and its false claims that the Communists were planning a "general offensive" in spring 1974. As for the International Commission of Control and Supervision, the author highlights its impotence while conceding that the Poles and Hungarians protected their friends even more than did the Canadians and Indonesians.

Yet for all its many strengths, Porter's book has bothersome features. The author frequently attributes motives to Nixon and Kissinger, toward whom he shows more malice than charity, without documentation or qualification. He asserts that Nixon's secret letter to Thieu and public interpretations of the agreement indicated to Thieu that he "could violate the agreement openly" with Nixon's backing. He subtly makes the reader into a cohort in distrusting the U.S. government by phrases such as, "but of course" to preface his discounting of U.S. statements or proposals. Unlike Tad Szulc, in *Foreign Policy* (Summer 1974), he gives slight attention to Kissinger's use of Soviet and Chinese channels to the DRV, or to the impact of the pending U.S. election in 1972, or the drag of Watergate on President Nixon's leeway after the agreement. Porter also contradicts Szulc, who attributed the delay in signing an agreement after Kissinger's "peace is at hand" statement to Thieu's intransigence, not, as Porter does, to a deliberate Nixon and Kissinger strategy.

In setting forth the DRV's multifaceted strategy, however, he occasionally ends up in self-contradiction, e.g., the DRV waged "full-scale war" while being willing to settle diplomatically in 1964; the Tet Offensive aimed at a "General Uprising" but this really meant a "prolonged strategic offensive"; the NLF "had no interest in proposing any compromise short of its ultimate objective" but its plan offered "considerable area for negotiation and compromise."

Occasionally his euphemistic phrasing catches the reader's breath. Thus the DRV's decision to launch a spring 1972 offensive was simply an attempt "to change the military and political context of the negotiations." Or, allowing for no anticommunist sentiment among more than two million refugees who fled from NLF-PRG-controlled areas, he attributes their movement simply to the bombing, artillery and the physical danger and economic hardship that "the war" imposed on them.

The DRV/PRG position is never without merit. Its shift from three years' insistence on the ouster of Thieu to the acceptance of the RVN and PRG as equals is called proposing their "minimum demand," as though it had been on their mind all along. If some PRG officers turned up after the cease-fire wearing

DRV uniforms, it was only because initially they felt more secure vis à vis the RVN in such attire.

It is ironic that this full account of the Paris Agreement gives almost no attention to the two articles that remain in contention today; Article 8 on provision of information and remains of Americans missing in action, and Article 21, which committed the U.S. to provide reconstruction aid to Indochina. When Porter wrote, neither he nor other Americans outside the White House, knew of President Nixon's letter to the DRV Prime Minister discussing a $3.25 billion aid package to be offered "without political conditions." With the surprise military walk-over of April 1975, however, the issue for the DRV has shifted quickly from the "peace denied" to the reparations denied.

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The Politics of Aid, Trade and Investment. Edited by Satish Raichur and Craig Liske. (New York: Halsted, Wiley, 1976. Pp. 218. $17.50.)

This second volume in the Comparative Political Economy and Public Policy Series contains eight chapters on a variety of aid, trade and investment topics. The contributions indicate as much about the relative merits of different approaches to the study of politics and economic policy as about substantive issues.

The chapter by Richard Van Atta and Dale B. Robertson uses "social-field theory" to determine "to what extent Soviet trade and aid can be accounted for by the position of the USSR relative to other nations in the international system" (p. 8). Attribute differences between the USSR and other countries account moderately well for Soviet aid and trade in 1955, but the analyses yield much weaker explanations, especially for aid policy, in 1963.

The authors conclude that "field theory is not itself adequate to assess behavior which is contingent upon the behavior of another actor in the system" (p. 32). They suggest modeling the "competitive inter-relationship" of the U.S. and USSR, but models which account for cold war behavior are probably not very useful for examining the mixed competitive-cooperative politics of the current era.

The weaknesses of simple explanations of foreign policy are illustrated in chapters by Frederic S. Pearson and John H. Petersen. Both test economic interest explanations of U.S. foreign policy. Pearson offers three explanations of U.S. policy (economic-imperialism, anti-communism and strategic interest) to test the "effects of economic interests on [U.S.] military intervention decisions" (p. 37). He finds that "no single variable explains much of the variance in U.S. interventions," although "friendly interventions" for strategic purposes occur most often (p. 53). Petersen finds that "private economic interest appears to be the most important factor in U.S. foreign policy behavior in Latin America" (p. 80). But Petersen uses simple correlations between U.S. exports, imports and investments, and the number of U.S. diplomatic personnel and amount of economic and military assistance in particular countries. His correlations show that "trade and the flag go hand in hand" (p. 75), but give no basis for determining which goes first. Petersen fails to account for such factors as promotion of exports through tied aid and the effect of country size on the magnitude of both the diplomatic mission and economic transactions.

Three chapters have relevance for the policy choice between multi-lateral and bilateral aid. James T. Bennett and Miguel A. Guzman use regression analysis to find that the allocation of aid by the World Bank and Inter-American Development Bank during 1971 reflected economic need, performance and credit worthiness of the recipients, not the political orientation of the recipient. A political allegiance variable based on voting in the OAS General Assembly did not contribute significantly to their equation.

Michael Rock tested alternative approaches to the theory of foreign direct investment and found that investment tends to follow existing export markets. Because tied aid promotes U.S. exports, he argues that "if a basic objective of aid is to foster development through capital inflows, this can best be achieved by the United States continuing to grant tied aid on a bilateral basis" (p. 119).

G. Robert Franco used a stochastic model to estimate the trade effects if the U.S. and 15 other countries commit themselves to untied multi-lateral aid. He found that "every donor country except the United States would tend to gain [directly in exports] from an untied aid system" (p. 157). Franco makes an important contribution to an understanding of the politics of aid policy.

The final two chapters represent more traditional, non-quantitative approaches. Charles W. Hultman examines the prospects for international agreements to assure U.S. access to an uninterrupted supply of commodities at reasonable prices. Hultman points to the inconsistency between U.S. export control policy and the "access to supplies" approach, and observes that this fact, together with the demands of developing, commodity-exporting countries for greater control over and larger returns from their resources, increase the risk of nationalistic unilateral actions.

Steven H. Arnold compares Brazilian export promotion policies under the democratic regime (1956–1964) and the authoritarian regime (1964–1974). Although he credits the military regime with stabilizing the economy so as to make possible effective export promotion policies, Arnold contends that much of the success achieved was the result of the fortuitous selection of politically skilled finance ministers by the military regime.

ROBERT W. RUSSELL

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The Public Order of Ocean Resources: A Critique of the Contemporary Law of the Sea. By P. Sreenivasa Rao. (Cambridge, Mass.: MIT Press, 1975. Pp. xi + 313. $27.50.)

Now Deputy Director of the Legal and Treaties Division of the Indian Ministry of External Affairs, the author initially presented this marine resources study as a doctoral dissertation to the faculty of the Yale University Law School in 1970. The book under review is an adaptation and extension of that earlier study.

Rao's organizing framework is derived from the work of Myres S. McDougal and Harold D. Lasswell, both professors at Yale while Rao was a student there. McDougal and Lasswell's policy-oriented approach to the study of international law adopts a systemic perspective which focuses attention on the dynamics of international problem solving and rule making. Employing this approach, Rao briefly identifies and describes the marine resources at stake, the competing claims and counter-claims concerning their ownership and development, and what he calls the "process of decision" for resolving problems and making rules. However, the principal strength of his work is not as a policy study, but as a collection and categorization of data on how marine resource problems have developed and how the international political system has dealt with them. There is little in Rao's book that will be new to anyone who has followed developments in international law of the sea; however, many readers will find his work useful as a sourcebook. He has made extensive use of a variety of difficult to use, often fugitive, United Nations materials, mining from them position statements made by participants in the International Law Commission, law of the sea conferences, and seabed committees. Somewhat more selectively, he identifies the interests expressed and positions taken by a range of nongovernmental actors with a role in marine resource policy making. And, finally, he reviews the work of legal scholars and those who have written about marine resources.

Despite these significant strengths, Rao's book is disappointing as a policy study, primarily because he appears to have applied his framework somewhat mechanically and consequently fails to convey any real sense of the dynamics of international problem-solving and rule-making. To be fair to Rao, his objectives may have been limited to collection and categorization. But one could use his data and categories to learn: (1) that governmental, intergovernmental, and nongovernmental participants in international politics have proliferated over the past 30 years or so, introducing into the system a great deal more diversity in values and interests; and (2) that while these structural changes have been in progress the potential value of marine resources has greatly increased, primarily because the development of new or improved technologies have made them accessible. But Rao leaves it to the reader to conclude that it is largely these structural and technological changes that have challenged the problem-solving and rule-making capacity of the international political system. In the context of a policy-oriented study, his more fundamental omission is the failure to make explicit what his study clearly shows, that the system has not ignored this challenge but has taken organized, purposeful responses that date back at least to the 1950s when the International Law Commission was charged with drafting a law of the sea treaty which would codify existing rules and, where needed, progressively develop new ones. In fact, his data collection and categorization basically begins at this point. And he does implicitly show the dynamics of the system by describing what has taken place within the United Nations, its committees, and law of the sea conferences. But a broader policy perspective is needed to see that, as a substantive problem area, marine resources provides an almost laboratory experiment-like view of the problem-solving and rule-making capabilities of the international political system.

In summary, then, Rao has produced a useful book in which he presents a large amount of data on marine resource problems separated into the categories of a policy-oriented framework. It would be a more beneficial contribution to the study of international law if Rao had been less mechanical and more interpretative in the application of his framework.

Irvin L. White

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Asia and the Road Ahead: Issues for the Major Powers. By Robert A. Scalapino. (Berkeley: University of California Press, 1975. Pp. 347. $10.95, cloth; $3.95, paper.)

Although this book is structured around six societies, its main thrust is to discuss policy options for the United States in Asia in the wake of the Indochina debacle. Long being one of the most articulate spokesmen for a big-stick policy against Communism, Mr. Scalapino amassed an impressive number of arguments, pro and con, to demonstrate the folly of the United States' contemplating a military withdrawal from that part of the world. In terms of theory, the author breaks no new ground. From start to finish, he adheres strictly to the traditional approach of balance of power and geopolitics despite the attempts made to touch bases by alluding to certain concepts of internationalism which cut across ideological and political lines.

Much of the analysis of the internal politics of China has been invalidated by the swift-moving events in that country from the deaths of Chou, Chu and Mao to the ouster of the "gang of four" in the course of ten short months between January and October of 1976. In surveying Sino-Soviet relations, however, the author is to be congratulated for pointing out the depth of their schism which probably can not be overcome, short of internal revolutions either in one or both countries.

However, the view that Peking relies mainly on balance of power politics and nationalism for policy making is open to debate. There is no question that China's foreign policy is influenced by considerations of both nationalism and Communism. Indeed, the only kind of Communism which the Chinese leaders know of is that which they borrowed from Lenin and Stalin, a Marxist system made compatible with the nation-state and social backwardness. The only kind of revolutionary experience they have had is the one which fights for the triumph of the communist system in every nation-state while defending their own national territorial base. They quarreled with the Russians over the goals and methods of Communism. Only when their party-to-party relations were damaged beyond repair did they come to maintain limited contact with each other on the state-to-state level. Since they accepted the split with the Russians, they have been as much concerned with preserving the communist character of their system at home as with the defense of China's security.

On the other hand, China's relations with the United States have been geared almost exclusively to matters of nation-state, such as recognition, equality, sovereignty, territorial integrity, and above all the legitimacy of the communist rule in China. It is inaccurate to maintain that in the early seventies there was a "dramatic Chinese volte-face toward the United States" for Peking had sought consistently but vainly since 1955 the establishment of formal diplomatic relations with the United States against an inflexible policy of nonrecognition and nonintercourse laid down by Dulles. The record of the Geneva and Warsaw talks for over a decade testifies to this consistency. Quite the contrary, it was Mr. Nixon's decision in 1971 to replace Dulles' policy with one of negotiation and peaceful coexistence that enabled China to reenter the world and made the limited detente possible. That Mao appreciated this American policy reversal was amply demonstrated in the fading twilight of his career.

It is equally questionable to hold that China's "tilt" toward the United States was based on its need for American power to offset the Soviet threat. The American record of helping one communist country against the invasion by another has not been such as to warrant false hopes by the Chinese. There is no reason to believe that the Chinese leaders have at any time counted on American power, political or military, as a lever to solve their problems with the Soviet Union. However, a lessening of China's commitment to ideology may cause such policy shifts in the future as to make American power a factor of Peking's policy calculations. But unless the United States can persuade the new Chinese leaders to forsake Taiwan for American military assistance, the likelihood of China's counting on the United States as an ally against external attack is virtually nonexistent. On the contrary, if we pursue a policy of an independent Taiwan and divided Korea as the author urges without a quid pro quo with Peking, we can be sure that China will always regard America as an adversary.

On the whole, the author sees no threat to peace in Asia. Yet he is concerned with the alleged "Chinese hegemony" in the region, arguing for the need to keep a United States military presence there as a matter of maintaining America's credibility. The lessons of Vietnam, according to him, are not that military intervention is ineffective and sometimes even counterproductive (Cambodia) as a means against communist insurgency, but that next time when the United States risks a limited war "the response should be swift and overwhelming" so as to insure victory in short order. He seems to regret that the United States committed the error of "military incrementalism" in a war which was lost mainly because of a lack of staying power by Congress and the American public.

The end of the Vietnam era is indeed a time for momentous decisions. Few advocate U.S. withdrawal from any part of the world, but the time has surely come for a reexamination and rearrangement of America's commitments and priorities. Military alliances, air bases and lethal weapons, created by a foreign policy, based on a geopolitical, horizontal and territorial approach to international affairs, have failed either to provide us with security or to prepare us to meet the new challenges of our time. These challenges are essentially vertical (rich vs. poor), socioeconomic and transnational. The call for replacing the policy of intervention with cooperation is real and well justified. Factors, groupings and relationships which form the basis of world politics have changed. Ideologically oriented revolutions are on the wane; the major powers have suffered a loss of influence because of their distorted proclivities; and the exigencies of modernization and development will probably shape the relations between the advanced and backward societies for the next century. The world's problems are now so vast and complex that it is beyond the capacity of any single state to offer meaningful and constructive solutions. The Soviet Union and China must also fall in and seek a common way out or face isolation and ostracism. Under these circumstances, multilateral efforts by both industrial states and developing nations to engage in long-range planning, to head off human disasters and to make the world a better and safer place to live in, are certainly in line with the "selective internationalism" which the author advocates. In rearranging its national priorities, therefore, the United States may have to shift its emphasis away from military-oriented objectives. If this is isolationism, let's make the most of it.

IKUA CHOU

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U.N.E.S.C.O. and World Politics: Engaging in International Relations. By James P. Sewell. (Princeton, N.J.: Princeton University Press, 1975. Pp. 384. $18.50.)

This study has appeared at a time when the UN system as a whole is being critically appraised for its efficacy in the promotion of peace and the general welfare of mankind. Although UNESCO is his focus, Sewell's "underlying concern is whether, how, and by whom existing global relationships might be reordered or improved. . . . To learn why intergovernmental organizations function so poorly, we must investigate how they function actually. By this means we can fathom the conditions for their effectiveness" (p. 3). His hope is that the detailed study of UNESCO will throw light upon the conditions for increasing the effectiveness of multilateral agencies generally, including the others of the UN system.

Sewell's general conclusion seems to be obvious: the effectiveness of international organizations depends, as in the case of UNESCO, upon the degree of commitment and will which governments of member states manifest toward meeting their obligations under the charter and the other international constitutions, and upon the extent and nature of their efforts toward making these organizations carry out their assigned purposes. Obviously, too, the effectiveness of the organizations depends upon the role given them by member states within the prevailing framework of world politics.

Central to the author's examination of how UNESCO functions is his identification of two special characteristics. One of these is the extraordinary multiplicity, breadth and variety of its concerns: the promotion and development of human rights, education, science, culture, and communications as factors conditioning the prospects for nation-state development and world peace. The other special characteristic is the intended engagement, in UNESCO's policy-making and program activities, of great numbers of interested private individuals and non-governmental organizations as well as of national governmental institutions and of the decision-making centers within UNESCO itself (directors-general, secretariat, executive board and general conference). The complicated interplay of these elements has no doubt substantially limited UNESCO's effectiveness, but it has also brought strength and considerable progress in many areas of UNESCO's activity. Sewell makes an important contribution to the literature concerning UNESCO by examinating how the process has affected decision making on policy, program and administration. He has especially noted important changes that have taken place under successive directors-general toward achieving a decision-making process and an administrative system that could increase the organization's efficacy within its constitutional limits and the requirements of world politics. Unfortunately, for fairly obvious reasons, Sewell has not been able to examine developments in individual nations' policies toward UNESCO. A study of U.S. policy is particularly necessary to explain our declining role after an enthusiastic initial display of support for the organization.

Much of what Sewell presents here about UNESCO's functioning as an intergovernmental organization will be help us understand the record of the other multilateral organizations. Yet few of these organizations are as complex as UNESCO in their scope and in the extent to which they deliberately seek nongovernmental involvement in their activities. This study's primary contribution is to provide a better understanding of the functioning of UNESCO itself.

Certainly Sewell's is much the best of the studies of UNESCO that have thus far appeared. In my view, however, Sewell's efforts to construct a new theoretical framework for the study of international organizations seems to obscure, rather than clarify how UNESCO has functioned, but further efforts to apply it may show its utility more clearly.

Meanwhile there is much in this volume that should help member states develop national policies which will further the organization's constitutional objectives and give it a more significant place in world politics. Especially the United States needs to ponder how its participation in UNESCO can be made more effective; at one time we promoted it as central to the United Nations world order. For example, we might situate our UNESCO policy firmly within the strategy of our total foreign policy; we could place responsibility for our UNESCO policy and action in the hands of competent specialists in the executive branch, whose work is coordinated with other aspects of our foreign relations; we could assure that the most highly qualified persons available represent the United States in UNESCO deliberations and activities; and we could make the American people understand the importance of UNESCO to our national interest and the nature of the obligation as well as the opportunities involved in our membership in international organizations.

Both the U.S. government and the many nongovernmental sectors in education, science, culture, communications and civic affairs functionally related to UNESCO obviously need to work harder at making UNESCO more effective. The real responsibility for most of the shortcomings of UNESCO and its seriously deteriorated condition rests upon them and their counterparts in other member states.

WALTER H. C. LAVES

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Asian Neutralism and U.S. Policy. By Sheldon W. Simon. (Washington: American Enterprise Institute, 1975. Pp. 111. $3.00, paper.)

We have come to expect a rare combination of rigorous scholarship and deep substantive concerns in the works of Sheldon Simon on East Asian politics and international relations. The present book, though brief, is no exception. Growing out of an interview and research journey through Asia in 1973, updated by analysis of events in and affecting the region through mid-1975, it spells out basic considerations which should help shape U.S. Asian policies in the wake of the Indochina collapse. America's long overdue new orientation toward the region, symbolized in the Nixon Doctrine, has been in part responsible for the rise of a searching, still hesitant Asian neutralism. The latter relies for solutions to Southeast Asia's security problems primarily on the regional actors themselves, and seeks the permanent elimination of great power interventions in the region's continuing ethnic and other internecine conflicts, as well as in its efforts to construct regional cohesion. Simon recognizes that if the neutralization of Southeast Asia occurs, whether through a system of external guarantees or mutually off-setting internal balances, it will perhaps be in larger measure owing to the intensifying Sino-Soviet conflict than to the diminishing efforts of the U.S. to exert its influence.

Simon is realistic in acknowledging the frailty of the regionalism now emerging and places only modest faith in its capacity to maintain Southeast Asia's security interests. Nonetheless, he sees progress emerging from the efforts of ASEAN and joins expressions of hope by many of his Asian contacts that the great powers will refrain from further interventions in a region where, as Simon quotes the Indonesian statesman Sudjatmoko, "stability at the present level of poverty is unattainable" (p. 106). As to Northeast Asia, Simon tends to downplay if not ignore the problems of Japan's future orientation, while he judges the prospects of political settlement in Korea to be less promising than they appeared earlier.

Much indeed depends on how the great powers judge and play their roles: Simon's view is that the CPR seems reconciled to the concept of Asian neutralism, about which it is in any case not in position to do much; while the USSR, aside from occasional reiteration of its call for a collective security pact, is doing little, even in the Indian Ocean basin, to establish a dominant or highly assertive posture. The U.S., on the other hand, although basically disengaging itself in accordance with the Nixon Doctrine, has not fundamentally altered its excessive base deployment (except in Thailand), nor rethought the defense doctrine underlying its MAP, nor reviewed its fears that if it does not fill the Indian Ocean "vacuum," its rival will. The author pleads for more serious consideration of a shift on the part of the U.S., bringing the Soviet Union along, from competitive naval deployments and security strategies, tending to repeat past compulsion for alignment by the smaller Asian states, to more limited strategies of economic access better attuned to the true security needs of the post-cold war future.

While praising the author's thorough documentation, I regret the absence of an index and a bibliography.

PAUL M. KATTENBURG

University of South Carolina

Secrets of the Fascist Era: How Uncle Sam Obtained Some of the Top-Level Documents of Mussolini's Period. By Howard McGaw Smyth. (Carbondale: Southern Illinois University Press, 1975. Pp. 305. $15.00.)

The author of this study, whose recent death is a grievous loss to his friends and colleagues, and to scholarship in modern Italian history, was the outstanding expert on those records of Fascist Italy, political, military and diplomatic, which were brought to light during and immediately following World War II.

This present work is both a minutely documented study of the searches for the archival material, and a dramatic account of the conditions of the quest. The ultimate discovery of certain collections of these Italian records is revealed as a series of episodes of historical detection—the uncoordinated and at times fortuitous work of Allied teams operating with varying and—at times—contradictory directives.

Although the terms of the Italian armistice of September 1943 stated that "The Italian government will supply all information and provide all documents required by the United Nations," the Civil Affairs Branch of the Allied Control Commission in Italy persistently worked for the early transfer to the Italian authorities of all documentary material unearthed in former Fascist offices or hidden caches, protectively treating them as ancient monuments to be preserved.

The immediate interests of intelligence and security had to be asserted in light of such resistance. Any concept of the historic value of such records was conspicuously absent. For example, the records of the Fascist Ministry of Popular Culture were found almost intact in Rome in June 1944, and were processed by the Allied Political Warfare Branch with the original intention of seeking evidence on which to screen Italian journalists and former officials of the Ministry for employment by Allied agencies. These massive archives were only later studied for more general purposes and selected files sent to Washington and London.

The Fascist Foreign Office archives were an obvious target for investigation, but the status of co-belligerency protected the Badoglio government from any enquiries into such archives as remained in Rome at the moment of liberation.

By accident it was discovered that certain key files had been removed, at the time of the Italian surrender, to the Italian Embassy in Lisbon. For the first and only time, the American authorities insisted that these documents should be temporarily handed over for inspection, and thus part of the top-level records of the Cabinet Office of Ciano as Foreign Minister came to light. They are meticulously indexed in this present study.

The odyssey of Ciano's diary (smuggled into Switzerland by Edda Ciano at the time of her husband's execution) is traced by the author with equal and thorough skill. New light is thrown on this episode based on Allen Dulles' papers relating to discovery of the manuscripts by his office.

The two main categories of Italian archival material, however, had been removed out of range of the Badoglio and Allied authorities. The files of Mussolini's secretariat, and the scattered archives of the ministries of the neo-Fascist rump Republic of Salò fell into the hands of Allied teams (PWB and G–2 of the Fifth Army) in the chaotic conditions of the last days in Northern Italy. Forty-two mail pouches were sent to Rome and very selectively filmed. None of the original material was removed at any stage from Italy.

The second collection represents the massive documentation of Italian military records removed by the Germans after September 1943, and housed at a special documents center in Bavaria for study by a special team of German archivists. This service was captured and ultimately removed to Washington, where it was analyzed by McGaw Smyth, who has rendered a most valuable service to scholars and students in preparing this major category of Italian material for record on microfilm, now held and made available by the National Archives in Washington. The presentation of these surviving collections of Italian political and military records, and the detailed technical analysis contained in this study is an unique contribution to Italian historiography.

The tantalizing gaps in these records, in particular the unresolved fate of Mussolini's personal archives, have been touched on by the author. The dubious "secondary" sources he quotes, for example, in his account of the Treasure of Dongo, and of the notorious myth of a Churchill-Mussolini correspondence—mainly invented in fact for internal electoral purposes in post-war Italy—seem perhaps a little out of place in this archival study. But they are surely intended as light relief.

F. W. DEAKIN

Var, France

1975 Yearbook on International Communist Affairs. Edited by Richard F. Staar. (Stanford, Calif.: Hoover Institution Press, 1975. Pp. 678. $25.00.)

Published on June 1, 1975, this important version of the customary *Yearbook on International Communist Affairs* is a major contribution to the solid body of information and increasing knowledge on the International Communist movement. Both the editor, Richard F. Staar of the Hoover Institution, the publisher, and the numerous editorial associates and contributors, should be complimented on producing such a significant and lasting work in this particularly murky and complicated field. In preparing the 1975 version of his *Yearbook,* Staar was aided both by a distinguished Advisory Board, including Brzezinski, Dallin, Scalapino, Bertram D. Wolfe, and others, as well as by a group of outstanding associate area editors, such as Milorad M. Drachkovitch, Charles P. Ridley, and Witold S. Sworakowski. In addition, a broad array of authors have contributed their talents to the articles on individual countries which are clearly and logically organized and neatly categorized. From this reviewer's point of view, the best short essays were written by Pano, Waldman, Kovrig, Donaldson and Kousoulas.

This volume truly offers a panoramic global review of the strength and weaknesses of various Communist parties and movements. As far as possible, the reviewer checked the factual accuracy of the individual contributions and found them to be beyond criticism. This is not an easy task, in view of the phenomenal scope of this volume, ranging from Iceland through Malaysia, France, to the People's Republic of China. For obvious reasons, the most lively and significant chapters deal with the nations of East Central Europe and the Soviet Union itself. The long essay on the USSR is logically divided into two parts: domestic and international. Robert H. Donaldson did a particularly fine job in presenting a broad-brush and challenging picture of the "International Views and Policies" of the USSR as they unfolded in 1973–74. The lengthy final section of International Communist Front organizations was contributed by Paul F. Magnelia of the California State College at Stanislaus; while comprehensive and detailed in its scope, this piece is somewhat too factual for the average reader. Like some of the other sections of this monumental study, it reads more like a dictionary or a sales catalogue rather than an analytical perspective of this interesting and complex facet of the world Communist movement.

If the reviewer has one overall criticism, it is precisely the point just raised, namely, that this work contains too much factual summary and is somewhat short on analytical propositions and theoretical considerations. The volume's ultimate purpose and rationale are open to question: because of its overly factual coverage it will be regarded as a dictionary-like reference work rather than a vibrant and informative text which one could assign to students. However, within the framework and strictures of this probably insoluble problem, Staar and his able editorial staff did an outstanding job in offering us invaluable raw materials for further study of the various facets of the world Communist movement and the current literature of contemporary international Communist affairs. Those of us working in Communist Studies eagerly look forward to the next few volumes of this series.

ANDREW GYORGY

George Washington University

The Inequality of Nations. By Robert W. Tucker. (New York: Basic Books, 1977. Pp. 214. $10.95.)

One of the most visible, most discussed yet least understood forces in contemporary world politics is the demand of Third World countries for a greater share in the world's economic product and a greater voice in the world's political process. There is, to be sure, no shortage of books and articles on the new role of the South in world politics, but few are as rigorous and systematic as Robert Tucker's book, *The Inequality of Nations.* Tucker's stated purpose is to explain the nature and consequences of the sudden and pervasive demand for equality by the Third World. His intent is to build a case for resisting many of those demands.

Thoroughly and systematically Tucker examines the content of the "new egalitarianism," going beyond categorizing specific Third World demands to examine the view of international relations contained in the proposed new order. What Tucker finds is something new and something quite old. What is new is the demand for equal opportunity in a system in which inequality has always been an accepted characteristic. What is quite traditional is that the sovereign, independent state remains the centerpiece of the system. The equality in question, Tucker shows, is equality among states, not individuals, and its achievement involves the strengthening of the state and the traditional search by nations for power and status. What the Third World is challenging is not the structure of the system, i.e., its state base, but the distribution of power and wealth within the structure.

Tucker contrasts these traditional elements of the new egalitarianism with what he calls the "new political sensibility." The latter, he says, represents the attitudes of the "regnant [i.e., liberal] intellectual elites of the developed states of the West," including those who believe that the world is becoming more interdependent. These intellectuals, argues Tucker, have been extremely sympathetic to demands for equality without realizing that their vision of the new world is quite different from that of Third World elites. In an important, if sometimes flawed critique of the new political sensibility, Tucker shows that the view of a world in which the state is losing control and in which military power and order based on force and hierarchy are being transformed is quite different from that of the new egalitarianism which concentrates on the state and state power. Although Tucker is wrong in lumping all western intellectuals together and in many of his accusations—many interdependence theorists, for example, have pointed out the irony of the weakened state and growing interdependence among developed countries, while the strengthened state remains the vehicle of independence for southern countries and not all assume that interdependence inevitably leads to international cooperation—he is correct in pointing out the gap between western intellectuals and southern elites.

Having defined the content of the new egalitarianism, Tucker then carefully builds a case for resistance. He refutes and accurately points to the vagueness of most arguments that developed countries are vulnerable to Third World threats. With the exception of oil, he says, the North is not vulnerable to the disruption of raw material supply or, if so, only from the least poor of the less-developed world, who are not likely to take desperate and reckless actions. Unlike many others, he sees little need for LDC cooperation in joint international economic management and no threat from the South to the general peace and stability of the world. The one possibility for southern threats which Tucker acknowledges, but of which he is quite skeptical, is southern unity with the support of the oil-exporting countries. Tucker argues that there is little likelihood of long-lived southern unity, yet he has important doubts, for his policy prescription includes an effort to undermine such solidarity.

There is also no moral basis, says Tucker, for responding to Third World claims. Tucker criticizes the analogy of the evolution of international politics with the evolution of the welfare state in domestic politics, arguing that moral suasions can bear little fruit when there is no community of values outside the state. He goes further to claim that there is little basis for the moral claims of the Third World. For example, he rejects claims for reparations based on past injustices by arguing that such injustices are hard to demonstrate and that most were legal in international law at the time, although he does not examine the arguments of the literature which contends that there were such injustices.

Not only are there no compelling reasons to respond to demands for equality, there are, according to Tucker, compelling reasons to resist such demands. Acceptance of the new egalitarianism will result in a decline in power and order. The developed capitalist states will remain the holders of power but will be unable to create and guarantee order while their challengers will be unable to establish a new order to replace the old. On this point Tucker takes on those who argue that the very basis of the old order is eroding. Tucker is quite skeptical that new institutions will be developed to replace the old order based on force. Rather, he sees interdependence and demands for equality producing greater conflict and competition without assuring a new order.

Egalitarianism, he says, also leads to a disjunction between order and justice. The powerful will be increasingly unwilling to support the present system if they view it as unjust. Yet because there is no "shared humanity" beyond the nation state, no new international justice will be forthcoming.

Tucker's prescription for the developed states, therefore, is to blunt the southern challenge by what he calls an "accommodationist strategy": modest concessions, efforts to undermine southern solidarity, the one threat to the North, and preservation of northern unity under U.S. leadership. Although this prescription and the analysis on which it is based will be criticized by many, the Tucker study offers an important and provocative critique of much of contemporary analysis of Third World politics and world politics in general, a critique which should encourage greater rigor in future analysis by the "regnant intellectual elites."

JOAN E. SPERO

Columbia University

A Theory of Public Bureaucracy: Politics, Personality, and Organization in the State Department. By Donald P. Warwick, with Marvin Meade and Theodore Reed. (Cambridge, Mass.: Harvard, 1975. Pp. xii + 252. $12.00.)

In the mid-1960s the State Department adopted a number of reforms intended to improve its organizational effectiveness. One initiative, ACORD (Action for Organizational Development), focused on the problem of excessive bureaucratic layering and hierarchy. Initiated by the then Deputy Under-Secretary for Administration, William J. Crockett, this in-house reform effort drew heavily on McGregor's theory "Y." ACORD involved three activities: sensitivity training, team building, and "MOP"—Management by Objectives and Programs—a reorganization in the administrative and support sections of the department. All these activities spawned strong internal opposition, and had the further ironic consequence of more "bureaucracy," i.e., new hierarchical structure and rules governing operations. To find out why these unintended effects arose, Crockett recruited an evaluation team from Michigan's Institute of Social Research (ISR).

Warwick was project director of the ISR group that evaluated ACORD in 1966 and 1967. This book draws heavily on that research, although the authors also examine an unsuccessful proposal (the "Hays" bill) to integrate civil service employees at State into the Foreign Service. Their concern is with "why bureaucracy grows, why it stays, and why it shows strong powers of regeneration when a part is removed" (p. 4). More specifically, why did the offices and rules stripped out by MOP begin to reappear within a year, so that by the early 1970s the administrative elements of the department were as bureaucratic as before Crockett's reform? Interestingly, the quantitative data collected in the ACORD evaluation are largely ignored in favor of qualitative assessments—as the authors explain, "the numbers became increasingly irrelevant to an understanding of the engines of bureaucracy in the State Department" (p. 219).

As the title suggests, Warwick and his colleagues advance a general theory of organizational behavior in public agencies. Drawing heavily on the systems approach of earlier theorists, especially Katz and Kahn, their theory includes four major elements: (1) *environment,* which includes the organization's "power setting" and its "operating circumstances"; (2) *inputs*—information moving from the environment into the organization; (3) *organizational subsystem,* i.e., internal structures and processes; and (4) *outputs*—the services the agency provides (pp. 59–63). These core elements are then examined to determine their impact on hierarchical structure and the rules governing operations.

Although it looks beyond internal organizational characteristics to the institution's environment for the causes of bureaucracy, the theory has two limitations: The first—as the authors recognize—is its restricted and perhaps atypical empirical base. Not only is the State Department an unusual agency, but part of their data relates to the handling of administrative activities, rather than of foreign policy issues. The second limitation is that the theory is insufficiently specific in terms of those environmental or organizational factors that must be taken into account to explain bureaucratization. To suggest that "operating circumstances" of "complexity, uncertainty, threat, and dispersion" contribute to bureaucracy does not, in the end, get us very far. The net is too loose; a theory requires more precise hypothesis suggesting, for example, the ways in which uncertainty, or any of the other factors, relates to hierarchy and rules.

As a case study of organizational reform, however, the book is both interesting and useful. Predictably, the authors find many causes of bureaucracy. For example, the external environment "creates both top-down and bottom-up pressures for hierarchy and rules" (p. 83); "the bureaucrat can be an important source of bureaucracy, whether in origination or preservation" (p. 186); and "efforts by senior leaders to stimulate creativity from the top down seem to have further dampened initiative at the bottom" (p. 128). These findings are supported by evidence from a variety of sources: the McCarthy investigations, State's relations with Congress, and the essentially non-participatory approach employed by Crockett himself.

This is an excellent book, well conceived, carefully researched, and clearly written. It is one of the few analyses of an organizational reform effort in a public agency. In that connection, the candid description of the ISR evaluation effort contained in the appendix will be of special interest to academic researchers and consultants. Foreign policy specialists will find the discussion of the State Department informed and thoughtful. The theory of bureaucracy, if incomplete in important respects, will interest organizational theorists and deserves further refinement and testing.

The book is valuable to yet another audience–the civil servants and political appointees caught up in organizational reforms. For this audience, Warwick offers some acute insights into the practical problems that plague those involved. Mandated reductions in staff are almost always counterproductive, he observes, while "top-down" debureaucratization is usually of dubious value. Reorganization "pegged only to considerations of rationality is doomed to failure" (p. 205). Although pessimistic about far-reaching change in the federal government, the authors believe there is room for "a modest reduction in bureaucracy" (p. 210). They conclude with this cautious prescription: "Any successful reform program . . . will have to be positive in orientation, respect those affected in the bureaucracy, be geared to long-term rather than short-term objectives, give due recognition to the linkages between the executive system and Congress, and otherwise take account of the distinctive features of the federal system" (p. 210).

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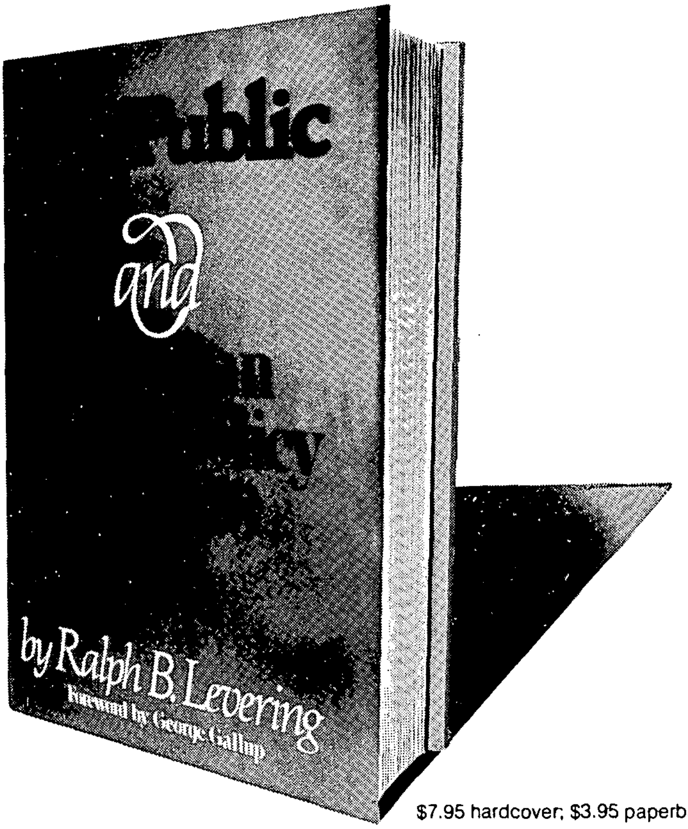

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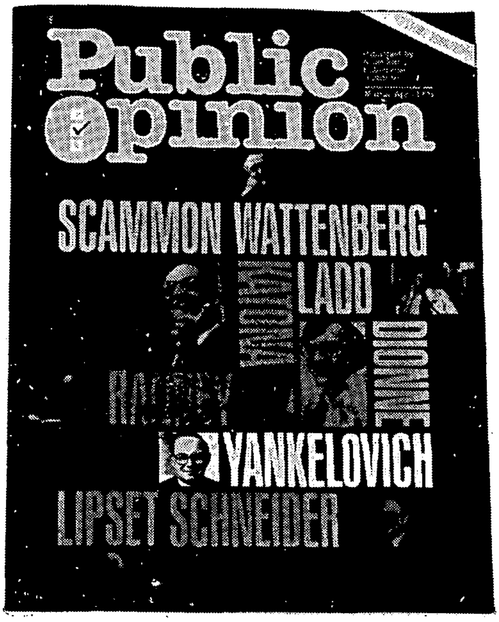

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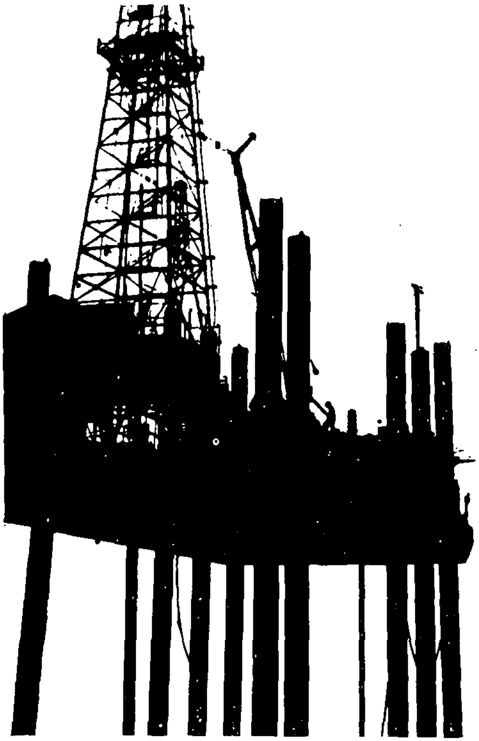

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published by
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produced by
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